NLSRT Memo No. 14

MASSACHUSETTS INSTITUTE OF TECHNOLOGY
DEPARTMENT OF PHYSICS
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MEMORANDUM

TO: K. Kellerman

FROM: B. Burke BFB

DATE: 29 November 1988

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

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

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

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

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

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

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

The "optimistic" estimate generated in this way may be low, but it recognizes that the pointing and rigidity requirements 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.