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NLSRT Memo No. 39

November 28, 1988

Dear Colleague:

Earlier this year NRAO began a study of modern filled aperture radio telescopes with the thought of possibly replacing the 140-ft and 300-ft antennas. Over the past year a small group has been discussing performance specifications and design concepts, and we have talked with and have received written comments from a number of people. We have also held one internal meeting in Charlottesville to discuss these issues. As NRAO is committed to the Millimeter Array as its next major project, and as the current climate for funding has not been encouraging, this has been a low key effort.

However, in view of the recent loss of the 300-ft, it is appropriate at this time to distribute a preliminary draft of our report, in the hope that it will stimulate interest in replacing the 300-ft antenna, and that it will serve to focus the discussion leading to the final performance specifications. In reading this report, please keep in mind: a) the study was initiated primarily in response to a perceived interest in replacing the 140-ft rather than the 300-ft antenna; b) we were better able to estimate the cost of a moderate sized, high-frequency antenna than a larger, low-frequency instrument; and c) we were instructed to explore alternate sources of funding to the NSF. The 300-ft loss may impact these priorities, but this is not completely reflected in our report, which gives more attention to a high frequency instrument. Our present needs appear to call for something which is at least as big as the 300-ft and performs at least up to 22 GHz.

In preparing this report, we have not had sufficient resources available to discuss design criteria and costs in a detail comparable to that of the earlier NRAO single-dish design efforts. Instead, we have used the material developed in these earlier studies, particularly the 1969, 300-ft and the 1972, 65-m designs of Findlay and Von Hoerner, together with experience gained from the construction of the 25-m VLBA antennas, to present designs and preliminary cost estimates.

This bench-mark design is for a 70-m diameter antenna with full efficiency at 1.3 cm, good performance at wavelengths at least down to 7 mm, and usable performance under favorable conditions at 3 mm. The cost of this antenna is estimated to be $50 million (1988 dollars). This cost estimate depends critically on the amount of inflation which has occurred since 1972.

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It is not easy to find an unambiguous value for this. A more accurate estimate will come from using current prices of steel, other materials, and labor costs. It also appears possible to construct a 100-m class instrument with good performance at 1.3 cm for about the same price. A more special purpose instrument may be built which has greater collecting area and covers only the longer wavelengths, but we have only very crude estimates of the cost of such an instrument.

A site in Green Bank, WV is suggested to exploit the unique support available there and to gain maximum protection from radio interference. In view of the potential applications to space VLBI and to SETI, it is suggested that funding might be sought from NASA.

It must be emphasized that the kind of instrument we are discussing is not available off the shelf. Experience with the procurement of the VLBA antennas suggests that the expertise needed to design a modern, high-technology radio telescope may not exist within U.S. industry. The NRAO design concepts for the instruments discussed earlier in this report were undertaken by a highly skilled group consisting of Findlay, Von Hoerner, Horne, Hvatum, King, Peery, and Wong. Many man years were involved, even though none of the designs were ever even carried to the point of preparing an RFP. The loss of Findlay, Hvatum, and now Weinreb, who have been the leaders in developing the unique instrumentation at NRAO over the past 25 years, will be especially felt.

Many people have contributed material used in the preparation of this report. Due to the limited time available to complete the draft, it has not been possible to check the manuscript with everyone of them, and some errors of fact or interpretation may remain. Your comments and suggestions will be important in firming up the concepts discussed in this report.

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K. I. Kellermann

A VERY LARGE DISH (VLD) RADIO TELESCOPE

I. BACKGROUND

The Radio Astronomy Panel of the Astronomy Survey Committee has noted in its report that:

> *All of the previous studies of the needs of U.S. Astronomy have recommended the construction of a large general purpose radio telescope to work at wavelengths of roughly one centimeter and longer. In the judgement of the panel on Radio Astronomy the arguments for such a facility remain very strong, and an instrument in the 100-meter class is an important priority for the 1980's.*

No mention is made, however, of a large steerable radio telescope in the report of the full committee.

The original plans for Green Bank included the construction of a fully steerable dish in the 300-ft class. Work at NRAO over the past two decades has led to the understanding of the homology (Von Hoerner, 1967, Astron. J. 72, 35) concept. Design concepts for the Largest Feasible Steerable Telescope (LFST) were studied in the mid 1960's (Findlay, Design Studies of Radio Telescopes, NRAO Report, 1965; Findlay, et al., Progress Report on the Design of the Largest Feasible Radio Telescope, NRAO Report, 1966).

Later proposals were prepared for:

a) A 300-ft antenna. ("A 300-ft High Precision Radio Telescope," NRAO Report, 1969). This design effort was later extended by the NRL (Yeomans, "The Design of a 300-ft Research Antenna," in Structures Technology for Large Radio and Radar Telescope Systems, pp 5-12, MIT Press, 1969).

b) A 65 m antenna. (Findlay and Von Hoerner, "A 65 Meter Radio Telescope," NRAO Report, 1969).

c) A 25-m millimeter antenna. (NRAO Reports I, II, and III, 1975, 1977, and 1982).

A 440-ft radio/radar antenna was designed and proposed by CAMROC (NEROC) and is described in detailed reports issued in 1967 and 1968. A 300-ft antenna was proposed jointly by Caltech, Michigan, and Berkeley in the mid 1960's. Regrettably, none of these telescopes was ever built. With the exception of a few, often in-house, research programs which have been run at several university antennas, the great majority of the U.S. radio astronomy community, as well as scientists from other countries (including Germany and Canada which have their own large radio telescopes), has depended on the Green Bank 140-ft and 300-ft telescopes for their single dish research programs.

The 300-ft is no longer available. The 140-ft has many discoveries to its credit, but its equatorial mounting is now obsolete, its gravitation deformations excessive, and its surface and pointing are inadequate for short wavelength work. Maintenance may become difficult and expensive. It is time to build a new filled aperture, fully steerable telescope of a quality commensurate with the unique instrumentation and support available at the NRAO. In the tradition of the VLA and VLB, we shall refer to this instrument as the Very Large Dish, or VLD

II. SCIENTIFIC APPLICATIONS

There appears to be a broad consensus that a modern, fully steerable, filled aperture telescope in the 70-150 m class would be "nice to have," but there is a wide-spread perception that in order to justify any major (i.e., costly) new radio telescope it must open up significant new scientific capabilities. Realistically, it must be recognized that the Arecibo telescope will have a much greater collecting area than any fully steerable dish that might be built, but only at the longer wavelengths and over the limited part of the sky where Arecibo is effective. A modern, fully steerable radio telescope will give major improvements in versatility, sky coverage, and sensitivity over other existing instruments. Moreover, because of the excellent low-noise performance of NRAO receivers, the availability of a relatively interference free site, and the flexible responsive management of the NRAO, a new instrument may be expected to substantially out-perform the Effelsberg 100-m antenna, even though it may be of somewhat smaller physical dimensions.

With the advent of simple low noise receivers made possible by the use of FET and HEMT amplifiers, the use of focal plane arrays of a large number of receiver/feed systems can give filled aperture instruments a mapping efficiency which approaches that of a multi-element array, but of course only at the expense of frequency versatility and possibly the lowest noise performance.

A single, general-purpose, fully steerable radio telescope which can replace both the 140-ft and 300-ft needs to have adequate collecting area to provide high sensitivity at long wavelengths as well as sufficient surface accuracy to work well at short wavelengths. This means compromises. It will not be possible to even approach the collecting area of the Arecibo telescope, but a realistic goal will be to match the effective area of the largest existing fully steerable antennas in Effelsberg and Jodrell Bank at long centimeter wavelengths. At short centimeter wavelengths, we will want to exceed the performance of any existing telescope.

With the exception of the VLA, there is no instrument in the U.S. which can put an effective area greater than a thousand square meters in the direction of the galactic center. The VLD will have an effective area at least twice this value and will cover at least 85 percent of the entire sky. Arecibo can see only one-third of the sky. The effect of increased sky coverage of a fully steerable dish is particularly important for galactic research, as the part of the sky inaccessible to Arecibo includes an exceptional concentration of interesting galactic objects, including the galactic center. This is illustrated in the following table.

SKY COVERAGE (percent)

	Arecibo	300-ft	VLD
Total Sky	33	66	86
Galactic Plane	26	62	80
Pulsars	30	62	77
HII Regions	16	52	70
Globular Clusters	18	34	86

It may be noted that in order to achieve truly large collecting areas with full sky coverage and tracking capability, it ultimately will be necessary to construct an array of large dishes. A single, new VLD in the 70 to 150 m class will have important applications to nearly all areas of radio astronomy, particularly to problems requiring high sensitivity to low surface brightness radiation.

The well-known tradeoffs between size and wavelength limit will need to be addressed in terms of the potential scientific applications. These are discussed briefly below:

a) VLBI: The VLBA will give radio images of unprecedented resolution and image quality. The individual elements of the VLBA are, however, of limited size, especially in comparison to the large collecting areas available in Europe. The addition of a single, large antenna will improve the sensitivity of the VLBA to faint features by a factor of three to six.

Particularly important is the need for a large steerable antenna working to wavelengths as short as 1.3 cm for use in space VLBI. During the next decade one or more countries are expected to orbit small radio telescopes dedicated to space VLBI. This will give a dramatic increase in resolution compared with ground based arrays, but the sensitivity will be limited because of the high cost and limited capability of orbiting large antenna structures of sufficient precision. Since the sensitivity of an interferometer depends on the product of the diameters of the individual elements, it is far more cost effective to place the largest possible collecting area on the ground, which may then work together with one or more space-based elements.

Use for space VLBI may be the single most important application of the VLD.

b) Pulsars: Most pulsar observations are made with large filled aperture instruments because good sensitivity with high time resolution is needed, confusion by steady sources can be rejected, and because pulsars are so small that they cannot be resolved with even the largest interferometer systems. Pulsars have steep spectra, so they are easiest to observe at low frequencies where the results are often compromised by radio interference. For pulsar observations, a large collecting area is more important than high surface accuracy, and a site free of radio interference is more important than one with low cloud cover or low water vapor content.

Timing measurements of pulsars are needed to determine their dipole magnetic field strength and to detect new binary systems. With its extensive sky coverage, frequency range, and tracking ability, the VLD will greatly increase our capability to measure pulsar periods and their change. This is becoming an increasingly important area of research and has important applications to fundamental physics.

c) Microwave Background: Observations of fluctuations in the microwave background on a scale of the order an arc minute are critically important to the understanding of the formation of galaxies and provide unique information on the early Universe. An antenna with a large collecting area which operates at least up to frequencies of 20 GHz where the effect of the non-thermal galactic radiation is negligible is needed for this research. In practice, the quality of the observations are limited by sky noise, so a site with good sky transparency will be critical, as will the ability to rapidly change frequency and reschedule to exploit the most favorable observing conditions.

d) Extragalactic HI: Extragalactic studies directly addressable through 21-cm line studies focus on three principal areas.

i) The geometry and kinematics of the large scale structure of the universe.
ii) The global properties of galaxies, especially well-defined classes (E's, S0's, etc.), or relatively rare systems (ring, Seyfert, interacting, etc.).
iii) Properties of galaxy clusters.

Twenty-one centimeter data, together with optical information, yields such important galaxy parameters as redshift, distance (both from the Hubble flow and the Tully-Fisher relation), mass estimates, HI content, total angular momentum, cooling-flow environment, etc. Clearly all-sky coverage is needed for full sampling in two of the spatial coordinates and an interference free setting for the third spatial coordinate (distance or red shift) to properly attack these problems.

Also of great interest will be highly redshifted HI absorption lines of the type seen in front of the quasar 3C 286. This work requires protection from interference at frequencies which are outside the radio astronomy protected band and which are in heavy use for other applications.

e) Atomic and molecular spectroscopy: Spectroscopy in general, but molecular spectroscopy and astrochemistry in particular, will exploit the scientific capabilities of a new, fully steerable antenna equipped with sensitive low-noise receivers. A telescope working to 43 GHz can cover many molecular species. Unlike most other scientific areas which utilize well chosen but specific molecular lines as probes of the physical conditions in dense clouds, the study of chemistry requires maximum frequency coverage. This is because the most important transitions of different types of molecules, under differing physical conditions, occur from centimeter to millimeter wavelengths. The need for centimeter wavelength molecular spectroscopy is becoming increasingly important since observations at millimeter wavelengths from the new generation of millimeter wave radio telescopes require confirming or complementary work at centimeter waves, and we recognize that the chemistry in the interstellar medium varies markedly from one environment to another.

Radio recombination lines from highly excited neutral carbon atoms have been observed at frequencies as low as 30 MHz. These atoms have Bohr radii of a few hundredths of a millimeter, making them the largest ever studied in any environment. The lines are pressure broadened and their excitation temperature is nearly equal to the gas kinetic temperature, so they provide a new diagnostic for studying cool gas in the interstellar medium.

Atomic and molecular spectroscopy require highly sensitive instruments. Sky noise is less important than it is for continuum observations. But because the frequency of observation depends on line-transition frequencies, it cannot be chosen to be in an interference free part of the spectrum. A site protected from rfi is essential for spectroscopy. Low standing waves are essential for flat baselines needed to detect weak lines.

f) Galactic HI and HII: Even now, the sky is still not well observed in the 21-cm line, thirty-five years after its first detection. High-quality observations of galactic HI are increasingly in demand, not only because many traditional areas of study have evolved in interesting directions, such as galactic structure and the ISM, but because of the ties between good galactic HI information and work at other wavelengths. Accurate high-latitude HI column density measurements are critical in correcting the observed soft X-ray spectra of QSOs and AGN for absorption due to our own galactic ISM. Need for these data will become more pressing when new satellites like ROSAT and AXAF come into operation. There is a similar tie-in with observations expected from the EUVE satellite, and the current literature is full of studies, prompted by IRAS observations, of the correlation between the atomic and dusty components of the ISM. There are also a number of projects planned for the Hubble Space Telescope which are designed around comparison of 21-cm and UV-spectra in the same direction.

Galactic HI observations for the 1990's and beyond require fairly high angular resolution of the kind provided by a 100-m class telescope or larger, and especially high main-beam efficiency. HI emission is ubiquitous and fills any telescope sidelobes; a fully offset paraboloid or a minimally blocked aperture can avoid serious contamination of the spectra from stray 21-cm radiation.

The majority of galactic HII regions have still not been measured in a radio recombination line, and thus their velocity, and even their secure identification as HII regions, remains undetermined. They are known now only as continuum sources. Measurement of galactic HII regions through their recombination line emission will have two scientific benefits: first it will help establish the velocity field of the galaxy in the sites of star formation; second, it will uncover continuum sources which have no recombination line and are thus candidates for other identification, possibly flat-spectrum SNRs, or even pulsars. These observations are most suited for large filled-apertures of the 100-m class. Since some of the youngest nebulae are the densest, and hence the most optically thick at centimeter wavelengths, the main requirements on a telescope are that it have good angular resolution, frequency agility to work at various recombination line transitions, and a minimum wavelength of at least 1 cm to detect lines from the densest regions.

g) SETI: SETI, the Search for Extraterrestrial Intelligence, takes two forms. In one case rather modest sized antennas are used to scan the sky looking for possible sources of non-natural cosmic radio emission. In the second case a large antenna is used in a targeted search mode to examine known, usually nearby, stars. The latter mode requires steerable antennas with the largest possible sensitivity and full sky coverage to reach the concentration of potential targets near the galactic center. The single biggest challenge to SETI research is to distinguish between man-made and cosmic signals; the protection from terrestrial interference is essential to a successful SETI program.

h) Galaxies, Quasars, and Cosmology: A large steerable radio telescope can make all sky surveys containing more than 100,000 sources. These surveys are needed for a wide variety of cosmological problems, including the spatial distribution of radio galaxies and quasars, their radio luminosity function and its dependence on cosmic epoch, and the search for new, interesting objects such as gravitational lenses.

Most studies of individual radio sources are best done with the VLA which has superior resolution and sensitivity. However, some important classes of observation are best done with a filled aperture instrument. These include multi-frequency observations of variable sources, particularly sources with rapid high frequency variability known as "flicker," and the high frequency observation of low surface brightness objects such as spiral galaxies.

The continuum observation of discrete radio sources needs the greatest possible sensitivity at frequencies at least up to 20 GHz and multiple feed systems to reduce the time needed to survey large regions of the sky.

i) Stellar Sources: Many exotic galactic stars can be detected by their continuum variability on time scales of hours or days. A radio telescope capable of mapping the galactic plane rapidly at fairly high frequencies would be the most effective instrument for discovering them. Observations of spectra and time variability requiring frequency agility will be important in understanding the physics of the emission processes.

III. DESIGN SPECIFICATIONS

Two general classes of telescope have been discussed.

a) Most discussion has been about a general purpose 70 to 100-m class instrument operating with full efficiency up to 22 GHz, with good efficiency up to 43 GHz (SiO), and with limited performance up to 86 GHz. With care, such an instrument might also be useful for CO research at 115 GHz. A radio telescope in this class reflects a good tradeoff in cost, collecting area, and wavelength limit. It would effectively replace the 140-ft antenna and the loss of the 300-ft antenna and would be used for a wide variety of important research programs. It is particularly needed to support space VLBI. With a careful design of the feed support and use of a shaped primary reflecting surface, the effective area will be comparable to the 300-ft and 100-m antennas at intermediate wavelengths. In this respect, it should be noted that the 100-m Effelsberg antenna has an effective diameter of about 65 meters at 1.3 cm, and considerably less at shorter wavelengths. To minimize the cost, it is likely that the antenna will be only partly homologous. But the effects of gravity deformations can be minimized by allowing the feed to move in the vertical direction.

At L-band, the effective area of a modern 70-m filled aperture shaped antenna will be comparable to that of other large antennas, such as the Bonn 100-m and Jodrell Bank 250-ft. At shorter wavelengths it will have more collecting area than any other fully steerable radio telescope. At 1.3 cm it will be a factor of 5 more sensitive than the 140-ft. At wavelengths longer than 20 cm, where operation will need to be from the prime focus, the effective area will be less than that of the 300-ft or 100-m telescopes.

A large filled aperture radio telescope can be equipped with about ten receivers covering the main centimeter radio astronomy bands, although this would likely be at some expense in sensitivity over a single state-of-the-art receiver. It would then be possible to change frequency in just a few seconds to allow a wide variety of multi-frequency programs not now easily feasible as well as greatly reduce operating costs by eliminating the need for frequent changes of receivers. Multiple receivers, each working at the same frequency, can be used to provide multiple, simultaneous beams to speed up mapping observations. Multiple feed systems probably cannot be accommodated on an antenna with a shaped primary reflector, and some compromises between the number of feeds and the antenna efficiency will need to be made. New receivers for filled aperture radio telescopes may be readily and inexpensively fabricated to exploit new scientific opportunities in a way not feasible with multi-antenna arrays.

The size of the subreflector will need to be chosen as a compromise between the desire for high surface accuracy, which suggests a small structure and low diffraction at the longest operating wavelengths which may require an uncomfortably large subreflector. Prime focus operation at the longer wavelengths may be necessary, but this would restrict the amount of shaping and compromise the efficiency at short wavelengths. If possible, the subreflector should be of symmetric design which will be easier to fabricate.

There are considerable cost savings achieved by requiring that the structure meet the specifications necessary to operate at the shortest wavelengths only under favorable conditions of wind and temperature differentials. This is the philosophy adopted for the VLBA antenna elements.

b) A second design concept is for a much larger (100 to 150-m diameter) antenna capable of working down to wavelengths of the order of 10 cm. An unblocked-offset feed design might achieve the same effective collecting area with a diameter of only 80 to 120 meters. At the same time, the unblocked antenna would be less susceptible to interference due its low level of sidelobe radiation. However, an offset design is estimated to cost perhaps 30 percent more than a conventional design of the same dimensions, so it is not clear if the added costs of obtaining an unblocked aperture would result in a less expensive design even though the overall size is less.

It is possible that a very large steerable antenna could be built for specific low-frequency applications such as 21 cm and pulsar research rather than for general purpose applications and could be constructed at relatively low cost. Moreover, by not trying to satisfy conflicting design requirements, the telescope may be optimized for specific applications. For example, a single dish covering a wide frequency range introduces severe, and often conflicting, requirements on surface accuracy, size, pointing accuracy, slew rate, subreflector size and accuracy, site selection, polarization purity, receiver changing speed, observing techniques, etc. Trying to satisfy these conflicting requirements can be very costly and seldom ends in an optimum solution for any.

It may be argued that it is more appropriate for a national facility to provide a general purpose instrument rather than to attack more narrowly defined research problems. However, it is likely that a thoughtfully conceived "special purpose" radio telescope will have applications which extend far beyond the original design goals, as has been the case with the 300-ft and Arecibo antennas. Even a general purpose instrument can do only one thing at a time, probably not in an optimum fashion. Several specialized instruments may do more science for about the same cost of construction, although operating costs would be higher. Moreover, more time would be available for innovative programs of a more experimental or contemplative nature on a special purpose instrument than has been the case with the highly competitive instruments such as the VLA. It is perhaps relevant to note that the very valuable 300-ft all-sky 20 and 6 cm surveys could have been done more easily on one of several general purpose instruments; but they were not, because these other instruments were busy doing general purpose things.

The design and construction of a low cost, special purpose antenna would follow and extend the philosophy of the 300-ft in the following ways:

a) Use standard steel members of cheap cross section.
b) Use simple joints with welding only in the shop and using bolts in the field.
c) Keep all mechanical parts as simple as possible.
d) Place no accuracy specifications on the steel structure, but accept what standard fabrication techniques give.

Among the design concepts which have been discussed and need to be considered for the VLD, but which are beyond the scope of this report, are:

a) Off axis designs and unblocked apertures to reduce spill-over and interference, and to increase efficiency.
b) Very low sidelobe levels to reduce interference.
c) Spherical primary allows all surface panels to be identical and reduces cost.
d) Three reflector surfaces to improve efficiency.
e) Cost savings from limited sky coverage.
f) Focal plane arrays for multi-beaming.
g) Active surface to improve efficiency.
h) Carbon fiber reinforced plastic vs steel.
i) Extent of homologous deformations.
j) Active pointing.
k) Active temperature control to reduce thermal effects.

A more detailed discussion of some of these concepts has been given by Von Hoerner (Proceedings of 300-ft Workshop, 1988).

IV. LOCATION

The choice of a suitable site will depend on the usual criteria of atmospheric clarity, influence of wind and temperature on the structure, the potential for radio frequency interference, and latitude, as well as the general administrative criteria which affect operations, such as sources of manpower and the availability of utilities and services.

In order to minimize operating costs, it is felt that the VLD should be placed at an existing NRAO site. Either Green Bank or the VLA would provide a site which is known to be suitable for operating a radio telescope and which can provide the needed technical and administrative support. We briefly summarize the advantages and disadvantages of these two locations.

a) Green Bank: The erection of a new VLD in Green Bank would effectively replace both the present 140-ft and former 300-ft antennas. Operating costs would be less than it was for these antennas, partly because we would need to operate only one antenna and partly because an antenna of modern design would probably be less expensive to operate than the ageing 140-ft. Location in Green Bank would allow support by the extensive administrative facilities which are already in place, as well as by an unmatched technical staff with years of experience in support of single dish radio telescopes.

A Green Bank site would exploit the unique asset of the National Radio Quiet Zone (NRQZ) to give protection from the ever increasing levels of rfi. There is no site in the U.S. which affords both the physical and legal protection available in the NRQZ, and the abandonment of this site would be an irreparable loss to the future of radio astronomy.

The percentage of clear days in the Green Bank area is among the lowest anywhere in the United States. Experience in operating the 140-ft at short wavelengths indicates that adverse atmospheric conditions will limit continuum observations for a significant fraction of the time. This will be of particular concern with the 70-m VLD which is optimized for short wavelengths. On the other hand, under favorable winter weather conditions, the atmosphere in Green Bank is known to be excellent for work even at short millimeter wavelengths, and frequency flexibility, combined with careful scheduling, should be adequate to exploit the capabilities of the instrument. The absence of direct sunlight will minimize pointing uncertainties, and may result in higher quality data for those programs not requiring the best sky transparency.

For VLBI, a large antenna on the East Coast will complement the large collecting area available with the VLA, as well as the large telescopes in Bonn, Nobeyama, and in the USSR. This will be particularly important for space VLBI where the space antenna will necessarily be of limited diameter.

The operation of a major new scientific instrument in Green Bank would probably require a reversal of the trend of recent years, of decreasing the size of the Green Bank scientific and technical staffs.

b) VLA site: For much of the time the relatively high altitude of the VLA and the clear skies is attractive for the operation of the VLD at short wavelengths. The high winds frequently observed at the VLA may restrict the fraction of time when the best pointing is available. However, these winds appear to occur with predictable regularity, so that it might be possible to minimize their impact by appropriate scheduling. A location near the VLA would facilitate the use of the VLD together with the VLA for increased sensitivity and to provide coverage of low spatial frequencies. The VLA/VLBA operation is increasingly becoming the centroid of NRAO operations. If located near the VLA, the VLD will be more accessible to the increasing New Mexico scientific and technical support staffs.

V. COST

Sufficient resources were not available for this study to determine the cost with sufficient accuracy to prepare a formal proposal. We have made cost estimates in several ways.

a) We have obtained through the MPIfR an informal budgetary estimate of 80 million DM ($50 million) from Krupp/MAN of the cost of reproducing the 100-m Effelsberg antenna in Europe. It must be emphasized that this applies only to an erection in Europe, and only for an exact reproduction.

Any significant modification to the design would probably necessitate a complete reanalysis and re-tooling which would result in a disproportionate increase the cost. Specifically, one cannot scale this estimate to get the cost of a 70-m telescope.

b) A careful cost estimate was done in 1972 for the NRAO 65-m design (Findlay and Von Hoerner, NRAO Internal Report). At that time Findlay and Von Hoerner estimated a cost of $10,056,000 for the antenna structure and associated site development. This number was used to estimate the 1988 cost of a 70-m class instrument in the following way.

i) The cost of a dormitory was deleted

ii) An estimate was obtained from Radiation Systems, Inc., for VLBA-type surface panels. These panels have a measured rms accuracy typically in the range 0.042 to 0.046 inches, or about 100 microns.

Tooling	$1,330,000
Panels	$2,940,000
Total	$4,270,000

iii) Escalation at 8 percent per year for 16 years increases the cost by a factor of 3.5.

iv) Scale from 65 m to 70 m using a 2.6 power law increases the cost by a factor of 1.2.

The final cost, estimated in this way is $37 million. Allowance for 20 percent contingency leads to a best cost estimate of $45 million (1988 dollars). We consider this to be the most accurate costing possible at this time. A somewhat more accurate figure may be obtained by using current prices of steel and other materials together with current labor costs in lieu of that obtained from the general escalation.

c) Assuming that the VLD has been designed to a comparable detail as provided in the VLBA RFP, the cost of a 70-m antenna scaled up from the VLBA design has been obtained using a 2.6 exponential dependence on diameter. The cost factors for engineering, design, servo systems, erection, etc., varied from 1.5 to 7.8. The cost estimated in this way is:

i)	Engineering/Design	$ 1,820k
ii)	Construction	13,380k
iii)	Erection	2,160k
iv)	Panels	4,270k
v)	Subreflector	100k
vi)	Foundations and Track	500k
vii)	Site Development	800k
viii)	Installation, Cabling, etc	350k
ix)	Service Tower	400k
x)	Focus and Rotation Mount	250k
	Total	$24,030k
	Contingency	4,810k
	Total	$28,840k

The performance of such a scaled up VLBA design is unknown, but most likely would not meet our specifications for surface accuracy or pointing.

d) An alternative cost estimate of a 70-m antenna with the same performance specifications as a 25-m VLBA antenna may be obtained by scaling the cost of the VLBA prototype antenna (Phase II of the RSI contract) which cost $2.123k (1986). Escalating this to 1988 by 6 percent, scaling for size by a 2.6 exponential law, gives a cost of $32.7M. As in the previous estimate, we must add the cost of engineering and design as well as the non-RSI items (foundation, secondary reflector, cabling, service tower, site development, etc., and contingency). This gives $44 million (1988 dollars), in good agreement with the estimate obtained from the 65-m design.

e) The cost of a 100-m class telescope may be estimated from the 1969 NRAO design study. At that time a 330-ft telescope was estimated to cost $10 million with a 10 percent allowance for contingency. We have increased the contingency to 20 percent, substituted the RSI surface panels, and allowed for escalation to arrive at a 1988 price fo $50.2M for a 330-ft telescope with good ($\lambda/16$) performance at 1.3 cm.

f) The cost of a low-cost, more specialized instrument discussed above may be estimated as follows:

At the time of construction in 1961, it was estimated that it would cost an additional one million dollars to make the 300-ft into a fully steerable antenna. This would raise the initial cost to $1.8M. Assuming a 26 year inflation of 5.2 makes the 1988 cost about $9.4M. In 1966 the back-up structure was strengthened and new feed legs added at a cost of $200k. In 1970 a new surface was added at a cost of $500k. Allowing for inflation brings these improvements to 3.8 million in current dollars, making the total 1988 cost $13.2 million. Allowance for contingency brings this to $15.8M.

If built in Green Bank, a 70-m high frequency VLD might replace both the 140-ft and 300-ft telescopes, but for some important observations it would be less sensitive than the old 300-ft. Although larger in size, the operating costs of the new VLD would probably not exceed those of the 140-ft and would certainly be less than the 140-ft and 300-ft combined. Thus, NRAO would be able to make available to the astronomical community a major new radio telescope at no increase in operating costs. Moreover, the experienced staff and available support facilities would ensure the optimum use of the instrument from an early phase, without an extensive start-up period. Alternatively, a 100-m class, low-frequency radio telescope would directly replace the loss of the 300-ft, but would require the continued operation of the 140-ft.

VI. FUNDING

It has become the practice in the U.S. to expend great effort and sums of money on the design of new astronomical instruments in order to obtain funding. Often, even after great effort devoted to extensive peer review, even the most highly rated projects often go unfunded. This has been wasteful of the limited resources available to astronomy, and the general lack of success has been demoralizing. This procedure may be contrasted to that used in Europe, where sufficient funds for a complete engineering design may be made available on the basis of relatively modest feasibility study, with construction funds being contingent on the demonstration of a satisfactory design.

We feel this latter approach is the only practical one for NRAO under the present circumstances. We have not had sufficient in-house resources to develop a proposal in the same detail as was done for the VLA, VLBA, or the earlier single dish projects. At the best we have a budget estimate which may be used as the basis to obtain funding for an engineering design. There is no question, of course, that given $50 million we can design and build an excellent instrument that will serve the needs of radio astronomy for years to come. The exact size and specifications of the telescope will need to await the engineering design; but we propose that the funds be committed, contingent to a satisfactory design, prior to the start of an extensive design program.

For a project of this magnitude, we can identify only two viable funding agencies. The National Science Foundation is the obvious choice, but the poor success in recent years by the NSF in funding of astronomical research is not encouraging. Moreover, the choice among a VLD and other competing projects in radio astronomy, such as a millimeter array or the Arecibo upgrade, are likely to involve lengthy discussion among many committees and endless delays.

One of the primary applications of the VLD is likely to be in support of space VLBI. A large steerable dish such as the VLD is essential to the success of any space VLBI mission, and even the construction of a new ground element for this purpose is very cost effective relative to overall mission costs. This is particularly true since a single ground element may support more than one space element, giving the U.S. an opportunity to contribute in an important way to the space missions of several foreign space agencies at a fraction of the cost of a single space element.

Another major application of the VLD may be in the NASA SETI program. Rather than spend large sums of money to adapt existing foreign instruments for this work, as is currently planned, it might be more appropriate for NASA to contribute to a new instrument in the U.S.

In view of the above, it is thought that NASA may be an appropriate agency to support the construction of a VLD.

VII. FUTURE WORK

We estimate that about nine man months of engineering and design plus three man months of drafting are needed to obtain an engineering design for a 70 to 100-m VLD suitable for an RFP of the same quality as the VLBA design done at NRAO. The NRAO cost estimate obtained during the preparation of the RFP was very close to the original RSI contract price.

There are a number of design concepts which will need to be studied in further detail to determine their effectiveness and cost impact.

i) Work is needed by a feed engineer to study the illumination efficiency and polarization properties of a fully unblocked system. We need to know if the polarization properties at prime focus will be adequate, or if a Cassegrain configuration is needed, and how to illuminate the dish at low frequencies?

ii) Engineering support is needed to determine more accurately the cost/wavelength tradeoff for various designs and the reality of cost savings introduced by the use of novel design concepts. We need to find out if the added cost of an unblocked aperture design is worth the improved efficiency, or if the same or improved performance is obtained by simply increasing the diameter for the same cost.

An alternate approach for NRAO is to prepare a limited number of important specifications (e.g., size, shape, surface accuracy, elevation range, slew speed) and to locate manufacturers who are willing and able to propose designs on all or part of the structure using techniques they are equipped to provide. If we are willing to compromise on specifications that save money and to concentrate on specifications that enhance the performance at modest cost, it may be possible to construct a rather large aperture at relatively low cost. Areas which have been identified for potential cost savings are:

a) Choose a site with low wind speed.
b) Restrict slew velocity and accelerations.

c) Restrict elevation to say 10 or 15 degrees above the horizon. Restrict azimuth range to 360 degrees. Give up some sky coverage near the zenith.
d) Keep the surface simple and parabolic. With a minimum radius of curvature of about 90 meters found in the center of a 100-m dish, 2-m panels can be flat and meet a $\lambda/16$ criteria at 5 GHz. In practice, one could do much better than this by using larger panels which are curved in the radial direction only, with flat panels being used only near the edge of the dish. With flat, or slightly curved panels, the argument relating to the high cost of panels for an asymmetric antenna is much weaker.
e) Avoid complex joints, non-standard materials, and hard to fabricate pieces.
f) Use prime focus only and restrict receiver and cabling weight. Below 5 GHz, excessively large subreflectors are required. Fortunately, receivers are getting simpler and lighter with FET's and HEMT's. In the future low noise performance may be achieved with little or no refrigeration. Multibeam receivers will need to be reduced in weight.
g) Use a simple and easy to program control computer such as the PC-AT.

VIII. ACKNOWLEDGEMENTS

The following people have contributed material used in the preparation of this report: R. Brown, J. Condon, J. R. Fisher, J. Findlay, F. J. Lockman, L. King, P. Napier, G. Peery, M. Roberts, G. Seielstad, S. Von Hoerner, B. Burke (MIT), and J. Baars (MPIfR).

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Nov 28, 1988