Dear George and Edwin,

January 17

In corroborating George's flux integrations of various components in A2256 I analyzed the zero-level of the map around the cluster and became more confident of the existence of the diffuse D component.

Figure 1 shows a line printer plot of the cluster at 50cm. One unit equals 0.45 Westerbork unit. I picked four areas to determine the zero level in the NE, NW, SE and SW corners which are shown. For each area at 50cm and 21 cm I made a histogram of the distribution of intensities and these are shown on the following pages. At 610 MHz three offsets agree very well with the SE significantly higher. At 1415 MHz perhaps the SE quadrant is a bit higher. In estimating the center of each distribution I ignored the right-hand tail which is contaminated by emission from the cluster.

From the histograms I have concluded a general offset in the SE quadrant of 0.10±0.02 mJy/beam at 610 MHz and 0.02±0.02 mJy/beam at 1415 MHz. One beam area is very nearly one square arcmin. The main uncertainty of diffuse D is the knowledge of the angular size rather than its general brightness level. Ten arcmin is a reasonable guess from Figure 1 or Paper I which leads to 100±20 mJy at 610 MHz and 20 ± 20 mJy at 1415 MHz. The ratio of the flux density between the two frequencies [100±20/20±20] is reasonably well-determined and I believe this shows that diffuse D has a relatively steep spectral index.

We also have a missing flux problem at both frequencies but I don't think this has any import on the existence of diffuse D.

I propose we strengthen somewhat our discussion of the diffuse D component by describing the above-mentioned analysis. I have send such a draft to Alan for inclusion in the final (?) draft.

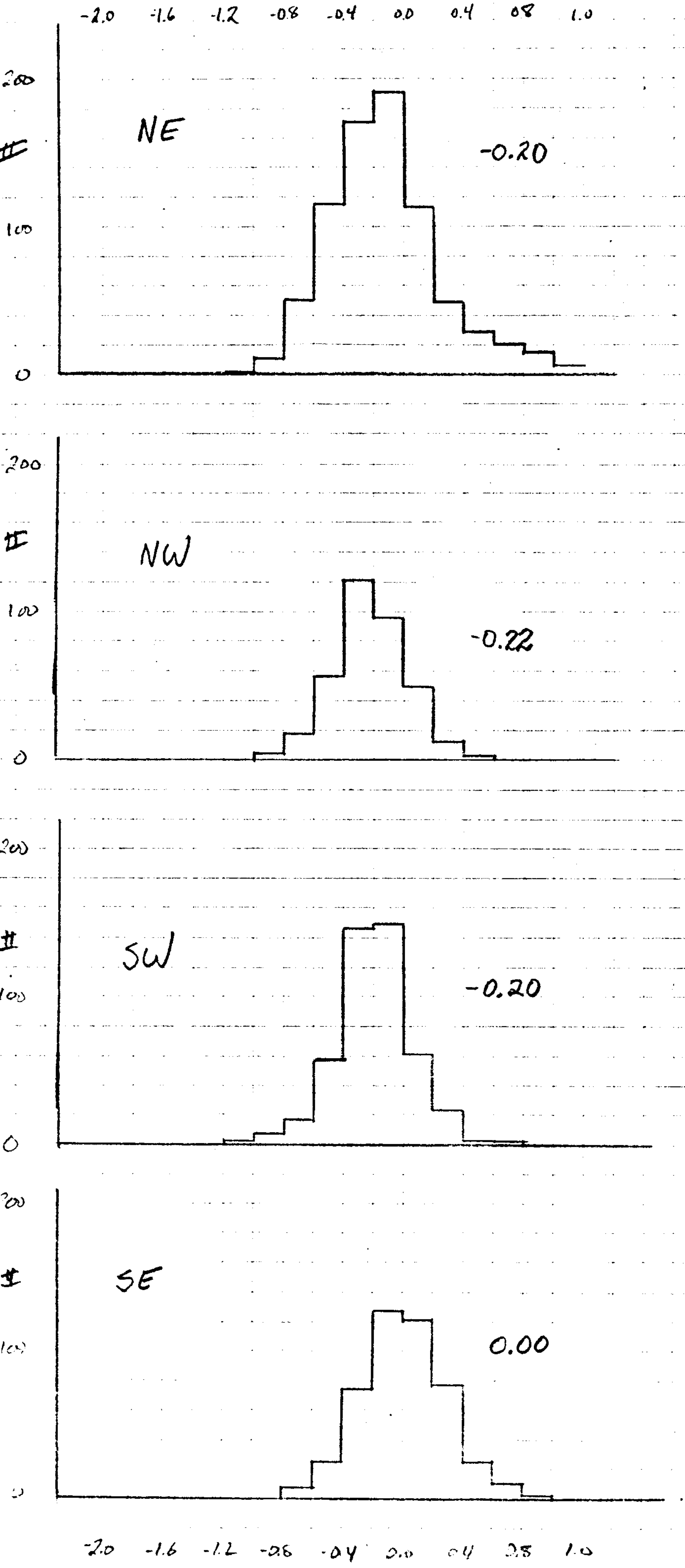
I was, in general, happy with the version you sent from Holland with the changes made recently by Alan.

Ed F

610-MHz offset

1 UNIT = $\frac{1}{10}$ WEST UNIT

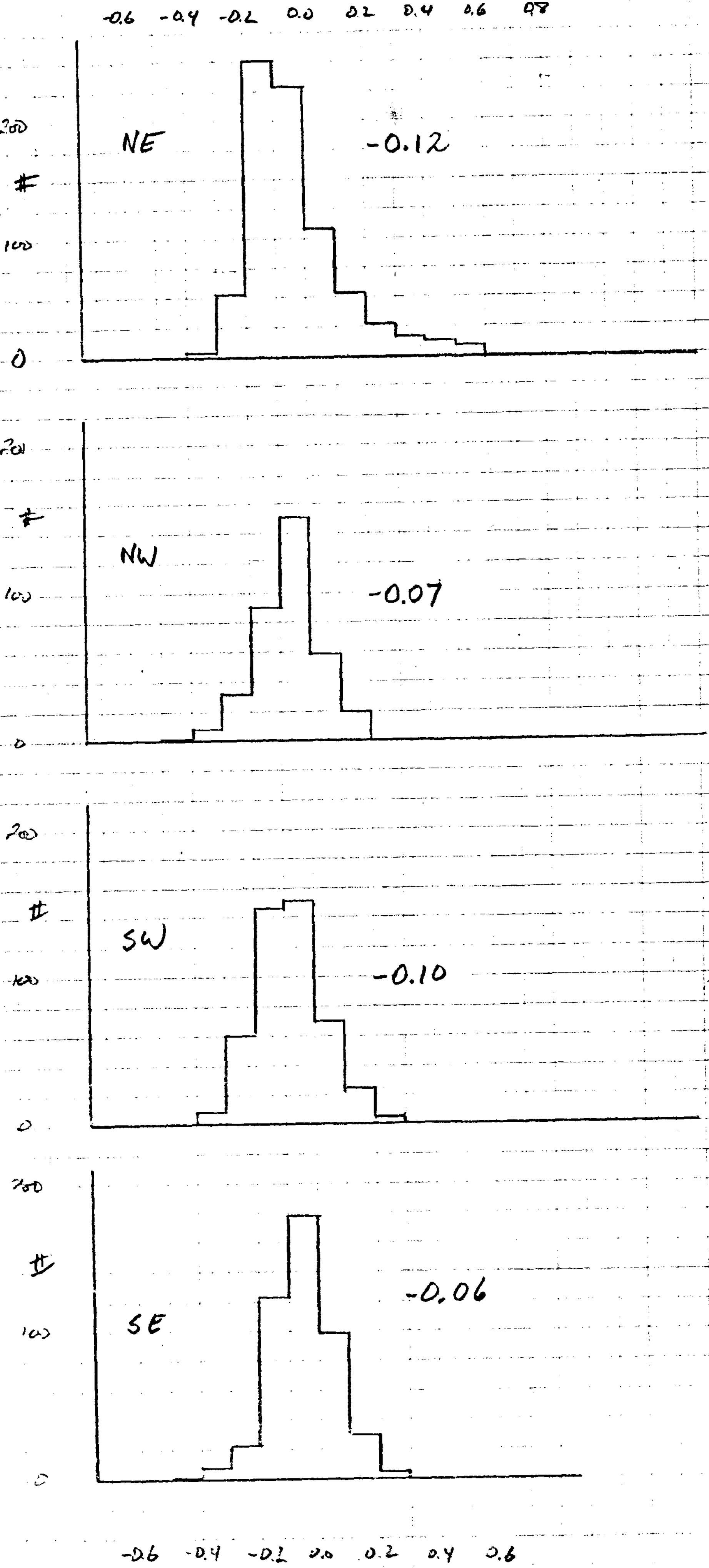
= 0.5 mJy / beam



1415 MHz offset

1 unit = $\frac{1}{10}$ Westerbork unit

= 0.5 mJy / beam



MAP OFFSETS AND DIFFUSE COMPONENT D

	610 MHz	1415 MHz	
OFFSET IN MAP	-0.103±.004	-0.048±.008	mJy / beam
offset near D	0.000±.02	-0.030±.02	mJy / beam
excess near D	0.10±.02	0.018±.02	mJy / beam

1 beam = 7.64 cells = 7.64 × [21."2]^2 = 58."8 square

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Dear Alan and Ed,

Here is a revised and (hopefully!) complete version of the Abell 2256 paper.

As you see Ed and I have radically rewritten the discussion section. I have several reservations about the previous draft.

First, in my view too much emphasis was given to the thermal versus inverse Compton X-ray problem as a motivation for the observations. In fact there was little chance that the 1415 MHz measurements could have discriminated between the two hypotheses. Anyhow a thermal origin for most of the cluster X-rays is now pretty well established from the Fe observations.

Secondly, in my view (Edwin agrees) the old section IV is based on too many shaky assumptions to be very meaningful. Just to list a few

1) F(II) is imbedded in the diffuse component. It is just as likely (to me more likely) that the diffuse component is a westward extension of Source F.
2) The identification of Source F. Probably has a 1 in 3 chance of being correct.
3) The volume of the diffuse component. I do not think there is convincing evidence that it covers the 10' diameter sphere uniformly. In fact it could well be (see assumption 1) a cylinder with dimensions ∿ 7' x 3'. This would reduce the volume by a factor of nearly a hundred.
4) The spectral index of the diffuse component. In my view we can only say that $\alpha > 0.8$. {The 610/178 MHz extrapolation has too many uncertainties (mating of the two beams/shortest baselines etc.) to be used as anything other than a very rough guideline. Look at the difference between the two 610 MHz maps! The missing flux is critically dependent on the shortest spacing.}
5) Of course the energy bounds over which the spectrum must be integrated for both the diffuse component and Source F are also uncertain.
6) The filling factor for F(II) is unity.

Maybe one could derive conclusions using one or two of these assumptions but taken as a whole I feel that it is a bit too much.

By the way Edwin or I cannot see how you get B = 50μG for the mag field in the diffuse component. Wouldn't pressure balance with an equipartitioned F imply that the magnetic field of the diffuse component was the same as the equipartition field of F i.e. ∿ 4μG.

Regarding the luminosity function we have stuck with Edwin's magnitudes. There is a worrying disagreement between his and your magnitude scales. However, the only important question is whether Abell 2256 has an anomalous RLF or not. Edwin's magnitudes have been derived in an exactly similar manner to those of the comparison sample of Auriemma et al.

Can you let us have comments changes etc. as soon as possible, because we would like to submit the article before HEAO-B data makes the discussion obsolete.

Wishing you both all the best for 1979.

Cheers,

George

George.

P.S. Tables 2, 3 and 4 will follow in a few days.