NLSRT Memo No. 3

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To: Ken Kellermann
From: Rick Fisher
Subject: New Single Dishes

I gather that you have agreed to chair the process of looking into ideas for a new single dish. Here are a few comments that you might want to think about.

Funding considerations aside, the whole process of planning and building new instruments is too long and probably too expensive. I think that this is mainly due to the apparent need to reach a broad consensus on each new instrument. Hence, we tend to expand the specifications to the point where huge committees meet to write design and justification documents, funding committees agonize over the decision, many people are involved in the final design and construction, and the construction time is very long - a significant fraction of a 40-year career. I would like to argue generally for building more specialized instruments quickly and moderately inexpensively and make a specific proposal as an example.

Scale: Let's say that the average instrument costs $10M and takes 4 years to build. This is $60M in 24 years which is about one third of the 1987 cost of building the VLA and VLBA in the same time. Hence, I'm not necessarily proposing that we spend the majority of our money on this class instrument.

Compromises: A specialized instrument would be less hampered by conflicting design requirements. This has advantages in both cost and performance. For example, a single dish covering a wide frequency range puts severe requirements on surface accuracy, size, pointing accuracy, slew rate, subreflector size, site selection, polarization purity, receiver changing speed, observing techniques, etc. all at the same time. Conflicting requirements can be very costly and seldom end in an optimum solution for any.

Proposals: Competition is on the basis of conceptual design and scientific merit in a report of 25 pages or less. Risk can be minimized by funding in two steps. If the conceptual design looks anywhere near feasible, something like 3% of the cost could be put up for proof of design with the promise of funding if proof is supplied. One might even carry two designs through this competition stage, but preferably not. Keeping the original proposals at the conceptual stage would reduce wasted time on unfunded proposals and increase the number of new ideas likely to be proposed. I think that the current system of requiring detailed design before a decision is made does not add much to the decision process and tends to be a committee's way of putting off a hard decision.

Operating costs: Smaller instruments are less likely to require a site other than ones that have already been developed, and they should be reasonably simple to operate compared to large arrays. New instrumentation need not be terribly general purpose and, hence, should be reasonably priced and quickly developed.

Who operates it?: This sized instrument could be a national facility, part of a university, or some combination of the two sharing site, operations, and instrumentation development.

Time for science: A general purpose instrument can do only one thing at a time. This is OK if it can do everything much faster, but some forms of research do not lend themselves to such an approach. Specialized projects would not compete so severely for telescope time if they were split among several instruments. Projects of this sort could tend to be of a more contemplative and experimental nature than those typically proposed on the largest instruments.

A Specific Example

Let's suppose that we were to build the largest fully steerable antenna possible for $10M to work to 5 GHz. This would be of considerable interest to the fields of pulsars, galactic and extragalactic HI and OH, recombination lines, and possibly some continuum and molecular line work. Jay Lockman can make a better scientific case than I.

A symmetric design would be in the 100 to 150-meter range or an unblocked, offset design would probably be in the 80 to 120-meter range. This is based on the argument that the 300-ft could have been made fully steerable in 1962 with another million dollars raising its 1962 cost to $1.8M. An inflation figure of 2.5 raises this to about $4.5M in 1987. A new surface was added in 1970 for about $0.5M to which we can apply inflation of 2.0 for a total cost of $5.5M. I asked Buck Peery why the 300-ft cost so little, and his opinion is that it was built with standard construction techniques for which manufacturer's assembly lines are typically set up using parts that were cheap and commonly available and that the performance specifications were kept modest.

As I understand it, Sebastian has argued that the cost of a single dish depends very little on the surface accuracy, and that a 100-meter dish should cost at least $50M. The simple calculation above convinces me that there must be something wrong with his argument. Maybe he is not considering the approach and cost savings that I'll outline below. Also, I have heard it mentioned that the 300-ft is not torsionally stable so azimuthal motion would be unwise. Buck's opinion is that this would not be a big problem as long as we didn't ask for large accelerations. We know that it is stiff enough for asymmetric wind forces. A new design could include some torsional stiffness if necessary.

I talked at some length to Buck about how one would go about contracting a large, inexpensive antenna. We know that our conventional methods will produce a high price tag. Buck's advice is to write down a very small number of specs (size, shape, surface accuracy, elevation range, and slew speeds) and beat the bushes for manufacturers who are willing to propose designs on all or part of the structure using techniques they are equipped to provide. We don't want to saddle them with a preconceived design, and we don't want to be the prime contractor ourselves because of the responsibility of meeting the combined specs. We might do the motor and control systems separately. We listen very carefully to the manufacturers for compromises on specifications that could save money and specifications that could be enhanced at modest cost.

Where would we save money over previous antennas? Pick a site with low maximum winds, otherwise, the site selection is done on the basis of cost. The 300-ft has seen winds no higher than about 60 or 70 knots and loses only a day or two a year to winds above 25 knots. Most antennas are specified to survive 120-knot winds and retain pointing accuracy up to 20 knots or more. Restrict slew accelerations and maximum rates. Beam switching is not a big issue below 5 GHz so don't try to trim the motion overhead too much. Restrict elevation and azimuth motions. Let's say zenith to 10 or 15 degrees elevation (galactic center elevation = 23 degrees at G.B.) in one direction only and 360 degrees in azimuth splitting at 50 degrees east of north. We might even give up some coverage close to the zenith since the azimuth rate would not be fast enough to track through this zone.

Keep the surface simple and paraboloidal. With a minimum radius of curvature of about 90 meters found in the center of a 100-meter dish, 2-meter panels can be flat and meet the 1/16 lambda criterion at 5 GHz. In fact, we would do much better than this by using larger panels curved in the radial direction only with flat panels near the edge of the dish. No compound curves. One of the arguments against an asymmetric antenna is the panel fabrication cost. With flat or singly curved panels I think this argument is much weaker. We avoid complex joints, non-standard materials, and hard to fabricate pieces.

Use prime focus only and restrict receiver and cabling weight. Below 5 GHz diffraction is too big a problem for cassegrain. Receivers are getting simpler and lighter with FET's and HEMT's and someday we may even see low receiver temperatures with no or only modest refrigeration. Multibeam receivers will have to work on getting the weight down, but we already see ways to do this. Signal digitization can probably be moved to the front end and the output and control signals transmitted on optical fibers. If a symmetric reflector were most cost effective I would try hard to keep feed support blockage to a minimum and be very careful not to introduce reflections that degrade spectral line performance. We can do much better than we've done on the 140-ft, 300-ft or the 100-meter in this respect. Focal plane arrays will probably come into use in the next ten years which should do for prime focus systems some of what shaped surfaces do for cassegrain systems. This is a gamble on future technology, but I think it is justified given the fact that the instrument is very useful without these developments.

The control computer would be a carbon copy of the 300-ft system with a PC-AT class machine in place of the H316 for about $150K. Back end electronics I shall leave out of the cost.

Given all of these arguments you can probably guess that I don't want to be a member of any study committee. I'll throw in my opinion on specific questions, and I promised Jay that I'd help him put together a brief proposal along the lines of the example above. Otherwise, good luck.