25-METER MILLIMETER WAVELENGTH TELESCOPE

MEMORANDUM NO. 135

Notes on a Work Session Held at Tucson February 28-29, 1980

Present: J. W. Findlay, M. A. Gordon, W. G. Horne, J. M. Payne

1. Plan of Discussion

Appendix 1 shows the outline of the subjects to be discussed, and makes clear that the object of the session was to study various problems and plans connected with the telescope surface. In addition, some thought was given to other critical areas--such as the pointing precision of the designed instrument.

2. Setting the Surface

(a) The engineering method. There was a general agreement that the first step in surface setting would be taken by using a range and angle method. Range measures would be by tape, but possibly also by using a laser ranging instrument (LRI). The final setting would need an LRI and so it might be wise to have it for both methods. Pentaprisms were preferred for angle measurements.

Some specific points were made:

(i) WGH - We may have to order pentaprisms fairly early. The production time may be years. Are they obtainable from any source other than Zeiss?

(ii) JWF - It may very well be that the details of the pentaprism mount at the dish center can be common to both the engineering and the final setting methods. These details will impact (probably not causing trouble) both the electronics design of the receiver room at the vertex and the actual telescope design at or near the vertex.

(iii) JMP - JMP has responsibility for all 25-meter electronics and has already started to review the vertex cabin design in some detail. Changes may be made--he will watch out for the requirements of the measuring system with WGH and JWF. The general plan would be to use, if possible, the vertex cabin for carrying the surface measuring systems and then to carry electronics. However, it must be remembered that telescope performance (particularly aperture efficiency) is a very useful parameter and so electronics and surface measuring may, to some extent, have to be planned to live together. This can be done when the details of the final system are worked out.

(b) The final method. The outline plan of this method had been considered carefully by all the group. Basically it consists of two elements. The first is the method by which a light beam is made to remain parallel (to about 0.2 arc seconds) to local gravity. This beam is then turned through 90° (or an angle very close to 90°--the angle must be known and stable to 0.2 arc seconds). This beam is used continually to monitor the up-down position of the end target of the telescope radius being measured. The same beam can be modulated to be an LRI. Thus the light beam essentially sets the (x,z) axes of the measuring reference. The (x,y) axes lie in the plane passing through the dish vertex; the z axis is parallel to gravity pointed upward. Distances along the beam must be measured with the LRI to ± 20 or ± 30 microns.

The second element of the system is to use the stepping method from the dish vertex out to the (monitored) end target of the radius being checked.

Before discussing the numerous details of this setting system, it seemed to those present that it was a sound method to attempt. JWF had modified his original plan to instrument all radii to a plan where the light beam would remain fixed and, one by one, radii to be measured would be set on the beam by rotating the telescope in azimuth. The same end-target would be used for all radii. This much reduces the cost and also ensures that the properties of the beam and end-target are identical for all radii. The points where further work or thought is needed are numerous. Some are listed below.

WGH - You want to level the radius being stepped. There are two radii where you can't do this by tilting the telescope. However, we are going to have to make a very level azimuth track and so we can find a telescope azimuth position where these radii lie within our level tolerances. Other radii are set by bringing them to this position and using the elevation drive to set the level.

(?) - When a radius is being stepped the telescope level may change. So it will, but it is monitored continually and automatically and the corrections are fed to the computer to update continually the desired stepping-bar tilt angle.

JMP - How stable is the "level" reading of the inclinometer? So far the inclinometer (-15° to 0° to +15°) has been mounted in a tilted position to cover 0° to 30°. This was not good, since we cannot test to answer this question. We will use an inclinometer which reads -30° to 0° to +30°; mount it level on the bar and at regular intervals check the bar on a level mount. These checks all need to be done to about 1 arc second. Everything in this system is possible--but it is all close to reasonable limits.

WGH - When stepping the bar, have you got the fine points of the geometry correct? Probably not--particularly when the steel spheres sit in holes in the surface. But it can easily be got right.

WGH/MAG - How do you get round the feed legs? It must be done by a kind of circumferential stepping to interpolate (or other ad hoc methods). Remember, the areas where setting is difficult are also those where the legs shadow the surface.

General Points - We have a mercury pool at the VLA. Can we devise any experiments to determine how precise the gravity referenced beam will be? Can the mercury pool be carried in the vertex room? (Vibrations will ripple to surface.) The best astrolabes work to better than 0.2 arc seconds; is this a reliable check on what we can do? Are there sources of advice on the system? MAG suggested Peter A. Franken at the optical sciences center at the University of Arizona.

Long Delay Items - If we adopt the proposed system we need an LRI. Payne's original work showed the principle. Herrero built one for Arecibo but (for various reasons) it has never been made to work. At the MPI a good "breadboard" model has been built. But to design and build a well-engineered instrument is quite outside the present work force available to Payne. Even with money to let a contract, JMP may find it hard to get an instrument built.

To autocollimate the light beam and make it truly parallel to gravity might need a 4-element servo, two elements translating the beam laterally (in x,y directions) and two more deflecting the beam through small angles (θ,ϕ). This could be quite a device to build--JMP can offer no manpower to do this.

(c) Summary of the Surface Setting Systems.

(i) We seem to agree so far that we can set the surface, but there are many difficulties and unknowns, particularly in the final method, to be explored.

(ii) We are seriously manpower limited. JWF can work on some problems--but things like the LRI would take 9-12 man-months of a JMP-like engineer.

(iii) We are in no position to solve our manpower problems with money, for we have no money.

(iv) Even if there were money, it is by no means clear that we could get what we need by spending it in industry. This situation must, of course, be altered before we enter the design stage. It is now well-recognized by those in the Tucson work-session. It is a management question to alter it.

(v) No decision could be reached on whether a radio-holographic method should be considered as a long-term way of checking telescope performance. The Green Bank 140-ft results may help us here. At any rate, nothing more can be done at present with available resources.

3. The Aeroneutronic-Ford Plates

(a) The various measurements made on the A-F plates had been summarized in Memo No. 129 of December 10, 1979. Nothing more can be learned, except that the "bent" A-F plate (25-meter No. 1) has now been bent back by S. Smith. He is preparing a report on this work--but basically a force of about 1140 pounds weight was applied to give the plate a permanent set of about 360 microns--somewhat less than the original bend. However, it seems that the experiment has confirmed that the plate must have been subjected to a considerable force (> 1/2 ton weight) to give it the shape it acquired.

(b) The "heavy" 25-meter A-F plates. WGH had noticed this problem from a Lee King report. Photographs and details of both the 65-meter and 25-meter plates were supplied by W-Y. Wong. It is clear that the 25-meter plates were not manufactured correctly. The main question seemed to be:

If 25-meter plates were correctly designed, cast and machined would they meet our performance needs?

The answer seemed to be yes. But it would probably be desirable sometime to make some correct plates to be sure on this point.

(c) Further work on A-F plates. If a satisfactory plate-measuring machine can be built at Green Bank there are several things to be done. First, JWF described briefly the plans for a step-by-step development of a machine. (W-Y. Wong has done most of the first design and work will soon start in the GB shops.) With such a machine we could recheck thermal and mechanical behavior of an A-F plate. Then, using the "bent" plate No. 1, various methods of surface modification would be tested and evaluated. The machine would be used for measuring and testing other types of surface plate. If its development is successful it would be a prototype of an NRAO machine to be used in quality assurance of the delivered telescope surface plates.

4. The Harris Contract

The first results of the recent measurements of the second set of three Harris plates and of the mold were available and were studied. For a more careful set of conclusions, Lee King's report, when it is submitted, will be studied.

The following comments are therefore only preliminary:

(a) The mold was far from the perfect shape, but this was to be expected since we had agreed to accept an out-of-shape mold.

(b) No plate replicated (agreed with) the mold.

(c) The three plates did not replicate each other.

(d) But two plates did replicate each other to about 30 microns; (RMS).

These are not particularly happy results. However, the fairly close agreement between just two plates is somewhat tantalizing. It was this kind of behavior that we were hoping for in all plates. We had expected possible mold-to-plate differences but hoped the plates themselves would replicate.

5. The Future

This final paragraph of the notes on our working session is being written after the fact that the telescope was removed from the NSF budget had become known. Nothing of what has been said earlier is affected by this change--but the future is affected. Also, comments in this paragraph are from JWF alone--the change in the budget came after our Tucson session.

I shall assume that we shall not drop the 25-meter from our plans but that we shall continue to work on it as best we can. I also assume that there is no hope for first funding (money in our hands) before January 1, 1982. I shall also assume there may well be a constriction both of money and manpower within NRAO in the year starting January 1, 1981. So, in this climate, what does one suggest for the future? I will separate my remarks into four headings:

(a) Organization. The working session convinced us all (I think) of the need for a good well-organized engineering group for the 25-meter. With little manpower, members of this group may be able to spend only a (small) part of their time on the project. But they should be named, assigned tasks and given a leader--a chief engineer. He in turn should try to plan the work for the next year. I, for one, need this kind of chief engineer and group if I am to make further progress in the surface problems--both in measurement and in ways of manufacturing. I do not need a committee, but I need first-class advice and help both in engineering and electronics. If manpower is short, so be it, but let us try to pick out the important tasks and work on them.

(b) The 25-meter within NRAO.

(i) Surface setting. I propose to explore for help and advice on my proposed system. I should, with persistence, get this for nothing. At present, we will build nothing more to explore the system's potential and problems. I am not happy with this, but we will have to rely on the MPI group for LRI development and set aside the autocollimator problems

(ii) Plate measuring. I propose we continue our work on developing a machine. I may have to interface with one of the 9825A's at Green Bank and economize. But such a machine will let us do work in NRAO at little cost on improving the surface. To put it simply, I want to "do a Leighton" in this area in a different way but use our skills to try to get a better surface.

(c) Work outside NRAO.

(i) A new type of surface plate. In the work session we considered what still might be done to look for a new surface plate. Two suggestions were either to try the Milliflect Corporation of Sunnyvale, California who made subreflectors for Tucson and the VLA or to explore an old Philco-Ford technique (WGH). We discussed sending the Harris-made mold to Milliflect and seeing whether they would make three plates on it, at as low a cost as possible. We would not ask them to do engineering, just the lay-up, curing and so on. We considered this because the VLA and Tucson subreflectors did seem to replicate well.

The WGH memory was that Philco-Ford did once make a plate on a mold by a simple method. It might be worth tracking this down.

However, in both these suggestions, the nonavailability of manpower and money may be the determining fact.

(ii) Improved techniques. We might try to explore (at low cost) whether NC machining may have improved over the years, or whether machining Leighton-type panel might give better results. Again, manpower and money will limit effort.

(d) A decision point? A review? Are we at a point in this project where we should review it--chiefly from an engineering point of view? I believe the other members of our working session might join me in saying:

(i) We believe we can build the 25-meter to meet the original specifications and performance requirements. But, as the area of surface making and setting shows, this belief is based partly on

some engineering information and partly on a trust that the solutions we propose for difficult problems will in fact work as well as we predict.

(ii) We think it possible that some potential users of the telescope may be too hopeful that the original performance will in fact be quite significantly improved when the telescope is built. This may or may not be true. It is too early to say.

It might be a good thing to set up in 2 or 3 months a one or two day review of the whole project--to be attended by our staff, scientists, engineers, computers, and so on. We could present the telescope, point out the decisions already made and emphasize the areas where we still see uncertainties.

The purpose of this would be to ensure that our own scientists see the project in a real way. It might also raise a few questions and/or suggestions on the way the work is being done.

John W. Findlay

Charlottesville
March 31, 1980

APPENDIX I

The Surface of the 25-Meter Telescope

Work Session with Bill Horne, John Payne, and Mark Gordon

Tucson, February 29, 1980

1. Review of Setting the Surface

   (a) The engineering method. (tape and pentaprism) Equipment and plans possibly common to this and the final setting method.

   (b) The final setting. (by stepping with a stable optical reference system) Questions as asked in JWF's January 30 memo.

2. The A-F Plates

   (a) Review of measurements on these plates.

   (b) The "bent" plate. As I suggested, this has now been bent back by Sidney Smith. I have a note of results.

   (c) The 25-meter plate weight problem. (Our two plates are seriously overweight.)

   (d) Further work needed.

3. The Harris Work

   (a) Lee King has given me the first results. I have not yet studied them--we will do so at Tucson.

   (b) Does this lead us anywhere?

4. Other Work

   Far-out ideas, if any. I have one or two.

John W. Findlay

Charlottesville
February 27, 1980