Phil

WORK AT THE 12-METER TELESCOPE JULY 17 - 23, 1987

by John W. Findlay

1. Look at Holography.

A total of six holographic measurements had been made before I arrived. These were recorded with the dish at elevation angles of about 21 degrees and 53 degrees. Each gave an array of 32 x 32 data points and thus could be a very useful set of data. The raw data with no corrections applied had been transformed into phase and amplitude arrays by Dr. D. T. Emerson, and a quick look that he had applied to the data suggested that the systematic divergence which we suspected to exist between holographic (H) and mechanical (M) measures might eventually emerge.

On July 18th and 19th, I chose radii 21, 57, 92 and 128 to attempt to make a comparison between the August 25 1986 M-map and these holographic measures. I chose these radii since they lie along lines of grid points in the H-arrays and it was thus easy to read the M-map at these points. The scatter in the H-data was very large, but I nevertheless averaged all six data sets along each of the chosen radii and then plotted the differences point by point along each radius. These plots are in the "Work Book" which is returned with this report. It is too early to be sure, but there seems to be evidence that the M-measures are giving systematically low readings for the surface elevation around the outer zone (R = 3 to 6 meters) and systematically high readings around the inner zone. I believe this result is in qualitative agreement with the quick look of DTE referred to above. It certainly makes it very important to process the H-data with all the corrections applied.

When this is done, a point-by-point comparison should be made between the H- and M-data. I return to this in paragraph 4.

2. Surface Measurements on July 20, 1987.

The measuring system was checked out by Paul Rhodes and Dennis Chase on July 17th and on the morning of July 20th. There were the usual difficulties with centering and securing the center-ball but the measuring plan was carried out starting at 0932 and ending at 1208 (Local Time). The ambient temperature in the dome was high; going from 26°C to 30°C as the measurements were made. During the measurements, the raw data recorded on each radius was displayed on a hard-copy print and was compared at once with the corresponding print from the August 25, 1986 data. The differences in position of the center-ball prevented a close comparison of the data sets but there was some evidence during the data taking that the readings from sensor #3 were appearing on the display as too low. (The data displayed, although "raw", has already had the sensor zero-point readings subtracted from it.) Checks were made to show that Sensor #3 was not sticking and appeared to be measuring correctly. However, when the data set was read into the first stage of the reduction process, the dish RMS was found to be 91 microns, considerably higher than the 1986 value of 80 microns.

3. The results of the measurement.

When the departures from the best-fit surface were plotted along each radius, it became clear that there was an apparent low point at Sensor #3 (R = 1485 mm). Several tests were made, including checking the sensor calibration and the possibility of an error having arisen in the programs. However, it was eventually concluded that the zero-point setting of Sensor #3 had changed by about 118 millivolts (about 150 microns) since the measuring system was last used in 1985. There is evidence for this in the values obtained when the template was mated with the reference jig on July 17, 1987 as compared with similar results in 1986. Also, when the sensors were being cleaned and checked early on July 17th it was noted that the tip of sensor #3 was slightly unscrewed and it was then tightened. For this to be the cause of the change since 1986, however, it has to be assumed that the 1986 measures were made with the sensor tip slightly unscrewed. The history of the mating results is considered in more detail in a short note placed in the "Work Book".

A new reduction of the data was made in which the VV value in NEWRADC for Sensor #3 was changed from 2204 mV to 2086 mV and the output from this reduction, as well as the similar output for the August 25, 1986 data, is given in Table 1 below.

Table 1. A comparison of the 1986 and 1987 measurements.

Parameter	August 25 1986	July 20 1987
Ambient Temperature	16.5 degrees C	28 degrees C
RMS	80.1 microns	85.4 microns
Focal length	5079.8 mm	5079.8 mm
Vertex X- coord	-1.711 mm	-1.377 mm
Vertex Y- coord	-0.614 mm	-0.906 mm
Vertex Z- coord	-0.115 mm	-0.021 mm
X-axis rotation	-25 arc seconds	-31 arc seconds
Y-axis rotation	31 arc seconds	22 arc seconds

There is little need to comment on this table, except to note that the vertex positions are determined by the setting of the center-ball in the dish and it is clear that a good job was done in reproducing the 1986 values in 1987. I would not be prepared to say that the change in RMS is real in view of the difficulties in the Sensor #3 zero-point value in 1987.

In comparing the data sets from 1985 to 1986 (see paragraph 3 of my report dated August 28, 1986) I made a point-by-point comparison for the 1408 points common to the two surveys and arrived at a RMS value for the differences between the two data sets of 27.8 microns. I have made a similar comparison between the 1986 and 1987 data sets (which have 1441 common points) and get an RMS of 26.2 microns.

4. Conclusions.

Despite the slight uncertainty introduced by the Sensor #3 problem I believe that these measurements show an unchanged telescope over the last year, insofar as its surface shape is involved. The close agreement of the M-data in the two years suggests that we could be ready for a good H-M comparison. This should be made on a point-by-point basis and, because we believe we are looking for systematic errors in the M-measures, it would be best if the points at which sensors touch the surface were chosen as the comparison points. The co-ordinates of these points is in the RMS program but I have also put a note of the geometry in the work book. I take it that the interpolation from the H-array to these points is not difficult (I think it is difficult but I believe programs exist to which it is easy). Once this is done, the comparison can be made either at Tucson by Betty Stobie, or by me using a 9825A borrowed from Green Bank.

Let me repeat my philosophy. Unless we find evidence to the contrary (for example that the six H-runs show serious differences), we are now using H as the absolute shape finder and M as the repair or adjust mechanism. The study to find systematic errors in M is of a secondary importance, although I wish to pursue it for my own satisfaction.

One last point on the H-measures. It is probably not necessary nor desirable to alter the data sets to allow for the elevation angles at which the data was taken. But if this were needed, remember that Lee King computed gravity deflexions at zenith angles of 30 and 40 degrees (see 12-M Report 204 of November 30, 1982).

Greenwood.
August 4, 1987

John W. Findlay