AK

Effelsberg
Upgrade Memo

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Present State: Problems, Results, Plans and Suggestions

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This is a review of the performance before the exchange of the outer panels, and a discussion of future plans. It summarizes some previous memos and new results, our talks at my visit last October, previous Green Bank investigations, and some antenna tolerance theory. A short summary of the main results and suggestions is given at the end.

I. The Primary Mirror

1. Surface Type

The inner rings, Nr.1 to 13, are solid surface. Ring 14 is perforated, replacement by solid panels is applied for. Rings 15 to 17 were open wire-mesh, and replacement by 672 perforated panels is under way, for about 2.5 Mio DM; 231 panels were mounted last October. Problems with these outer panels are the increased wind force. Also the partial transparency for short wavelengths causes pickup of ground radiation (at $\lambda = 7$ mm this was 30 K with wire-mesh, and may be 15 K perforated). It also causes gradual loss of signal, below a cut-off wavelength λ_0.

Table 1. The outer panels.

		OLD				NEW			
Ring Nr	Diam. (m)	Holes (mm)	(%)	f_0 (Ghz)	λ_0 (mm)	Holes (mm)	(%)	f_0 (Ghz)	λ_0 (mm)
14	80-85	6		29	10	0	0	-	-
15	85-90	6	60	25	12	7	33	25	12
16	90-95	6	55	19	16	7	36	25	12
17	95-100	8	61	25	12	7	36	25	12

The data of Table 1 are mainly from the Memos of Mattes [Reference 1] and Thamm [Ref 2].- Since it seemed a bit awkward to replace mesh by perforation, with similar hole sizes, it had been discussed previously to change the order for panels of Rings 15 to 17 maybe into solid plates, with advise against it by Reich [Ref 3]. We also discussed briefly whether one could close the perforation holes now by pasting foil over, but thin foil would be caved in by snow loads, and thick foil would worsen the surface accuracy. In any case, the increased wind force would speak against it.- In conclusion: We have to live with the performance given by the three outer NEW panels of Table 1. But it would be very important to obtain good solid panels for Ring 14, to have a proper surface for short wavelengths at least up to a diameter of 85 meter. A new closed Ring 14 will increase the solid surface by 13% for future millimeter-wave observations.

2. Illumination

The data of Table 2 are from Altenhoff [Ref 4]. The illumination of the prime mirror was changed in 1990. Before 1990, the illumination diameter varied from 100 m to 60 m, according to the observational wavelength. But from early 1990 on, all receiver feeds were now designed for 100 m illumination, for both prime and secondary focus, and for all wave-lengths, long or short. We must keep this in mind when we compare data before and after 1990.

Table 2. Prime focus data.
At ≈ 35° Elevation, corrected for extinction.
The aperture efficiency corresponds to 100 m diameter.

	λ (mm)	HPBW (")	Apert.Eff. (%)	Epoch
before 1990:	110	270	47	1970
	62	159	47	1970
	28	79.5	45	1970
	20	60	36	1970
	12.4	39	24.0	Sept.83
	7.0	24.6	18.8	Nov. 87
after 1990:	10	29.9	31.7	Oct. 95
	9	31.5	-	Aug. 92
	7	20.7	-	Feb. 95
	3.5	13.2	4.2	-
	3.5	11.0	-	Nov. 97

We ask for the *effective (illumination-) diameter*, D_e, which will decrease with wavelength λ, because of the decreasing illumination before 1990, and because of the increasing transparency of the outer rings with decreasing λ for all epochs. The halfpower-beamwidth generally is

$$\mathrm{HPBW} = 1.2\ \lambda / D \tag{1}$$

thus, in usual units

$$D_e(\mathrm{m}) = 248\ \lambda(\mathrm{mm})\ /\ \mathrm{HPBW}('') . \tag{2}$$

For short λ, the surface errors, $\sigma = \mathrm{rms}(\Delta z)$, become more important. The HPBW as a function of σ/λ has been investigated by Zarghamee [Ref 5, Fig.4]. If σ is about constant along the aperture (or its illuminated part), which we may assume at 35° Elevation, then the HPBW does not depend on σ/λ. We thus assume that the illumination diameter may be obtained from (2), wit no regard to σ. Table 3 shows the result.

Table 3. The effective telescope diameter, from equation (2).

before 1990		after 1990	
λ (mm)	D_e (m)	λ (mm)	D_e (m)
110	101.0	10	82.9
62	96.7	9	70.9
28	87.3	7	83.9
20	82.7	3.5	65.8
12.4	78.9	3.5	78.9
7	70.6		

The older diameters show a steady decline, while the newer D_e have much (unexplained) scatter. But comparing the last two lines "before" with the first three lines "after", we may conclude that the new increase of the illumination angle has indeed somewhat improved the effective telescope diameter for $\lambda \approx 10$ mm, even with the wire-mesh. The same follows from the efficiencies of Table 2. But the pickup of ground radiation is a disadvantage, for low-noise receivers.- For the new panels Mattes and Reich suggest using feeds of different illuminations, to find the best one. I would add: please observe system temperature and HPBW too.

3. Surface rms error and corrections

The aperture efficiency, $\eta(\lambda)$, is mostly written with the simple Ruze equation [Ref 6, Fig.6]:

$$\eta = \eta_0 \, \mathrm{EXP} \, \{-(4\pi \, \sigma/\lambda)^2\} \tag{3}$$

where η_0 refers to long wavelengths. The surface error, σ, could then be obtained as

$$\sigma = (\lambda/4\pi) \sqrt{\ln(\eta_0/\eta)} \tag{4}$$

from the measured efficiencies η at short wavelengths, **if** we had always the same illumination, the same telescope diameter, and a rather flat dish. Which is not the case for our data.

1. **Normalization.** I suggest to use the effective diameters D_e from Table 3, and to normalize the original efficiencies, η_{orig} of Table 2 (which referred to D = 100m) with equation (5). This was done for Table 4.

$$\eta(D_e) = \eta_{orig} \, (100\mathrm{m}/D_e)^2. \tag{5}$$

Table 4. Efficiencies normalized with equation (5).
(Elevation 35°, extinction corrected.)

	λ(mm)	η(%) orig.	η(%) norm.
before	110	47	46.0
	62	47	50.3
	28	45	59.0
	20	36	52.7
	12.4	24	38.6
	7	18.8	37.8
after	10	31.7	46.1
	3.5	4.2	9.7

2. **Surface slope.** Ruze [Ref 6, Fig.7) plots a correction factor, A, "due to reflector curvature". It thus is a function of the focal ratio, and it is plotted for two cases: whether the surface errors are defined *normal* to the surface (panel measurement, Vertex analysis [Ref 7]), or defined in *axial* direction (theodolite or holography measurement). From Ruze's Figure we read, for usual telescopes with F/D ≈ 0.43, and for the deeper Effelsberg dish with F/D = 0.30:

F/D		A		A
F/D = 0.43:	normal	A = 0.88,	axial	A = 0.76,
F/D = 0.30:		**A = 0.78,**		A = 0.60. (6)

We use the *normal* definition, A = 0.78, and instead of equation (3) we have now

$$\eta = \eta_0 \, \mathrm{EXP} \, \{-A \, (4\pi \, \sigma/\lambda)^2\}. \tag{7}$$

This curvature correction should be used, especially for our deep dish. And when sky observations are compared to holography, the taper of the former must be applied to the latter.

3. **Correlation.** Ruze treats also the case where the surface errors are spatially correlated over circular fields of radius c. From his equation (10) we rewrite our equation (3), with slightly different notation, as

$$\eta = [\eta_0 + (2c/D)^2 \, S(\delta)] \, \mathrm{EXP}(-\delta) \tag{8}$$

with

$$\delta = A \, (4\pi \, \sigma/\lambda)^2 \quad \text{and} \quad S(\delta) = \sum \delta^n / (n! \, n) \quad \text{for } n = 1 \ldots \infty. \tag{9}$$

I am sure that correlation is negligible in a wide surrounding of the adjustment at 35° elevation, but it does appear far off, as shown by the Vertex Analysis [Ref 7] for El = 0°, when adjusted at 30° (the third of the nine coloured figures between pages 24 and 25). Estimating $2c/D \approx 1/3$, I calculated equations (8) and (9) and found: this correction may be neglected for $\lambda \geq 15$ mm and it becomes unreasonably large for $\lambda \leq 6$ mm because of a very steep increase of $S(\delta)$.- I will not use the correlation correction at all (which most others havn't used either).

4. Results. As a good way to use (and to check) equation (7), I plot in Fig.1 the normalized $\ln(\eta)$ from Table 4, which then must be a linear function of $1/\lambda^2$:

$$\text{with } X = (10/\lambda)^2 \text{ and } Y = \ln(\eta) \quad \text{we expect } Y = a - b\,X \tag{10}$$

the arbitrary factor 10 with X serves for a better scale. The best-fit regression line is shown.

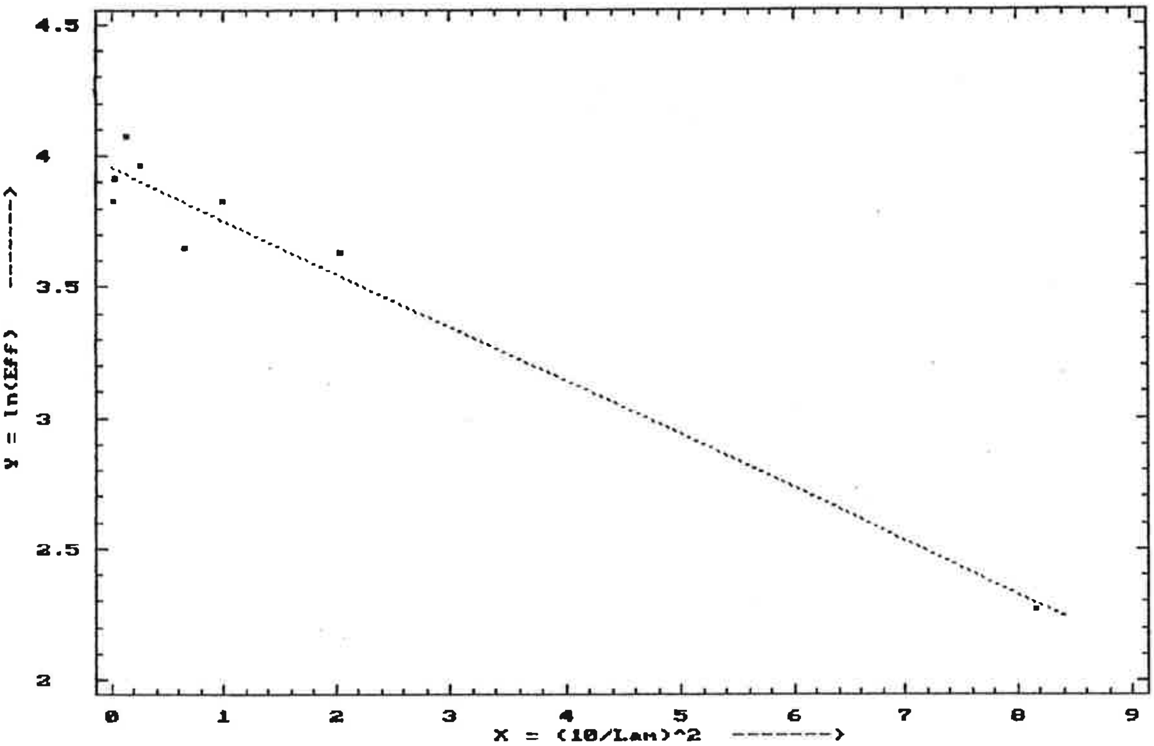

Fig.1. Efficiency versus wavelength: plot of $Y = \ln(\eta)$ over $X = (10/\lambda)^2$. Elevation 35°, normalized.

From the linear regression we then obtain $a \pm \Delta a$ and $b \pm \Delta b$, and finally the two wanted values and their mean errors as

$$\eta_0 = \mathrm{EXP}(a) \quad \text{with } \Delta\eta = \eta_0\,\Delta a, \tag{11}$$

and

$$\sigma = (10/4\pi)\sqrt{(b/A)} \quad \text{with } \Delta\sigma = (\sigma/2b)\,\Delta b. \tag{12}$$

The results are

$$\text{long-wave efficiency} \quad \eta_0 = 52.45\ \% \pm 2.58\ \%, \tag{13}$$

$$\text{surface rms error (at El 35°)} \quad \sigma = 0.407\text{ mm} \pm 0.016\text{ mm}. \tag{14}$$

I think that (5) is a good way to compare observations done under different conditions, which then are plotted like Fig.1, but with more data between 7 mm and 3.5 mm wavelength.

5. Elevation 80°. We ask for the increase of σ with gravitational deformation at extreme Elevation, using five figures of Altenhoff [Ref 4] at the primary mirror. They are in different units (flux, efficiency, gain, temperature) which does not matter since we need only their ratio (35°/80°), and we need no normalization. But the atmospheric extinction depends on elevation, and two of the five figures have no such correction. As a rough remedy, we read the average extinction correction from the other three figures, and apply this to the two where it was missing. This gives the ratios of Table 5, called η_{35}/η_{80}. From equation (7) and A = 0.78 we then have

$$\sigma_{gr} = 1.132\ (\lambda/4\pi)\ \sqrt{\ln(\eta_{35}/\eta_{80})}. \qquad (15)$$

Table 5. Efficiency ratio and gravitational deformation at 80° elevation.

[Ref 4] Fig.	λ mm	year	η_{35}/η_{80}	σ_{gr} mm
1a	7	1987	1.913	0.508
1b	12.2	1983	1.216	0.486
3a	10	1992	1.342	0.489
4a	7	1995	1.881	0.501
5a	10	1995	1.331	0.482

$$\text{average } \sigma_{gr} = 0.493 \pm 0.005 \text{ mm} \qquad (16)$$

and together

$$\sigma_{80} = \sqrt{(\sigma_{35}^2 + \sigma_{gr}^2)} = 0.639 \text{ mm}. \qquad (17)$$

There is a problem. From the structural analysis of Vertex [Ref 7], the graph on page 23+3 (only few pages have numbers!), I read

$$\sigma_{gr}(\text{Vertex}) = 0.274 \text{ mm} \qquad (18)$$

for moving from 30° adjustment to 80° elevation. Both (16) and (18) hold for the same conditions: normal to curved surface, tapered illumination. But they disagree by a factor 1.80, to be taken seriously for all short-wave observations. As a cause I can only suggest defocussing, probably lateral (perpendicular to optical and elevation axes). It gives a row of coma lobes, and from their spacings I could determine the off-axis location of the subreflector at the 140-ft telescope [Ref 8], suggesting a computer controlled lateral shift. I also found that small gravitational deformations can cause a "gliding rotation" of the best-fit paraboloid, with a rather large lateral focus shift ΔY [Ref 8, Fig.5 and Table 1]. If defocussing is our cause, I use another paper of Ruze [Ref 9, Equ.(10) and Fig.4] and obtain, for the increase from (18) to (16):

$$\text{lateral offset} \quad \Delta Y \approx 10.5 \text{ mm}. \qquad (19)$$

If it were an axial offset, this had to be $\Delta F = 34.6$ mm. But I think this is less probable.

Altenhoff showed me that the HPBW <u>decreases</u> at high elevation. He said if the deformations are larger at the center than at the rim, this would approach a ring-type telescope which then has a smaller beam (but higher sidelobes).- The first five colored Vertex figures have indeed the most drastic deformations at the center. It also agrees with Case I, Fig.4 of Zarghamee [Ref 5].

6. Adjustment. The Vertex graph shows clearly that the future adjustment angle ϑ, after the holography, should be larger than our present one. From Vertex, page 23+1, first table first line, the non-adjusted deformations are about equal at EL 0° (0.334 mm) and 90° (0.313 mm). For equal performance at both extremes, $\vartheta = 45°$ is best. For a more useful equal performance at EL 20° and 80°, then [Ref 10, Fig.1] $\vartheta = \underline{50°}$ is best. See also [Ref 4] Figs. 1a+b, 3a, 4a.

II. Wind Force

1. The VERTEX Analysis

In order to improve observations at millimeter wavelengths, the institute wanted, if possible, to replace the three outer rings of wire-mesh by solid (or perforated) panels. This increases the usable aperture. But also the wind loads, regarding structural stability and surface deformations. *VERTEX Antennentechnik* was asked for a structural analysis, delivered in March 1994 [Ref 7], with discussion by Reich in 1997 [Ref 3]. The Vertex book shows a detailed study with modern means, treating the whole telescope (elevation and azimuth parts) as one system, with 12 876 joints, 24 392 members, and 77 202 degrees of freedom. Wind is perpendicular to the elevation axis, for eight elevations from 0° to 90°. From the five load cases and various treatments we shall use the following, with adjustment at EL 30° for Case 1:

Case 1: dead loads (weight), no wind,
Case 2: wind 10 m/s with wire-mesh panels, no weight,
Case 5: wind 10 m/s with all solid panels, no weight.

Vertex gives a large amount of detailed and useful information about the rms surface error (deviation of deformed surface from best-fit paraboloid), in tables and colored figures. It also gives the forces at several main points, and the force for elevation drive (*EL-Antriebslasten*). But there is very few information about stress (*Spannung*) and stability. What I miss in an analysis of this size, that are statements whether all 24 392 members are below maximum allowed stress, or which ones don't and when and by how much. With old wire-mesh, and with perforated or solid panels. It is also difficult to quote, since 2/3 of all pages have no numbers. And difficult to understand, when in all load cases the best-fit homology parameters are given in units of <LE> if linear, and in <RAD> if angular which cannot be radians since 675.44 <RAD> would not make sense. If there is a definition of these units I could not find it. I also miss the amount of focal offset, meaning the distance between the shifted prime focus and the deformed focal cabin location (this is not FOK' and L_0 at page 18 + 2, since the deformed focus may be far off the cabin axis).- I wish Vertex could be asked to do another run of a few main cases, to supply the missing items. And in addition, at the extreme elevations, I would ask: which fraction of the surface rms is astigmatic? This fraction could be removed by a deformable subreflector.

2. Stability

A stress analysis is given only for seven types of apex cabin members, on pages 26 to 36, without wind. Force is given in N (Newton) or KN (Kilo-Newton, where 1 KN = 102.0 kg weight). Maximum allowed stress is in N/mm^2, but actual stress in KN/cm^2. And with the USA unit psi (pound per square inch): 1 KN/cm^2 = 10 N/mm^2 = 1450 psi. Five of the seven members have a stress high above the allowed one, up to the break stress.- But E. Fürst told me that already in 1994 work was done and money spent on these problems, so this seems to be alright now.

The effect of the wind on the structure I can only estimate regarding the elevation structure as a whole, since the forces at the elevation bearings are given for all load cases. The largest external load will be survival wind (where I assume 45 m/s = 100 miles/hour as the fastest storm to be survived) in stow position, EL 90°. The dead load on one bearing, Case 1, is given as Fz = 8667 KN (page 43 + 4). For 10 m/s wind it is Fy = 80 KN in Case 2, 140 KN in Case 5. The force goes with velocity square, we thus multiply Fy by $(45/10)^2 = 20.25$. Since Fy and Fz are perpendicular, we add both quadratically, $Fsv = \sqrt{(Fy^2 + Fz^2)}$ for Cases 2+1 and 5+1:

Case 1:		Fz = 8667 KN,	dead load (weight),	(20)
Case 2+1:	Fy = 1620 KN	Fsv = 8817 KN,	weight, 45 m/s wind, wire mesh,	(21)
Case 5+1:	Fy = 2835 KN	Fsv = 9119 KN,	weight, 45 m/s wind, solid panels.	(22)

The step from wire mesh (which had been stable so far) up to solid panels is only 2.8% for Fsv, and may be about half of this for the new perforated panels. Structural stability seems alright in general. But problems could occur for the direct backup members of the outer panels. This can be investigated with little paste-on stress gages, in face-on wind.

3. Drive Motors

According to Reich [Ref 3] the main problem is not the structure, but the limited force of the elevation drives: to move the telescope, or to hold it, against even medium strong winds. The wire mesh entries of Table 6 are from the present operating manual, set up 1992. The other two for new panels are derived by Reich from the Vertex data, and lost times are his rough estimates.

Table 6. Stop operation, go to stow.

	wire mesh	perforated	full panels	
Strong gusts	> 18	16.1	13.2	m/s
Constant wind	> 15	13.4	11.0	m/s
Ice, snow on surf.	> 12	10.7	8.8	m/s
Time lost	≈ 2%	3%	6%	

Since the limited elevation drives are felt as a hindrance even now, Reich suggested the perforated panels. And full panels only if the drives were strengthened, for about 4 Mio DM.

I add an excerpt about wind limits for the 100 m Green Bank Telescope [Ref 11]: "**Auto stow due to wind** - by Monitor and Control, the antenna will be moved from any position regardless of status to wind stow. Current scenario for auto wind stow is, when wind has gusted to 40 miles per hour 3 times in less than 5 minutes, or anytime such velocity persists for more than 20 seconds, the antenna will be automatically moved to wind stow in both axes." (40 mph = 18 m/s)

4. Wind and Surface rms.

Vertex data for face-on wind give only moderate deformations, as shown in Table 7, even for solid panels. Wind from the rear has less force, and side wind is represented by EL 90°.

Table 7. Surface rms from wind of 10 m/s (Case 1 is adjusted EL 30°).

EL	Case 1	2	5	2+1	5+1	
7°	0.128	0.068	0.073	0.145	0.147	mm
15°	0.085	0.066	0.059	0.108	0.103	
45°	0.083	0.088	0.160	0.121	0.180	
60°	0.165	0.089	0.161	0.187	0.231	
75°	0.246	0.064	0.117	0.254	0.272	
90°	0.324	0.081	0.146	0.334	0.355	

But I must add a warning. The worst result of strong wind, for short-wave observation, is usually not the surface error, but the wind induced pointing error. For which we have no data. Another point to keep in mind: The telescope up to now had at EL 35° an rms = 0.407 mm, see (14). But the new panels are specified for ≤ 0.50 mm, and from a sample of 8 panels [Ref 12], 7 were OK at T = 15°C; but only 2 at 5°C, one had 0.60 mm.- What about cold winter?

III. The Subreflector

1. Present State

The Memo of Altenhoff [Ref 4] has only five entries of efficiency for the secondary focus, too small a number, and three of the five are noted as uncertain. But still I shall give it the same treatment for comparison. They are all from 1997. If someone could collect a larger number, also from past years, it would be interesting to repeat the treatment. Table 8 has from Altenhoff: λ, HPBW, and the original η referring to the full 100 m aperture. From this and Equ. (2) I find the effective (illumination) diameter D_e, and with (5) the normalized efficiency η_{norm} from which we could find the combined surface error with (7). Without discussion of all oddities of Table 8 we just proceed to the linear regression shown in Fig. 2, again at EL 35°.

Table 8. Original and derived values at the secondary focus.

λ (mm)	HPBW (")	η_{orig} (%)	D_e (m)	η_{norm} (%)
110.0	259.0	55.0	105.3	49.6
20.0	51.0	43.0	97.3	45.5
13.0	36.7	28.0	87.8	36.3
9.0	25.3	25.9	88.2	33.3
7.0	19.5	13.0	89.0	16.4

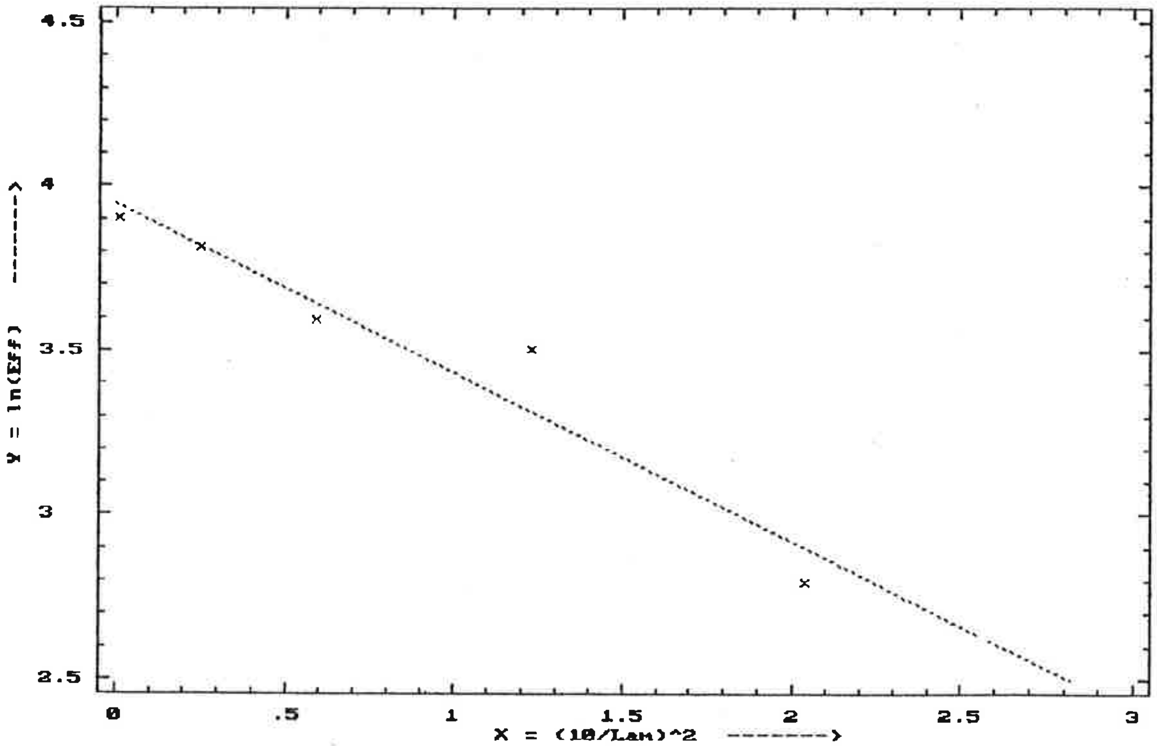

Fig. 2. Efficiency versus wavelength at secondary focus. Few and uncertain data.

From (11) and (12):

$$\text{long-wave efficiency} \quad \eta_0 = 51.8\ \% \pm 4.6\ \%, \tag{23}$$

$$\text{combined rms error} \quad \sigma = 0.646\ \text{mm} \pm 0.050\ \text{mm}. \tag{24}$$

To complete the method, but without much trust in the data, we subtract quadratically the 0.407 mm of (14) and get for the subreflector

$$\sigma_{SR} = 0.502 \pm 0.066\ \text{mm}. \tag{25}$$

2. Future Plans and Wishes

First some data, before discussing changes. The present Gregorian subreflector was 1985 installed. It has 6.5 m diameter, its weight is about 3 ton, and 5 ton is just its mount. The SR consists of 16 about pie-shaped panels which have 6 adjustments: 4 at the corners, 2 at the middle of the long sides. The Gregorian is an ellipsoid of rotation, with half-axes of $a = 14.30$ and $b = 7.39$ m. The distance between the two foci then is $2e = 2\sqrt{(a^2 - b^2)} = 24.48$ m, and the distance between secondary focus and mirror center is $e + a = 26.54$ m. For improving the efficiency, firma Eikontech used holography 1989 at primary and secondary focus. From the difference one gets the surface errors of the subreflector. Thus a new adjustment was done, but something went wrong. The SR surface was not improved, is probably a bit worse than before.

A photogrammetric measurement of the SR is now in preparation, by firma Rollei, and one of our own staff should be taught the method, such that for repeated uses we just could borrow the camera. But their special expert has left the firm, and replacement may take time. Unfortunately, any further decisions about changes may have to wait for the result of this readjustment; the hope is to get a surface rms of 0.25 mm.

Should we order a new subreflector? This was discussed several times. If all wishes could be fulfilled, it should have accurate surface (considerably better than the main reflector), should be able to wobble (at least by 4 arcmin with 1 or 2 Hz), it should be deformable if the primary mirror has enough astigmatism, and it should be movable by its mounting in all 6 degrees of freedom (3 translations, 3 rotations).- However, it should not cost too much.

Accuracy. The shortest wavelength used so far is $\lambda = 3.5$ mm, at 85 Ghz, which mostly is regarded as the future limit, too. But there was a note in 1996 [Ref 13] that the development should go up to 100 Ghz, which apparently has been dropped. The present 85 GHz limit leads already to stringent conditions for the SR.- To express this in numbers needs a rough estimate with some assumptions. Skipping all details, I expect with the new panels an effective diameter $D_e \approx 93$ m, with 0.430 mm rms average, for $\lambda \leq 6.0$ mm. And $\eta_0 = 52\%$ for the whole 100 m. Table 9 shows the result, for several rms values σ_{SR} of the subreflector. While $\sigma_{SR} = 0$ represents the primary focus.- Table 9 assumes an ambient temperature $T \geq 10°C$, and no sunshine [Ref 12].

Table 9. Expected future efficiency, with regard to D = 100 m.

	$\lambda = 3.5$ mm	$\lambda = 3.0$ mm
σ_{SR}(mm)	η (%)	η (%)
0	6.71	3.37
0.15	5.35	2.48
0.20	4.49	1.95
0.25	3.58	1.43
0.30	2.76	0.98
0.35	1.96	0.63

Wobble. The present SR is too massive (much tilt-inertia). A new SR would best be of carbon fiber, thinner and much lighter. And thermally stable. A smaller SR would also have less inertia. But from Mattes and Zinz I learned that the theory of good feed horns gives a limit: The beam of the horn cannot be made more narrow than 12° or 13°, and at present it actually is already 14.8°.- The present mounting is also too massive and not built for a fast wobble.

Mounting. A new SR would call for a new mounting as well. I think the best would be a hexapod: three points on the backup structure are connected to three on the SR (rotated 60°) by six telescopic rods, of computer-controlled length. This gives all six degrees of freedom in a most direct way. It probably will also be the SR support with the smallest inertia.

Orientation. The full freedom is also needed for optimizing location and direction of the SR (old or new), as a function of elevation. If ideal, the upper SR focus will coincide wit the prime focus, the lower SR focus with the phase center of the secondary feed horn. Even if this would have been met at the adjustment elevation of the primary, it certainly will be off at both ends by gravity at other elevations. Especially, the distance between prime focus and feed horn will be different, whereas the distance between the two SR foci cannot be changed. Accuracy is most important at the prime focus, and more permissive (by the large magnification factor) at the secondary focus. Thus, if all freedom is available, the upper SR focus should always be moved to the prime focus, and the lower SR focus should point in the direction of the feed horn.

Deformable. The astigmatism of the primary, as a function of elevation, can best be measured with a pyramidal rectangular flat feed horn, as I did at the 140-ft [Ref 14]. For $\lambda = 28$ mm the horn was 120 mm long, its aperture was 19 x 70 mm. At each elevation, receiver and horn were stepwise rotated 360° and the differences ΔF of focal length noted, and amplitude and angle of max ΔF were derived. The amplitude A_{as} of the rim astigmatism then is found from the peak-to-peak ΔF (from horizon to zenith, see Figs 2 and 3 of Ref 14), with

$$\text{ptp } \Delta F = 32\, (F/D)^2\, A_{as}. \qquad (26)$$

A deformable SR was built [Ref 15] which more than doubled the efficiency for $\lambda = 13$ mm. It is wobbling, too, with up to 3 Hz. From the Vertex pictures I expect also some astigmatism at Effelsberg, but definitely smaller. I would recommend to do a similar measurement, in order to know whether, and by how much, a deformable SR would increase the efficiency .- The next task is to get cost estimates for new subreflectors of various types, and for a new mount.

IV. Pointing Errors

How much **Accuracy** do we need? For short-wave observations with the new panels, I expected in the previous section an effective diameter of $D_e = 93$ m. With $\lambda = 3.5$ mm as the future limit, I expect from (1) and (2)

$$\text{HPBW} = 9.3 \text{ arcsec.} \qquad (27)$$

Usually, the demand for the pointing error $\Delta\varphi$ is HPBW/10, or marginally HPBW/5. As it seems, we will find it difficult to meet even this marginal limit. And if we succeed, this may hold only during calm nights:

$$\text{Marginal demand } \Delta\varphi \le 2.0 \text{ arcsec.} \qquad (28)$$

There is a Memo from Altenhoff [Ref 16] about AZ encoder errors, and EL errors from AZ collimation, with many data. But I feel not confident to deal with it, since I am not familiar with the details of the Effelsberg pointing system.- At my Bonn visit in October, I had some talks with several MPI members about **Hysteresis**, in both AZ and EL, which I will discuss briefly.

First, let me quote from an old Memo: "It is not always realized that a completely healthy structure cannot have any hysteresis at all. Hysteresis can only be produced by friction (gears, bearings), slack (gears, loose bolts), or oild-canning (a joint with only coplanar members). Hysteresis, large enough to be measured, should never be tolerated."

As to the present situation I was told: being on a source, and moving a small distance and back, the beam may be off by 2 arcsec, but up to 10 arcsec after a wider distance. Moving in AZ by 360° at the encoder, the beam is usually off by 10 arcsec. And moving large distances in EL, beam and encoders may differ by 8 arcsec, and, most disturbing, both EL encoders do give different readings. Another problem may be stiction at very slow EL movements when tracking over the meridian and at high declination, but stiction can be avoided by a small "dither" of the elevation drive.

One would like to have something *absolute* to measure against. For example, the ground. Regarding the 360° problem in AZ, it was suggested by E. Fürst to have a fixed theodolite on the ground, looking at a mark on the AZ-rotation platform at start. To compare, after AZ rotation, the "encoder 360", versus the "true 360" when the mark is again at the theodolite cross hair. This certainly is possible and worthwhile, as a first step. It would measure the error, as a function of rotation direction and speed: steady, changing, zigzag. But it could not tell where, or along which part of the circle, the error appears, which could give a good hint for improvement. This would need stepwise measurements during the AZ rotation.

A suggestion for this goal was to mount a theodolite on top of the feed arm structure, or above the secondary feed, at the axis of the AZ rotation; measuring the true angles, to a most distant point. To be compared with the AZ encoder. The distance must be large because the theodolite location may change a bit during rotation (from misplacement, or uneven AZ rails). For smaller distances, one would need three points about 120° apart, to separate angle from location. It is basically a good method. But a bit tedious and cumbersome, needing a platform for the observer, to walk stepwise around in a full circle. Regarding elevation, it could be measured from ground by two theodolites and triangulation, and compared to the two encoders. But which part of the dish, deforming with elevation, should be looked at by the theodolites?

What I would like most for the EL, would be to buy a good <u>inclinometer</u>, of 90° range and ± 1.0 arcsec, mounted permanently close to the dish center, and if not too expensive, a second one on top of the feed arms. The output going to the control computer, being always available to the operator.- And for the AZ, I would love to see a <u>gyrocompass</u>. If very expensive, borrow one from Space research, the Navy, or a shipyard. If we can buy one, we may not need the nautical type which orients itself slowly to the Earth axis; maybe we get a cheaper one which just holds a given (≈ axial) direction within ± 1 arcsec, during one stepping AZ rotation of, say, ½ or 1 hour. Mounted close to the center of the AZ structure, wired again to the control, and compared with the AZ encoder.

Finally: what are cost and accuracy of inclinometer and nautical gyrocompass? If affordable, I can very well imagine both as a permanent <u>integrated</u> part of a future pointing system. This could even reduce the large pointing errors from sunshine and wind.

Refraction was also discussed in Bonn. I heared that instead of the old less suited measuring location, two new ones have been selected at different places, promising good results. As to the equation you will use, please make sure it does not diverge close to horizon (the one I found at Green Bank did so). Most equations I have seen, give the EL difference, observed minus true, as a complicated function of the given true EL; which is not easily inverted, if you want to get the true elevation of a source at a given observed EL. Usually, though, a well selected iteration will do in few steps.- I also was told that a radiometer will be bought, to measure the atmospheric **extinction**, which sounds very good, for real-time corrections.

V. Summary of Results and Suggestions

The efficiency data for various wave lengths, collected by Altenhoff, contain observations with very different illumination widths. To determine their common rms surface error, it seems best to derive their effective illumination diameters D_e from their observed halfpower-beamwidths, and to use them to normalize the aperture efficiencies η, which apply then to their aperture D_e instead of the 100 m. Plotting $\ln(\eta)$ over $1/\lambda^2$ must then give a straight line, and from linear regression I get, for the main reflector and at 35° elevation, the long-wave $\eta_0 = 52.5 \pm 2.6$ %, and the rms $\sigma = 0.407 \pm 0.016$ mm. The correction (6) for surface curvature should always be used.

At high elevation, there is a strong discrepancy. For the additional gravitational deformation, moving from 35° elevation to 80°, the observations give $\sigma_{gr} = 0.493 \pm 0.005$ mm; whereas the careful structural analysis of firma Vertex gives only 0.274 mm. This could indicate a lateral feed offset, perpendicular to optical and elevation axes, by $\Delta Y = 10.5$ mm. Which could be checked by extended beam mapping, looking for coma lobes. (Or a different explanation?)

Altenhoff observed that the beamwidth decreased with high elevation, suggesting stronger central deformations, which I found confirmed by Vertex plots and tolerance theory. And from both Vertex data and observations, I suggest that a new surface adjustment should change the present adjustment angle from 35° to 50° elevation, for equal performance at 20° and 80°. Shortwave observation is bad close to horizon, and there is only few sky close to zenith. While adjustment at 45° would give equal performance at the extremes, at 0° and 90°.

Regarding the increase of the wind force with the new panels, the structural stability seems alright in general. Any danger I suspect only for the members directly holding the panels, which can be checked with little stress gauges. Wind-induced surface deformation is small, but pointing errors may be larger. The essential wind problem is the elevation drive motors. They were not strong enough even for the wire-mesh, which was the main reason against solid panels, as described in detail by Reich.- The new panels are specified for 0.50 mm rms, but their thermal deformation makes it larger below 10°C. Which seems a bad problem in winter.

For the present subreflector I derived $\sigma = 0.50 \pm 0.07$ mm. It will be remeasured and readjusted, with the hope to achieve $\sigma = 0.25$ mm. To order a new one was also considered. One would like to have it more accurate, less massive to allow fast wobble, deformable if the primary has enough astigmatism (best measured with an elongated rotating horn), and with a new light-weight hexapod mount giving all 6 degrees of freedom, as needed for optimum orientation. We need cost estimates, for several types of reflector and mount.

For new panels and $\lambda = 3.5$ mm, I expect a beamwidth of 9 arcsec, demanding a pointing accuracy of 2 arcsec. The main problem is hysteresis, in elevation and azimuth, up to 10 arcsec; unexplained and not to be tolerated. For a detailed reliable measurement, with regard to the ground, Fürst suggested methods with theodolites, good to start with. If not too expensive, I suggest an inclinometer with 90° range and ± 1 arcsec accuracy, permanently mounted at the dish center, wired to the control computer. Maybe a second one, on top of the feed arms, to detect anything loose. For azimuth I suggest a gyrocompass, borrowed from Space or Navy, mounted close to the axis. If affordable, my ideal would be the permanent inclinometer plus gyrocompass, both wired to the control, as an integrated part of the pointing system. This would even reduce large pointing errors from sun and wind.- Please try to get cost estimates.

And, when you apply for money, mention always the Green Bank Telescope, coming soon.

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