

#1 27-MAR-1987 15:34:32

NEWMAIL

From: CVAX::ABRIDLE
To: RLAIN, ABRIDLE
Subj: Meeting finalises paper

I have to go to a computer planning meeting that will go on until after the mail leaves here this afternoon. I have therefore run off the double spaced version of the paper and put it into the paper mail. I'll do the preprint version on Monday (and make any further changes if you didn't like the trend of my last E-mail). I realised when making the last changes that your comment about the missing flux brightness was probably provoked by the typo mJy for μJy in the text I E-mailed (not by a disagreement over the numerical result) ! But I think the more careful sum I have done today is more worthy of M.N.'s sophisticated readership anyway.

I presume you will compose a suitable cover letter for His Shakeshaftship.

Let me know if there are any residual problems, and I'll mail a revised doublespaced version on Monday. But I think this may be the end of our

Press RETURN for more...

MAIL>

#1 27-MAR-1987 13:15:30

NEWMAIL

From: CVAX::ABRIDLE
To: RLAIN, ABRIDLE
Subj: Missing flux

I've looked a bit more closely at our uv coverage, given the sensitivity of the "missing flux" arguments to the actual short spacing coverage. We do in fact have foreshortened spacings down to 106 m at 20cm and to 32 m at 6 cm, though they come on only one track. If you convert these into angular scales of a Gaussian FWHM at which we would still pick up half the flux, they are 2.9 arcmin at 20cm and 2.8 arcmin at 6cm. So perhaps a not-quite-so-worst case is to say that if we recovered all of the flux from the visibility function we might miss half of what is in an angular scale of 2.8 arcmin. If there's 100 milliJy on that scale we would then miss 26 microJy per beam. I'll go with this calculation unless I hear from you otherwise. Question still is - should it be in the paper or in the letter to JRS ?

MAIL)

#22 27-MAR-1987 10:02:47

MAIL

From: CVAX::GATEWAY::"RLXUK.AC.RO-GREENWICH.STARLINK@AC.UK"
To: ABRIDLE AT NRAO
Subj:

Date sent: 27-MAR-1987 14:21:06
To: ABRIDLE@NRAO

Soory I couldn't get back to you until now: the PSS line to the UK was down. I am happy about the changes, except that you must mean 10 microJy/beam for the distributed flux! Also, there is an "and" missing in the Acknowledgements in the sentence thanking EWG. On the matter of minor-axis PA's, I am in a slightly embarrassing position in that I have had to read the Sansom et al. paper (not a pleasant experience, but I think that the data are OK) and might find it difficult to explain to the authors why I ignored it! On the other hand, they contradict Palimaka et al. and aren't actually published yet. That is really why I wanted to ignore the issue, but I do not feel strongly either way.

Doubtless, JRS will mess around with the units if he cares about them!

Press RETURN for more...

MAIL)

#15 20-MAR-1987 10:35:18

MAIL

From: CVAX::GATEWAY::"RL%UK.AC.RO-GREENWICH.STARLINK@AC.UK"
To: ABRIDLE AT NRAO
Subj:

Date sent: 20-MAR-1987 15:23:27
To: ABRIDLE@NRAO

Dear Alan,

Thanks for the message. I suggest that we compare measured and single-dish fluxes (Laing & Peacock have BDFL/Kellermann & PT corrected to Baars et al., so that will do as a reference) and make the point that the largest scale is that of the lobe. We then say that any missing flux is less than $x \pm$ a lot on a scale $>$ something and could not possibly influence the measurements. A note in the caption to the DP - RM plot as to a typical error bar is quite enough, I think. As to units, I still don't like Km^{-2} and would prefer X-ray temperatures in K, otherwise I am not bothered. Still, we may get edited!

Regards,
Robert

MAIL>

Reply to referee's comments on "Rotation Measure Variation
across M84" (M116; Laing & Bridle)

(1) We have included a Table giving full details of the observations. Duplicated information has been removed from the text.

(2) The "standard VLA documentation" mentioned by the referee is presumably the "Observational Status Report". The limits refer to components of a particular angular scale. As we now discuss in some detail, this means that we might have missed a halo surrounding the whole source, but not any flux in the lobes. By comparison of flux densities determined by integrating over our maps with standard values, we show that the surface-brightness of such a missing component is below the noise level. There is no "unused C-configuration data" (as Table 1 now makes clear).

(3) Some details given twice in the text and Figure captions have been removed from the former (Figures 2, 3, 5 and 6).

(4) The integrated luminosity should be $1.9 \times 10^{23} \text{ WHz}^{-1}$ at 1.4 GHz.

(5) We have amended the values of density and temperature for the Virgo cluster atmosphere given in the Introduction.

(6) The values of the RM gradients in 3C 66B and NGC 1265 are a bit more problematical - the larger gradients suggested by the referee depend on single points. On balance, we feel that our original estimates (approximate!) are adequate. We have, incidentally, added a comment about the effects of anomalies in the Galactic RM distribution.

(7) We have indicated the typical and maximum errors in the caption to Figure 10.

(8) Finally, we have made the comparison of pressures easier.

We trust that these changes will meet with the referee's approval and are grateful for a constructive critique. Any attempt to include more observations will lead to an even more ludicrous delay, so we assume that the referee does not wish the final paragraph of his or her report to be taken too seriously.

OK?

R. Laing

From: CVAX::GATEWAY::"RL%UK.AC.RO-GREENWICH.STARLINK@AC.UK" 13-MAR-1987 18:54
To: ABRIDLE AT NRAO
Subj:

Date sent: 13-MAR-1987 23:53:01
To: ABRIDLE@NRAO
Dear Alan,

I have got the referee's report back on the M84 paper. There are no serious problems, and no comments at all about the science. I am just off to La Palma for 2 weeks and have had no time to get into TEX yet, so if you could make the alterations and send by post, I would be most grateful.

I paraphrase the comments and suggest some emendations.

(1) More details about the observations in the form of a table. Something like: configuration, date, frequency, bandwidth, integration time, HA coverage, rms noise levels in IQU.

I don't have the exact integration times to hand (easiest way is probably to get the number of visibility records). Otherwise, the numbers would seem to be:

Conf.	Date	Freq. GHz	BW MHz	HA cover	rms mJy	I	Q	U
A	1980 11 09	1.413	12.5	^{2hrs of} Cuts -6 to +6 h !	0.40	0.18	0.17	
B	1981 06 25	1.413	25	^{1hr of} Cuts -4 to +4 h !	0.40	0.17	0.17	
C	1981 11 16	4.885	50	^{5.5-hr} Continuous track -4 h to +4 h 336min. ^{7hrs of cuts} -4 to +4	0.05	0.04	0.04	
B	1981 06 25	4.885						

I am not absolutely sure about the coverage, but can't be far off. The referee was confused by the first para. of Section 2 into thinking that there was unused C-configuration data at 1.4 GHz. If we just replace this para by a reference to the Table, this will not matter. The references to noise levels in Sections 3.1 and 3.2 could then be removed.

(2) The referee is worried about inadequate sampling of the low spatial frequencies since "according to standard VLA documentation, the observations used are not adequate (by a factor >2) to map such a large source." What it means is that the Observational Status Report says 80 arcsec is the largest size for a full synthesis at 6cm C/20 cm B. We can get out of this by saying that the integrated flux densities from the I maps were 2.79 Jy at 4.885 GHz (cf. 2.88 Jy as interpolated from Laing & Peacock 1980) + whatever they are at 1.4 GHz (I am afraid that I don't have a value for the integration over the map, but the interpolated single-dish flux density is 6.43 Jy). L&P flux densities are adjusted to be on the Baars et al. scale, so all should be self-consistent. Given that the total flux has been recovered, we can say some reassuringly anodyne words about the effects on the measurement of depolarization, which is what the referee appears to be worrying about.

(3) There is a claim that "much of the information in the figure captions is duplicated in the text". Some of this, as I recall, was deliberate. The possible excisions in the text are:

- Fig 1 none ✓
- Fig 2 Figures 2(c).....clarity in the display (p6) ✓
No polarization vectors.....4:1 (p6-7)

cleaned
635
Inley
6.34

OK-do

"The vectors are shown.." (p6) to "4:1" (p7). The first 2 sentences of 3. 2 seem to me to be an essential introduction.

- Fig 3 Cut from "The vectors in Figure 3" (p8) to "vertical"
- Fig 4 none
- Fig 5 Transfer sentence "Note that Figures 5 (a) and (b)... frequency (p9-10) to caption?"
- Fig 6 Move "Vectors are drawn.....4:1" to the caption and omit "In the absence of noise....vertical" from the caption, as it is already in the text?
- Fig 7 none
- Fig 8 none
- Fig 9 none
- Fig 10 none
- Fig 11 none

Does this look like a sensible response?

(3) Numerical things

Referee says that the luminosity is 1.8E23 W/Hz at 1.4 GHz. I get 1.9E23 when I do the sum for D = 15.7 Mpc and S = 6.47 Jy (L&P). Don't know where 1.4E23 came from (not Bridle & Perley, who say 1.7E23).

Also complains about the values of ne and T for the Virgo ICM/M87 halo in the Intro. Says that ne is < 2E-4 cm-3 at >400 kpc for M87, and T is about 2E7 K (although this is very model-dependent). Both these assertions appear to be correct, although not relevant.

RM gradients in NGC1265 and 3C66B are said to be 30-40 and 80 radm-2, respectively, along jets. Looking at Fig 6(e) of Leahy et al., I would have said about 60 radm-2 for 3C66B (given the size of the error bars). The difference between 20-30 (us) and 30-40 (referee) for NGC1265 is ludicrously silly. I think that we can safely ignore this comment.

Leahy 60 rad/m^2 in 8 arcsec = 2.4 kpc.

(4) Misc.

Reference to Leahy et al. should be A&A 156, 234 (not 158, 234).

Representative error bars on Fig 10 (yawn). *→ caption.*

The referee wants SI units (they are actually MN policy). I must admit that I almost changed the Km-3 unit, but left it in to see what would happen! It seems to me that we could either: change only the bit at the end of p18 (pressure to ergcm-3 and temperature to K), which is the only confusing bit, or put things consistently in SI. This means that one of us has to remember whether a nT is bigger or smaller than a microGauss. I am marginally in favour of the second idea i.e.

- densities to m-3 *x 10^6*
- nBL in nT.m-3.kpc
- luminosity in W (we use W/Hz anyway)
- pressure in Nm-2 *Pascal = 10 dyne/cm^2*
- temperature in K (not keV)
- Tesla = 10^4 gauss nT = 10^-5 gauss.*
- 1 Km^-3 =*

I suspect that this will get changed for us by JRS, so it might be as well to do it now to avoid errors. What do you think?

The referee ends up with "I regret that, after their data have been 5 years "in the can", the authors should have chosen to present only half of it." Various comments come to mind, ranging from "mea culpa" through "I wonder what the referee has been doing in the mean time" to "**** ** and I was going to say thank you until I saw that". The report was sensible, if pedantic, so an acknowledgement would be appropriate, I think. It was

A few additional thoughts of my own:

On p11, the description of the construction of the pseudo-2.7 GHz map is unduly modest. Why not "the approximation is a good one for our purpose"? ✓

p16 Could you remove the multi-sentence bracket from "Similar conditions" to the end of the first para? I decided that it would be difficult to follow and took it out in the submitted version. ✓

Finally, I have cold feet about the reference to minor-axis alignments. The result appears to have gone away for the Shaver et al. sample (when looked at with better optical data) and does not now appear to be significant for a combined sample with decent radio and optical PA's (yours + Shaver et al. + Birkinshaw & Davies, basically). Would you be terribly offended if we removed the last sentence of p. 19? (Incidentally, we do have some evidence from an image of M87 taken in very good seeing on the AAT that the isophotes do rotate to give minor-axis alignment close to the middle of the galaxy, albeit at very low ellipticity, but thoughts on all of this should wait for a more relaxed occasion).

Could you send a revised top copy + a version suitable for preprints if you are happy about these suggestions? I will arrange for distribution.

On another matter, various of the UK astronomy/computing community were wondering whether AIPS was planning to adopt the IDI standard for image display. Eric's complaint about GKS was always that there was no concept of a pixel - this is the image extension which goes with GKS. STScI and STARLINK are certainly going to use it.

STARLINK is now employing an applications programmer (Richard Prestage, who did his PhD with John Peacock at ROE and then went to Steward for a postdoc) to work on synthesis software of interest to the community at large (i.e. VAX AIPS) and JCMT reduction packages. He is based at Jodrell. He has got stuck in to producing a good set of TV routines for the SIGMA Args (nothing having materialised from Paris) and will then do some for the Digisolve Ikon (a much better and cheaper display, which will gradually replace the ARGS over the next few years). The STARLINK management is now prepared to distribute AIPS updates to selected sites and is being very cooperative. The feeling is that the computing requirement for the RGO/Cambridge site may well justify a big vector machine as well as the MicroVaxes which are proliferating around IoA at the moment. Those who decide STARLINK policy are much less hung up on Dec machines than I has thought, so we may end up with quite a good mix.

I will be on La Palma from March 16 - 29, but can log in from there (Spanish PSS lines permitting).

Regards,
Robert

Royal Greenwich Observatory,
Herstmonceux Castle,
Hailsham,
East Sussex BN27 1RP
January 28th 1987

Dear Alan,

Thanks for your letter. I have not received any of your attempts at E-mail. This is odd, because Frazer is able to get through from the VLA. My address is RL@UK.AC.RO-GREENWICH.STARLINK and Frazer's message went via HAMLET and the EARN gateway at the Rutherford Lab. Have you tried via Caltech? Can you get through to any of the STARLINK machines from Charlottesville? I will enquire as to possible difficulties at our end. Just at the moment, all of our access to the outside world by E-mail is dead and cannot be repaired because British Telecom engineers are on strike, but this is a relatively recent wrinkle on the problem.

The M84 calibration story is as follows:

A-configuration data at 20cm after correction for ionospheric Faraday rotation using hourly values:

T_{AC} (3C286) = -171, -174, -174, -168, -173 (mean -172 deg), followed by a jump to 177, 176 (occurring at an obvious time). The data were corrected in two blocks.

B-configuration data at 20cm, corrected using monthly median values (since hourly values were not available):

T_{AC} = -123, -120, -116 deg (corrected using a slope)

In both cases, the position angles for the synthesis calibrators were consistent with the assumed values, but were quite noisy. For the 6cm data, all 3 arrays had T_{AC} consistent with a constant value throughout the run, with a maximum excursion of 2 deg. As you may recall, the first run had very poor AC phase stability before correction for IFR, and we were lucky to repair it. There appear to be no hourly data for the B-array (I thought at first that I had analysed it before the data arrived, but checked again later).

I suggest a form of words roughly as follows:

The amplitude scale was set by observations of 3C 286, whose flux density was assumed to be ... Jy and ... Jy at 1.413 and 4.885 GHz, respectively (Baars *et al.* 1977) and is believed to be accurate to 2 per cent at both frequencies. Initial phase calibration was done by interpolation from observations of 1236+077 (A configuration) and 1252+119 (B and C configurations), which were also used to determine the instrumental polarization. The zero-point of E-vector position angle,

χ , was set by observations of 3C 286 which was assumed to have $\chi = 33^\circ$ at both frequencies. Corrections for the effects of ionospheric Faraday rotation were applied using measurements of atmospheric electron content from the World Data Center and a simple model of the Geomagnetic Field (reference?). The systematic error in χ , as estimated from the scatter in the measurements for 3C 286, is less than 1° at 4.885 GHz and less than 3° at 1.413 GHz.

We should also mention that the bandwidths were 50 MHz for all of the 6cm observations and 12.5 and 25 MHz respectively for the A and B-array observations at 20cm. What is the standard reference for VLA calibration these days?

I did receive the glossies. I am not sure what changes you made as a result of discussions with Simonetti and Killeen: was there anything major?

I cannot remember whether I mentioned to you what I felt was the matter with the A+B+C array map of M84. I am fairly sure that the core has varied slightly and that this is the cause of the low-level artefacts near the nucleus. I started with the B+C and A-array datasets. The B+C data had already been corrected for a difference in core flux (B array had 10.0 mJy subtracted). I suspect that the A-array is not consistent with this. A pure A-array map gave a peak (MAXFIT) flux density of 179.8 mJy. I do not have the analogous number for the B+C array map (you have a copy on tape), but the map maximum was 180.1 mJy, so the fitted value may be larger. If the relative scaling of the two maps is set by the flux in larger-scale structure (as it might well have been in the procedure I used), then there may be a problem. You may well have realised all of this, but your comment about mapping larger fields suggested otherwise. Mind you, the maps aren't too bad as they are!

Have you done anything more on 3C 334? Do you believe in the counter-thing? I was impressed not so much by that, but by the weak substructure in the hot-spot. I have found several examples of this now, but it is invisible in typical maps of resolved hot-spots (including earlier ones of this source).

The result on depolarization asymmetry has got much stronger as a result of the follow-up project. What really convinced us was that when we looked at the only blatant counter-example, we discovered that Peter Barthel's map (which gave us the jet direction) was printed upside-down!

We are about to increase our usage of AIPS, as both new post-docs (Steve Unger and Eli Brinks) have a radio-astronomical bent. I will probably put a large subset of it permanently on the 780 instead of restricting it to a removable disc. I have been working on a version of QMSPL for out Canon laser-printer and now understand the contouring, although grey-scales are still beyond me. I only really understood things when I acquired a QMS manual as well as one for the Canon (which is in Japanese English). As Jodrell now has an Alliant and STARLINK provided a programmer (based at Jodrell), who might be expected to write TV routines, things are looking up on the computing front. I am trying to

convince people that they need a big vector machine for the Cambridge node (which is currently a disaster area) but am running up against the problem that STARLINK is wedded to VMS. Please convey my apologies to Eric for ever doubting the need for portability.

I won't bore you with the hideous mix of politics and commissioning chores which constitutes life at the RGO. I enjoyed the snow because the RGO was quiet and I could walk in to work whilst the road was blocked. Any good Canadian would sneer at the utter inability of England to deal with snow, but I remember Charlottesville as being just as incompetent.

Best wishes

A handwritten signature in blue ink, appearing to read "Robert", with a horizontal line underneath it.

P.S. Have you given up the idea of coming to Cambridge this summer?



NATIONAL RADIO ASTRONOMY OBSERVATORY

EDGEMONT ROAD CHARLOTTESVILLE, VIRGINIA 22903-2475
TELEPHONE 804 296-0211 TWX 910 997-0174

23 Feb 1987

Dear Robert,

Here, at long last, is what may be a
submittable version of the M84 RM manuscript.
I leave it to you to decide whether to put it
with cover letter and glossies and send to
M.N., or to E-mail me any further changes.

A few (small), mainly English, changes are
included. I have quoted parameters for the
King model in the introduction, rather than the
vague mean electron density over the source that
was used before. Also dropped t_c , as it was
not used later.

Best wishes.

A handwritten signature in cursive script, likely belonging to the sender of the letter.

NATIONAL RADIO ASTRONOMY OBSERVATORY
Edgemont Road, Charlottesville, Virginia 22903-2475

Dr. Alan H. Bridle, Tel. 804-296-0375, FTS 940-7375

January 16, 1987

Dr. R. A. Laing
Royal Greenwich Observatory
Herstmonceux Castle
Hailsham, East Sussex BN27 1RP U.K.

Dear Robert:

I've tried various ways to send you E-mail through our "new, improved" networking. Some of them obviously didn't work (i.e., I got a recognisable "can't deliver" message from an identifiable machine in a finite time) but others provoked no response either from the network software or from you. Did you get anything at all from me ?

Meanwhile, M84 epic awaits the calibration text for last draft. Could you send me what you want to put in ? Did you get the glossies o.k. ? (I haven't heard from you since I sent them).

Hope you are surviving the Big Freeze over there. I won't tell you how warm it's been here.

Main AIPS news is that calibration software is appearing at a steady rate, and Phil Diamond has arrived to join the group. We have largely absorbed Gary Fickling's work into Eric, Nancy and some part-time help from Leroy Napier. So we are going to try to keep Phil as a second "Bill Cotton" - i.e. mainly applications code, especially spectral line and VLBA.

Best wishes



From: VAX3::LAING
To: CVAX::BRIDLE
Subj: Datasets

10-SEP-1986 19:55

I will send you copies of the M84 ABC and 3C 334 AB datasets and maps. In fact, I see little point in my keeping the uv data, as I would have to throw everyone else off the Jodrell Starlink node to do anything. I suspect that some experimentation with subtraction of a point source would help for M84. I won't be able to do this, but will work out what I think ought to be done when I get home. I will be in Erice from September 16-25, thereafter at home until the end of October. Could you let me know when the link via UVa is likely to be accessible?

Two minor suggestions/typos in the draft I have: on p6, there is a missing full stop in the last para of Section 4 and I suggest that "also" should be deleted on line 4. The structure function stuff sounds interesting. I look forward to seeing it. The task to fit synchrotron loss curves to AIPS cubes is called PSSPY and lives in [AIPS.PAT] on both VLA Vaxes. I haven't done much with it, as I did not get my hands on the necessary MERLIN data in time.

I have got the final graphics for a respectable number of projects, so the first priority when I get back is to dump those into M.N., avoiding the microfiche trap. Progress on map analysis should be much faster now that I have the MicroVax. If only I had a TV. Also, the 4.2m tests seem to have slipped until January, so the last quarter of the year contains only one trip to La Palma.

Good luck with the viniculture.

Robert

From: CVAX::BRIDLE "Alan Bridle" 10-SEP-1986 14:53
To: VAX3::LAING,BRIDLE
Subj: RE: Figs

I will send two sets of glossies (one for you, one for M.N.) to RGO and also a set of 35mm slides. We should now be very close. Neil Killeen has almost finished commenting on it; I will be surprised if we hear from Ed, but you might bug him out there if you see him.

John Simonetti has been motivated to write an AIPS task that calculates 2-D structure functions from AIPS images, and we ran it on the M84 3.86 arcsec resolution RM image as its first real-life test a few minutes ago. The banded structure shows up beautifully, also the two major peaks that correspond to the E-W maximum fluctuation of the N lobe and the N-S maximum fluctuation of the S lobe. Slices through the structure function map that his code produces might still be a good idea for our paper, though I think one can describe it all in words reasonably well. I'll send you a FITS tape with the structure function images soon, so you can decide whether they are worth adding to the paper.

I'm not surprised you did not get to 68.1; I am still grinding away on 3C175, even with the speed of the Convex behind me. I now have all of the data communally calibrated, but lack the space to sort the fully combined data set (Paul Coleman is here processing his mammoth survey alongside me). I do have the straight addition of the AC and BD clean maps for the A+B arrays however. This has an rms of 16 microJy and is pretty faint away from the source; looks almost noise-limited except just around the hot spots outside the lobe. There is still no counterjet.

Will you be sending me a copy of the M84 superdata from VLA or RGO ?
Do you want a copy of 3C175 ?

Have a good trip back. I'll do what should be a last edition of the text of the paper as soon as you let me know what you'd like to insert into the calibration section.

From: VAX3::LAINB 9-SEP-1986 13:12
To: CVAX::BRIDLE
Subj: Figs

Please send the Figs to R60 - I have disabled any further forwarding of mail. I will not get to 3C68.1 before I leave, as 3C334 has held me up more than I had anticipated. I have got rid of some bad correlators, but there is still a little garbage around the core. I think that I can get it to the optimum point PROVIDED that I can gather enough disk space to sort the dataset again. M84 ABC has progressed well. I think that there is a residual problem with core variability, but the datasets are phased together rather well. The only artefacts are very close to the nucleus. I will probably get a final map at 0.4 arcsec, but not at the lower resolutions. I will probably get a final map at 0.4 arcsec, but not at the lower resolutions. The whole process is badly machine-limited, both in space and CPU and I have had to switch machines more than once.

Given the state of the Dec-10, the easiest way for me to check the consistency of M84's AC phase calibration is probably to wait until I get home, where I can consult my notes. OK?

From: CVAX::BRIDLE "Alan Bridle" 1-SEP-1986 18:53
To: VAX3::LAING,BRIDLE
Subj: RE: M84

The north lobe "correlation" is that the largest departures toward very negative RM come in regions of low polarization.

We did not modify the graphics to show the fits -- what you saw is what we are getting. In fact it's easier for people to judge without having a line to bias them !

Software sounds interesting - do you have it somewhere on VAX3 that I might copy it from ?

Re AIPS -- you had mentioned to me that you should be the Contact Person for RGO MicorVax AIPS from now on, but I gather you have not told Nancy about it -- e.g., your address/phone, node name, configuration, etc. Could you do this before you leave ? Just send a Vaxmail to CVAX::NANCY.

From: VAX3::LAING
To: CVAX::BRIDLE
Subj: M84

29-AUG-1986 23:00

Simonetti's suggestions sound very reasonable. I am not quite sure what you mean by correlation between B-field organization and RM in the N lobe: could you explain?

Did you alter the PA vs λ^2 diagrams to show the fits - I can't remember.

I have concocted a way of fitting $\exp(-\lambda^2)$ laws to the degree of polarization (this seems to be a reasonable thing to do in many cases). I have also acquired a program to fit synchrotron loss spectra to multifrequency data. Any use?

From: CVAX::BRIDLE "Alan Bridle" 29-AUG-1986 15:22
To: VAX3::LAING,BRIDLE
Subj: J.Simonetti comments

John has read the MB4 paper, and here are his main comments:

1. He is convinced that the effect is in MB4, not in our galaxy, and largely convinced that it is a screen rather than being mixed-in (mainly by the depolarization argument).
2. He notes that there is some correlation between RM and B field organization (p) in the North lobe, but not in the South. I agree and think we should point out this possible difference.
3. He was puzzled by the description of the 11cm position angle "prediction", but did seem to have got the basic idea O.K. I think the real difficulty is still the sudden appearance of Q/I and U/I as parameters. Could you concoct any more explanatory text there ?
4. He strongly favored the Delta-chi displays and urged us to leave them in whatever the detractors may say.
5. He gave me a preprint of his paper with Jim Cordes on the structure function of RM variations at various latitudes. I will read it and concoct a suitable reference to it for the text. He would like to think about 2-d structure functions using our RM data; I have offered him a FITS tape of our RM image if he decides to code something up and would like to try it on MB4.

From: CVAX::BRIDLE "Alan Bridle" 26-AUG-1986 09:37
To: VAX3::LAING, BRIDLE
Subj: Some small text changes

I have been making some "final" changes to the M84 text, subject to your approval. The graphics are now at GB for photography. I will send you a new TeX file for you to print out, but here is a summary of my suggested changes:

Abstract. Add statement that we think the rotatory medium is responsible for the diffuse X-ray emission.

1. Minor English changes. Call distance d when first mentioned.
 2. Give resolution in pc as well as in arcsec.
 - 3.1 Give p.a.'s of both jets.
 - 3.2 Renumber figures to 2(a), 2(b), etc for consistency with graphics. Minor typography and English changes.
 - 4.1 Call position angle rotation $\Delta\chi$ as soon as it is used. Note that Figure 3 displays this quantity without resolving the 180° ambiguity (basically an English change).
 - 4.2 Tidy up Figure labeling γ 5(a), 5(b), etc. Approximate sign in front of 140 deg γ (last sentence).
 - 4.3 Add "smoothed" before "distribution" in last sent., para 2. Minor English changes.
 5. Change "jet axis" to "ridge line of the jet".
"brightness gradients in source" to "brightness gradients in lobes".
 6. Break second paragraph into two at "furthermore". Add sentence saying we expect mean foreground RM to be near zero just before break.

Bullet 3 - add explicit statement that Figure 9 shows no banded structure akin to that in Figure 4.

Minor English changes.

Third para from end - italicise "brightest" in first sentence.

Last para. - change "define the axis" to "influence the shape".
 7. Minor English changes.
- Fig. captions. Bunch of small changes to make them consisten with final graphics. I suggest you look at the Figures and the revised captions together.

From: CVAX::BRIDLE "Alan Bridle" 22-AUG-1986 17:06
To: VAX3::LAING, BRIDLE
Subj: M84 graphics

My own perusal of the M84 graphics suggested three changes:

Figure 5 Lower case for the ARCSEC labels
Figure 7 units missing from lambda-squared axes
Figure 11 (a) and (b) labels missing from panels.

I also propose that we label 11(a) 16" X-Ray and 11(b) 8" X-Ray.
OK?

I have received the AB369 B config tape; no disk space on Convex at the moment and we have a bunch of visitors on CVAX. So I will concentrate on the (hopefully final) edits to the M84 paper text.

Almost done !

From: VAX3::LAING 21-JUL-1986 20:17
To: CVAX::BRIDLE, LAING
Subj: M84 etc

Thanks for the revised version of the paper, the IMVIM plot (suitably random, I thought) and the optical data. A quick look at the plots of the H-alpha maps leads me to agree with you: Stefi's data look much cleaner. I am not inclined to believe the southern extension, although there is probably some emission in the area - I will try smoothing the data.

One typo I noticed in the paper: "sych" on line 11 of para 1.

In the near future, I will work on a more accurate superposition (I am sure that you have it nearly right, though) and will check the calibration.

The weather was reasonable during AB369, except at the very end, when I think a thunderstorm mutilated a scan or two.

Have a good trip to Canada.

Robert

From: VAX3::LAING 21-JUL-1986 00:38
To: CVAX::BRIDLE,LAING
Subj: M84 calibration

The comment about calibration means that I will check the polarization solution (instrumental and angle) as soon as I can get the relevant datasets on disk. Otherwise, the calibration is as standard as could be (3C286 with the usual flux densities and position angles). Disk space is at a premium, partly because I am in the middle of calibrating the new AB369 data. These look entirely adequate, with none of the sampler errors that afflicted the second A array run. I got the operator to reset the delays in the first hour, given that we had 26 hours instead of 24. I should have the calibration finished by tomorrow evening.

Regards,
Robert

From: CVAX::BRIDLE "Alan Bridle" 17-JUL-1986 16:59
To: VAX3::LAING,BRIDLE
Subj: M84 optical line data

I have been fiddling with the above-mentioned in the Convex. I have an approximate calibration of position and scale for both Stefi Baum's data and the Hansen data now. I have written the "calibrated" images onto the end of the original tape that Hansen sent me, and am now mailing that to you so that you can examine it yourself.

Points
=====

1. There are some stars on Stefi's map that could be used to check the calibration; ignore my earlier message.
2. Stefi's data look to be of better quality, and show little or no evidence for the southern emission extension on the Hansen map.
3. The Hansen data have some rather obvious subtraction problems, which may have helped "create" the southern extension.
4. The registration of the emission line data on the depolarization data does not suggest a good correlation, but the problem is that the apparently "repolarized" base of the north jet also sits in the emission line region. Registration accuracy is therefore critical. I will send you the preliminary overlays of optical contours on depolarization greyscale for you to draw your own conclusions about what to believe. My own feeling is that there is not much evidence that the southern spur on the Hansen map is real or that it correlates believably with depolarization on our map.

From: VAX3::LAING
To: BRIDLE
Subj: M84 positioning

13-JUL-1986 21:46

It occurs to me that it would be trivial to measure some star positions in the field of M84 if there are any visible on the chip. This would nail the position (and chip PA if it isn't perfectly known). I would be happy to do this on the measuring machine here, which is a joy to use. If you think that this is worthwhile, I will teleport the image from wherever you keep it.

Regards,
Robert

#1

10-JUL-1986 23:52:02

NEWMAJ

From: VAX3::LAING
To: CVAX::BRIDLE, LAING
Subj: M84 Paper

Dear Alan,

I have detected an error in Section 6. The depolarization law for a slab model is $\sin(2*RM*\lambda^2)/(2*RM*\lambda^2)$ if RM is the rotation measure OBSERVED. The Faraday depth is 2RM. This makes the argument that RM and depolarization are not correlated significantly stronger. For RM = 25 rad m⁻², the predicted depolarization at 20cm is 0.35 and for 35 rad m⁻² one hits the first null.

Regards,
Robert

P.S. Could you let me know the peak surface-brightnesses of the extra-nuclear emission in 3C 175 and 3C 68.1 - I did not bring the AB369 file with me.

#2

11-JUL-1986 01:46:38

NEWMAJ

From: VAX3::LAING
To: CVAX::BRIDLE, LAING
Subj: M84 again

Dear Alan,

The paper by Leahy, Jagers & Pooley (A&A 156,234 (1986)) had, indeed, emerged. We should probably refer to it. They claim that a large fraction of the RM is external, but that there is one region of the source where there are significant departures from PA proportional to wavelength² and a correlation between RM and D. I am not sure that I believe this, given the data they present: what do you think?

Cheers,
Robert

P.S. The paper is on 3C66B, in case you didn't know.

MAIL> reply

To: VAX3::LAING, BRIDLE
Subj: RE: M84 again

%MAIL-E-OPENOUT, error opening UMA3:[BRIDLE]MAIL_02A7.TMP; as output
-RMS-E-CRE, ACP file create failed
-SYSTEM-F-DRVERR, fatal drive error

Press RETURN to return to reading your mail

3C68.1 0.14 Jy in 0".4 beam.
3C67.5 0.067 Jy 0".4

highest vis — 0.75 Jy
0.53 Jy.

Alan,

Here's the M84 optical stuff.

- You have (1) an R image
(2) a 26606 NB image ($\Delta\lambda = 75\text{\AA}$)
(3) a ~~the~~ "NB-BB" image which is the 6606 image minus an appropriately scaled R image, where the ~~appropriately scaled~~ ^{empirically} scaling was determined as the scaling which ~~caused~~ caused the emission from the Galaxy envelope to be zero on the NB-BB image. The factor I used was 1.08. You may want to play with this (i.e. subtract the R image from the 6606 image using different scaling factors). Clearly the appropriate scale factor is highly subjective. I have usually (purposely) ~~erred~~ erred slightly on the side of over-subtraction. →
over

We observed standard stars thru the same
filters and so I will be able to tell you
the conversion from counts to flux ^(to within a factor of ~2) (as soon
as I talk to Tim (tomorrow?) and make sure
I'm doing the calibration correctly!

Happy
CCDing,


steph

P.S. Plate Scale is

$$0.60''/\text{pixel} \pm .01''/\text{pixel}$$

and the fitted peak of the
optical nucleus of M82
is at pixel location

$$205.5 + 205.7 \quad 205.53, 205.73$$

P.P.S. I've also given you ^{i.e. +7.53 + 7.73} contour
plots of the CCD images as a
preview  _(i.e. -4.518 | + 4.638)

ROYAL GREENWICH OBSERVATORY

Herstmonceux Castle, Hailsham, East Sussex, BN27 1RP

Telex 87451

Telephone and Telefax (0323) 833171

National Radio Astronomy Observatory,
Edgemont Road, Charlottesville,
VA 22903-2475, U.S.A.

Your reference:

Our reference:

Date: May 22nd 1986

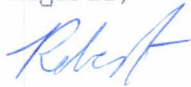
Dear Alan,

I took the opportunity to put the M84 paper through our internal refereeing system (a fancy term for getting Jasper Wall to read it). He had some useful comments on style, the most important of which was that the Introduction did not introduce the subject. I have written a new first paragraph to go before the current sketch of M84 (enclosed). I have incorporated a number of his minor corrections. Jasper also thought that there were too many Figures (!), and in particular that only one of Figures 4 and 5 was necessary. He preferred the grey-scale and thought that Mon. Not. was capable of reproducing screened plots quite satisfactorily. I seem to recall that your opinion poll was strongly in favour of contours. I am not very fussy about this, and would only insist on the grey-scale out of Figures 3-5. We could also mark the loci of the slices on Figure 5 and therefore remove Fig 6(d).

What do you think about the enclosed revisions? Did you ever get the Hansen *et al.* data in a satisfactory form?

I hope that we can finish this off at Aspen. I look forward to seeing you there; meanwhile I shall have a frantic fortnight clearing my La Palma desk, making sure my house is in a fit state to rent and getting all my maps onto tape at 6250 bpi (we have a high-density drive, at last).

Regards,



Robert Laing

National Radio Astronomy Observatory

EDGEMONT ROAD, CHARLOTTESVILLE
VIRGINIA 22903-2475, U.S.A.

Dr. A.H.BRIDLE
tel. [804] 296-0375 TWX 910-997-0174

February 10, 1986

Dr. R.A.Laing,
Royal Greenwich Observatory,
Herstmonceux Castle,
Hailsham,
E. Sussex BN27 1RP,
U.K.

Dear Robert,

Thanks for your two letters re M84 and AB369.

I have now read the Danish tape; the problem turned out to be in IMLOD's handling of 32-bit data and it took a while to turn Eric's attention to it. Now I can read the images, but was dismayed to discover that they were written with no co-ordinate information. I shall have to try to invent some to get it right. In the short term this displaced M84 behind some other jobs I am doing but I will be back to it very soon. I will remake the 11cm E vector plots and get it all in to Graphics as my next job.

AB369 is now scheduled for 24 hrs on March 29th. My census of the project participants (Ian Browne, Jack Burns, Dave Hough, Colin Lonsdale and you have replied) indicates general dissatisfaction with the referees but also general agreement that putting some results under their noses will be the most effective response. I will circulate a second letter to the mob about source selection very soon.

I do plan to attend Aspen; Dave has fingered me to organise a session on the internal structure of large scale jets. Will you definitely be there ?

Our Convex C-1 is running AIPS, though the interface to the IIS was delivered with hardware problems and we have no display yet. EtherNet to the VAX is working and we are pressing to get our Cray, Convex and VAX versions of AIPS all the same so we can update all of our machines from the same master code daily. Then we will have a much saner environment in which to address code development.

The Convex is indeed very fast; U.Va. is not yet hooked up to its share of it, so we may see some slowdown when they are all aboard. Nevertheless, we are enjoying the pleasures of a really fast system at present (about a factor of three over the VAX). I hope it may still be in such good shape this summer. We would, of course, be happy to see you here again on your way East or West; you would be more than welcome to stay Chez Bridle while exercising the vector pipes on the Convex (one can even do that Chez B. thanks to my new galloping PC and the wonders of modern communications).

With best wishes,

Alan

ROYAL GREENWICH OBSERVATORY

Herstmonceux Castle, Hailsham, East Sussex, BN27 1RP

Telex 87451

Telephone and Telefax (0323) 833171

Your reference:

Our reference:

Date: *January 6th 1986*

Dear Alan,

Here are some more detailed thoughts on the M84 RM paper.

*p.2, l9 replace "3.86 arcsec by 16.25 arcsec" by "this".
Should we mention MEM deconvolution (as used for deep maps), or will this just confuse? It might be worth putting noise levels in a table, to avoid cluttering up the text. The units are $\mu\text{Jy}/\text{clean beam}$, as in the captions.*

Sec. 3.1, l7 delete "also"

p2, l3 from end insert comma after $40\mu\text{Jy}$.

Refer to Wardle & Kronberg (1974) for Rician bias?

p3, l6 delete "Both"

l13 the low degree of polarization at the edges of the jets is due to beam smearing (as we know from higher-resolution maps)

Sec. 4.1, para2 could we delete "and in what follows...." as we say this again (after justifying it) in the next para.?

para3, l3 double full stop

Sec. 4.2, para1 after "is consistent with the DX data", add "We justify this assumption in Section 4.3."

Sec. 4.3 "Regrettably, ..." We are vulnerable to the criticism that we made no attempt to drive at a 2nd L-band frequency. $35 \text{ rad m}^{-2} \rightarrow \Delta x = 39^\circ$ between 17 + 22 cm, after all. I suggest (regrettably) that we omit this sentence.

p5, l4 from end "...intensity, or where...."

p6, l4 Δx

Sec. 6 Leahy's limit on high latitude RM variations on the scale of 1984 is $< 4 \text{ rad m}^{-2}$, set by errors in the data + intrinsic variations.

Doubtless the actual number is much less.

Should we refer to Burn (1966) +/or my (our) GB Workshop paper re λ^2 laws, foreground rotation, etc.?

p07, para2, l3 replace "Faraday screen" by "thermal matter" - it's not a screen. Could we change the emphasis to make sure that people realise that it is the absence of a correlation, not the lack of depolarization at the level of $D \approx 0.85$, that is desired (+ important) I suggest: "If thermal matter were uniformly mixed with the synchrotron-emitting material, then we would expect a correlation between the variations of RM and D , simple slab models predicting that the depolarization $D_{4.9}^{1.4} \approx 0.85$ in the regions of extreme Faraday rotation. Such a ~~varia~~ relation is not desired (Figures 5 and 10)."

$D_{4.9}^{1.4}$ should be defined in the first line of (4).

Are there any significant areas where depolarization is so strong that we should quote limits (ie where the maps are blanked at 1.4 GHz)?

p9, l7 scales

p9, l4 from end: delete "to"

p10 Separate heading "Acknowledgements"

Add thanks to: "the staff of the Cambridge 5-km telescope for supplying the 2.7 GHz data and the Danes (.....) for providing their CCD images in FITS format."

Refs. Lang (1987) reference is OK
Shaver et al IAU
Leahy (1985) is University of Cambridge

Captions

Fig 2 $x.x = 0.72$, I think
last sentence "... to maximise overlap"?

Fig 3 delete "the face of" ?
"positive Δx ", not " Δx positive"?

Fig 10 It is difficult to read the areas of
heavy segmentation. Is there any way
to distinguish these from background /
show limits?

I enclose a printout of data for x vs x^2 plots.
The format should be obvious. I hope that you
can read the Danish tape.

I hope that we are almost there now. I will get
the current draft internally refereed (i.e. I will persuade
Jasper to skin through it) in parallel with final
polishing.

Happy New year,

Robert

National Radio Astronomy Observatory

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Dr. A.H.BRIDLE
tel. [804] 296-0375 TWX 910-997-0174

January 3, 1986

Dr. R.A.Laing,
Royal Greenwich Observatory,
Herstmonceux Castle,
Hailsham,
E. Sussex BN27 1RP,
U.K.

Dear Robert,

Happy New Year !

I got the tape with the optical data from Hansen, but we are having trouble reading it. Eric may be able to rescue the situation, but there seems to be a problem with their FITS format.

Here are plots of depolarization versus rotation measure on 2 arcsec and 4 arcsec cells at 3.86 arcsec resolution. There is indeed no obvious correlation. I have done the lobes separately for clarity. Getting errors attached to each point would take much longer. This is a developmental task now called IMVIM (image vs. image) in the 15JUL86 version of AIPS. I'll exercise it some more with other data sets. Eric has gotten interested in this problem now so there may be more progress (it came out as a generally useful function requested by several different types of user at the AIPS Workshop).

Our Convex C-1 is now here and we have 15JUL85 AIPS running on it (the last debugged UNIX version), but no display. We hope to have the IIS from the ModComp hung on it by the end of January. Then it will be about 3 times faster than the VAX plus AP (we hope). Any chance this might lure you to Charlottesville for a visit ?

With best wishes,

Alan

STARLINK NDA 212700/0001 10DELOJ 07-41707-07 E01
VIA: 9109970174

TO: 62215490

NRAOCHARLVA UD

NRAOCHARLVA UD

87451 RGOBSY G
NRAOCHARLVA UD

1626 85-12-13 14:33

TO : DR A M BRIDLE
NRAO
CHARLOTTESVILLE

FROM : ROBERT LAING
RGO

DEAR ALAN

JUST A QUICK RESPONSE TO YOUR LETTER AND DRAFT. I HAVE JUST GOT BACK FROM JODRELL AND HAVE NOT HAD TIME FOR A DETAILED LOOK YET.

I THINK THAT THE I CALIBRATION AT 2.7 GHZ MAY BE A BIT OFF. I GOT 3.57 JY IN CLEV.N70 COMPARED WITH 3.95 +/- 0.06 JY (SINGLE DISH). WOULD SCALING I BY 1.1 HELP? I HAVE ASKED GUY POOLEY TO CHECK. IT IS NOT QUITE AS SIMPLE AS THAT, BECAUSE THE FEEDS GIVE I-Q AND U-Q.

IT OCCURRED TO ME THAT THERE MIGHT BE A SLIGHT SHIFT BETWEEN 6 AND 20CM VM MAPS, BECAUSE I DID NOT ALIGN THE CORES. CGEOM SHOULD COPE, IF THERE IS.

NEN FIG 5 IS FINE. NO EMISSION-LINE DATA YET: I WILL FOLLOW UP. X-RAY PICTURES ARE EXCELLENT.

NO PROBLEMS ABOUT THE CYG A SLIDE - I HAD ALREADY AGREED TO THE SEPARATE IMAGES.

→
Ag Weems
←

MY STARLINK ADDRESS IS RGVAD::RL. THE RGO 11/780 IS KNOWN AS GXVS ON JANET. THE VLA (PAT MOORE) HAS DETAILS. I CANNOT RETURN MAIL YET AS SERC HAS NOT MAINTAINED ITS ARPANET SUBSCRIPTION.

MORE ON THE DRAFT FOLLOWS SOON

REGARD, ROBERT

END++
NRAOCHARLVA UD

87451 RGOBSY G

National Radio Astronomy Observatory

EDGEMONT ROAD, CHARLOTTESVILLE
VIRGINIA 22903-2475, U.S.A.

Dr. A.H. BRIDLE
tel. [804] 296-0375 TWX 910-997-0174

January 2, 1986

Dr. Leif Hansen
University Observatory
Øster Voldgarde 3
DK-1350 Copenhagen K
DENMARK

Dear Dr. Hansen,

Thank you for sending me the FITS tape with your data for M84. I will send you the superposition of the radio and emission line data as soon as possible. Robert Laing and I are very grateful to you for making your data available to us in this form.

Yours sincerely,



Alan H. Bridle

December 19, 1985.

Dr. A.H. Bridle
NRAO
Edgemont Road
Charlottesville
VA 22903-2475
U.S.A.

Dear Dr. Bridle,

Recently we got a telex from Dr. Laing telling that you are working on VLA observations of M84, and that you wish to compare with our optical images. I have now converted the corresponding two IHAP files to FITS format. The tape has been mailed under separate cover. Together with this letter I enclose a copy of the IHAP manual describing the format. If you encounter any problems please contact Dr. Nørgaard-Nielsen. I leave for La Silla to the end of January.

We look forward to hear about you results with great interest.

Sincerely,


Leif Hansen

cc: Dr. R. Laing

National Radio Astronomy Observatory

EDGEMONT ROAD, CHARLOTTESVILLE
VIRGINIA 22903-2475, U.S.A.

Dr. A.H.BRIDLE
tel. [804] 296-0375 TWX 910-997-0174

November 24, 1985

Dr. R.A.Laing,
Royal Greenwich Observatory,
Herstmonceux Castle,
Hailsham,
E. Sussex BN27 1RP,
U.K.

Dear Robert,

Thanks for the tape with all your Jodrell reductions. As I am just about to go to the VLA for three weeks, I am sending you what I have done up until now. It's not polished, but the text and some diagrams have changed enough from the last draft that it's time you saw it all and commented on it again. I will return on December 12th and be back in Charlottesville at least until the end of the year.

The VM map at 20cm was a big improvement and the POLCO/VM estimates of polarization and depolarization should be much more reliable than the previous ones. I had played with POLCO and the blanking before you sent me your work, and agree that at the 4σ level the POLCO has little real impact on our statements, but the highly depolarized regions need to be POLCO'd anyway. The VM improvement at 20cm is probably the most significant. I have used the POLCO/VM maps for the paper now.

I am puzzled that the observed polarized fluxes at 11cm seem to be higher than those predicted by applying the 6cm degree of polarization to the 11cm I map. Do you understand why this is? Otherwise the prediction seems to have gone well and I am happy with the results.

Some comments on the Figures:

I am proposing a revised version of Figure 5, to show the I contours. I found this very helpful. I enclose a "top copy" of the new Figure with the paper, as well as a reduced scale copy attached to the paper. Because of the change, Figure 5 no longer can no longer show the profile tracks, so I suggest that these become their own panel in Figure 6d.

I will add the items to the pictures that have to be hand-drawn (errors on the RM profiles in Figure 6, and their tracks in Figure 7d).

I realised just after running off the plots for Figure 7 (and there's not time for me to change them before I go away) that the NS pixel elongation in the maps you sent me has interacted with the PCNTR plots to stretch all the vectors NS. This means that the vectors as drawn here do not reflect the P lengths correctly (the NS components are all stretched as in the pixel scale). I will fix Figure 7 when I come back.

I include a "top copy" of the depolarisation grey-scale for Figure 10, as well as the reduced-scale copy attached to the paper. The RM versus D plots cannot be done automatically using existing AIPS software, I have concluded. I have got Eric interested in coding something to do this, but am not sure of the timescale except that it will not be immediate. I may resort to a hand-done clooge as a result. In the meanwhile I have deleted them from the paper plan. Can we get along without them given what is now in the text ?

I propose that we show the X-ray data in Figure 11, as this comparison is interesting and I have the X-ray data smoothed and regridded in AIPS now that Bill Forman and Fred Seward sent me the tape. An X-ray spur runs near the region of "anomalous" behavior at the east side of the north lobe. I have not mentioned this in the text, yet. Bill Forman said to go ahead and use his data. I enclose a "top copy" of the new Figure 11 panels, as well as the reduced copy attached to the paper.

I also enclose some top-copy plots of some other images I found of interest, but which I don't think we should include. These include superpositions of the 6cm I contours on grey scales of the RM gradients, and also greyscales of the 6cm degree of polarization and of the RM and I gradients (Sobel filters applied to the RM and I maps using the AIPS task NINER). These may be useful for you to look at in parallel to reading the paper, but I am not suggesting them as Figures. I also enclose some other miscellaneous plots of the smoothed X-ray data, with and without the radio data superposed.

I am sending you under separate cover the FITS tape of the X-ray data that I got from Fred Seward and Bill Forman. I have added the smoothed X-ray images (regridded to the same scales as our VLA maps) to this tape. Note that the AIPS Gaussian smoothing have to be specified at twice the beamsize that was attached to your CFA plots in order to get comparable images (do CFA use HWHM as the specified parameter ?).

Have you managed to obtain a FITS image of the optical emission line data ? That would be interesting to superpose on the depolarization data, though the latter is still noisy.

Miscellaneous (more or less) unrelated matters:

I enclose a 35mm slide of the Cygnus A montage that was used as the cover of the Green Bank P.E.T.E.R.S. workshop. I was pressed into making a slide of this after various NRAO bigwigs got enthusiastic about the Workshop cover - I hope you don't disapprove of this use of your data. There will also be a copy in Peggy Weems' NRAO collection henceforth. (She already has the separate images in there, from Rick Perley).

Can you be reached by Vaxmail through Starlink ? I suspect that there is a Starlink node that connects BitNet to JANET in London; do you happen to know about that ? We are connected to BitNet through CalTech (the VLBA connection).

We have our new Convex C-1 arriving in mid-December, so CVAX will be being moved as soon as I get back. There may therefore be a bit of a delay before I can get the final version of Figure 7 done.

Hope this is getting closer to a final form, though there's still some way to go.

With best wishes,

Alan

National Radio Astronomy Observatory

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VIRGINIA 22903-2475, U.S.A.

Dr. A.H.BRIDLE
tel. [804] 296-0375 TWX 910-997-0174

November 21, 1985

Dr. W. Forman
High Energy Astrophysics Division
Smithsonian Astrophysical Observatory
60 Garden Street
Cambridge
MA 02138

Dear Dr. Forman,

I enclose superpositions I have made of two different smoothings of your HRI data on M84 over a grey scale representation of the VLA 6cm image of the radio source. I convolved the HRI data with Gaussians of 8 arcsec and 16 arcsec FWHM, and regrided the convolved maps to the pixel geometry of the VLA image for this purpose.

The anticorrelation between the 8-arcsec smoothed image and the radio data is very striking, and strongly suggests that the X-ray gas has played a role in collimation of the radio source. The C-symmetry of the bright radio jet also reinforces the notion that the system is subject to sweeping from a direction slightly north of west.

The other superpositions show the X-ray contours on a grey-scale representation of the Faraday rotation measure distribution deduced from the radio data. The scale of the X-ray source and of the Faraday rotation fluctuations are clearly comparable, but the largest deviations from the mean rotation measure occur just north and south of the bright X-ray "front" on the western side of the source at 8 arcsec smoothing.

I will send you a draft of our paper dealing with the radio polarimetry as soon as possible. In the meanwhile, my thanks again for providing me with the Einstein images so promptly.

Yours sincerely,

Alan H. Bridle

National Radio Astronomy Observatory

EDGEMONT ROAD, CHARLOTTESVILLE
VIRGINIA 22903-2475, U.S.A.

Dr. A.H.BRIDLE
tel. [804] 296-0375 TWX 910-997-0174

November 19, 1985

Dr. Fred Seward
Harvard-Smithsonian Astrophysical Observatory
60 Garden Street
Cambridge
MA 02138

Dear Dr. Seward,

Thank you for sending me the FITS format tape containing the HRI data on M84, Einstein sequence 4320. This will be most helpful to Robert Laing and myself for comparisons with the VLA polarization data, and I thank both you and Dr. Forman for your very prompt response to my request for it.

Yours sincerely,

Alan H. Bridle

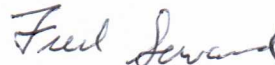
November 14, 1985

Dr. Alan Bridle
NRAO
Edgemont Road
Charlottesville, VA 22901

Dear Dr. Bridle,

Thank you for your recent request for a FITS format tape of M84, Einstein sequence 4320. Enclosed is a mag tape, written at 1600 bpi, using the FITS format for this sequence. The array is 512*512 in pixel dimensions, has a resolution of 2" /pixel (4*4 array elements/pixel) and is centered at the center of the field (2047*2047 in pixels). We hope this satisfies your request for data at this time. If you require further assistance, please contact us.

Sincerely,



Fred Seward/Sherene Aram

Center for Astrophysics

60 Garden Street
Cambridge, Massachusetts 02138

Harvard College Observatory
Smithsonian Astrophysical Observatory

November 7, 1985

Dr. Alan H. Bridle
National Radio Astronomy Observatory
Edgemont Road
Charlottesville, Virginia 22903-2475

Dear Dr. Bridle:

I have asked Dr. Fred Seward, who coordinates requests for Einstein data, to provide you with a FITS format tape (1600 BPI) of M84. The tape will contain a 512 X 512 array of the image with 2 arc-second size pixels (somewhat smaller than the HRI resolution). From this file you should be able to produce the maps you require. If you need additional information, or you would like to have some processing done here, please let me know.

Several years ago, Bob Laing visited SAO, and we looked at the then existing radio and X-ray maps of M84. During his visit we generated a variety of X-ray contour plots, and we exchanged X-ray and radio maps. At that time we planned to write two separate papers on this galaxy (on a time scale of about one year), emphasizing our own particular points of view and areas of interest, and we agreed to use the complimentary wavelength information if appropriate. Since so much time has elapsed, I think that you should feel free to use the X-ray data as you wish. We still plan to write a paper emphasizing the X-ray observations, and we will send you a draft as soon as it is ready.

Enclosed are copies of two maps of M84 superimposed on the optical photographs which were made during Bob Laing's visit. The "smoother" contour plot shows what we have interpreted as motion to the west, through the intracluster gas. This seems consistent with the northern portion of the radio emission, but not the southern half. The two plots were made with 4" and 8" Gaussian smoothings. The point source to the south-east is a QSO, and has been observed optically by Malcolm Smith and Bev Oke.

Again, let me know if you need any additional information. I look forward to reading your paper.

Sincerely,



William Forman

WF/pe
Encl.

Jodrell Bank,
21/10/85

Dear Alan,

This tape may not have quite all you need, but includes all the VLA processing. I have:

1. Cleaned up the 11cm data and done the planimetric comparison 6-11-21cm, using RM.
2. Made VM maps at 20 + 6 cm (3"86 resolution)
3. Derived better 2D planimetric & deplanimetric & spectral index maps by using POLCO + VM.
4. Generated a "pseudo-11cm" map as we discussed. It turned out that COMB contains suitable machinery for turning a vector map into UBC.

The conclusions are as follows:

1. 3-frequency RM confirms our original values everywhere except in regions of deplanimetric &/or differential rotation across the large beam in Dec.
2. The pseudo-11cm map looks like the real one except in regions marked. The one I do not really understand is marked (2) - centred on 12 22 33, 10 30 00 or so.
3. The VM's clean maps are very similar at 6cm (one device or tape) - note that they appear to be very slightly shifted. Otherwise, there is a slight systematic difference at the outer ends of the "main axis".

The 20cm map cleaned up significantly using VM. There are obvious differences of a 'clean didn't work' sort between VM's clean.

You should be in a better position to analyse differences of superior TV.

4. x vs λ^2 looks tolerably linear. The easiest way to get decent numbers is to step through the cube.
5. I haven't come up with any clever scheme for plotting PM vs D. A procedure to write numbers into the message file is very unceremonial. I'll think about it.

I've put the "low-resolution" or on the tape. To follow:

- CCD images
- High-resolution VM's

Regards,

Robert

P.S. We should quote blanking levels. I have used (consistently)

- 30 on POLCO
- 50 on I

What were the levels for the PM maps? Could you check the history files?

P.P.S. Have you seen the recent preprint on 3C66B (Leahy, Jägers & Pooley?). They claim significant deviations from λ^2 rot² in one area of the source, together with a correlation between depolarization & PM. Lessons for us:

- I can see ^{little} correlation between PM & D in their plot, but a table of median values does show it. (probably)
- They do not consider emission-line gas & assume that ~~the~~ beam depolarization occurs only because of PM variations on scales \sim those they measure. It is certainly true that emission-line gas in the way will tend to cause deviations from λ^2 rotation and a correlation between PM and depolarization, ~~because of variations of depolarization~~ if it's not spread evenly across the source, and especially if the B-field direction varies rapidly with position.

National Radio Astronomy Observatory

EDGEMONT ROAD, CHARLOTTESVILLE
VIRGINIA 22903-2475, U.S.A.

Dr. A.H.BRIDLE
tel. [804] 296-0375 TWX 910-997-0174

October 18, 1985

Dr. W. Forman
High Energy Astrophysics Division
Smithsonian Astrophysical Observatory
60 Garden Street
Cambridge
MA 02138

Dear Dr. Forman,

I am working with Robert Laing of R.G.O. on writing up an account of an unusual rotation measure structure revealed by two-frequency VLA polarimetry of the radio source 3C272.1 associated with the Virgo cluster elliptical M84. As part of this work we are seeking to compare our radio polarimetry directly with other data indicating the presence of ionised gas in this galaxy – both the LINER-like optical emission line region and the extended soft X-ray source. There appear to be features in the radio rotation and depolarization data that may correlate with both of these.

We therefore noted with interest that you published an Einstein HRI image of the M84 X-ray source in the volume *Clusters and Groups of Galaxies* edited by Mardirossian *et al.* Would you be willing to make a FITS tape of this image available to us for overlay on the VLA radio data ? If so, I wonder if you could send me such a tape at either 1600 BPI or 6250 BPI density. I will be happy to provide the tape itself, or to return yours, if you would do this. I will also send you the results of the superposition as soon as possible thereafter. Any Einstein image of M84 at higher angular resolution than the published one would also be of interest to us.

Yours sincerely,

Alan H. Bridle

National Radio Astronomy Observatory

EDGEMONT ROAD, CHARLOTTESVILLE
VIRGINIA 22903-2475, U.S.A.

Dr. A.H.BRIDLE
tel. [804] 296-0375 TWX 910-997-0174

September 23, 1985

Dr. R.A.Laing,
Royal Greenwich Observatory,
Herstmonceux Castle,
Hailsham,
E. Sussex BN27 1RP,
U.K.

Dear Robert,

Thanks for your letter of September 6th. This may get to you before I do. I am not sure how my traveling will fit together once I am in England; it depends to some extent on my mother and on other family things. But I will hope to visit with you around Oct. 10/11/12. I'll telephone you the week before, once I have had a chance to assess the home situation. Mary will be with me until the 11th, when she will be going to Ireland for a quick visit with her family. We're thinking of making a short trip to Cambridge before that (Mary has only been there once) so will probably try to fit that in on the 9th, when you said you might be busy. Would it be possible for both of us to stay with you on the 10th, then just myself for another day or so while we plot science? I think we would stand to get more done if my visit is "unofficial", i.e. if I'm not giving a talk during it!

I will attempt to put a rough draft of an M84 paper together between now and when I leave, so we have something in detail to talk over. I am up to my neck in administering the AIPS group now (inherited that from Ed when he left) so don't have as much time for science as I would like. I'm sure the problem sounds familiar.

I'll be in touch by phone later next week.

With best wishes,

Alan

National Radio Astronomy Observatory

EDGEMONT ROAD, CHARLOTTESVILLE
VIRGINIA 22903-2475, U.S.A.

Dr. A.H.BRIDLE
tel. [804] 296-0375 TWX 910-997-0174

August 26, 1985

Dr. R.A.Laing,
Royal Greenwich Observatory,
Herstmonceux Castle,
Hailsham,
E. Sussex BN27 1RP,
U.K.

Dear Robert,

I will be in England from Oct 2 to Oct 14, mainly visiting my mother, who is ill. However, this could be a chance for us to get together to discuss various papers. Will you be around at that time ? Is there a date that would be more convenient than others for us to get together ?

I would like to give some priority now to the M84 rotation measure stuff in a short article as we discussed at the GB workshop. Did you receive the displays of the 3.9 arcsec 20/6 cm RM data for M84 ? Did you have a preference over which to use ? I will give them to graphics to turn into glossies before I leave, so please let me know your preference a.s.a.p. I hope all is going well at La Palma and that you are surviving the latest round of austerity moves o.k.

With best wishes,

Alan

National Radio Astronomy Observatory

EDGEMONT ROAD, CHARLOTTESVILLE
VIRGINIA 22903-2475, U.S.A.

Dr. A.H.BRIDLE
tel. [804] 296-0375 TWX 910-997-0174

January 15, 1985

Dr. R.A.Laing,
Royal Greenwich Observatory,
Herstmonceux Castle,
Hailsham,
E. Sussex BN27 1RP,
U.K.

Dear Robert,

Here are various displays of the 3.9 arcsec 20/6 cm RM data for M84, as food for thought re our impending 1989 publication of the RM gradient maps.

First, for reference, a screened greyscale of the RM map. Second, a similar grayscale with superposed contours at 10 rad.m^{-2} intervals from a map to which $+35 \text{ rad.m}^{-2}$ had been added everywhere (making all the contours solid). Third, the RM contours alone, unlabelled – a few silly ones have been excised by the “Cambridge CLEAN” technique. Fourth, the RM contours with a suggested labelling. I have shown this to various people here, who find it basically comprehensible. *I think we should make this display, or something like it, part of the paper.* Use the unlabelled contour plot (and the greyscales as a guide) to edit or relabel this diagram to your taste – I’ll make any changes to you require to my copy before giving it to NRAO graphics.

After that, the position angle plot, as reproduced in your (our) paper for the Workshop. I think we should also use this – it will start people thinking about this method of display, which we want them to face up to one day.

After *that*, a set of slice plots, for your perusal. I think they are self explanatory given the key drawn on the next greyscale. There is a small problem with blanked values; the slice software doesn’t understand blanking and treats blanked values as zeroes. This gives its spline algorithm a spastic fit. I have therefore chosen the slices to avoid blanked values as much as possible (strategem needed for reliability anyway), and have excised a few spastic fits with the aforementioned Cambridge CLEAN technique.

Contemplate the above and consider which displays, if any, you might wish to inflict on the great astronomical public.

We can submit the color slide, with just the MILLI???? units removed, to *Nature* and hope they might use it on their cover – we could also rephotograph it with the labels and RAD/M**2 units (tastefully inserted in the header by A.H.B. over the strangulated sobs of Eric Greisen the S.I. Standardiser) if you desire.

I hope you are alive and well. Send confirmation of this speculation if possible.

With best wishes,

A handwritten signature in black ink, appearing to be the name 'Alan' in a cursive style.

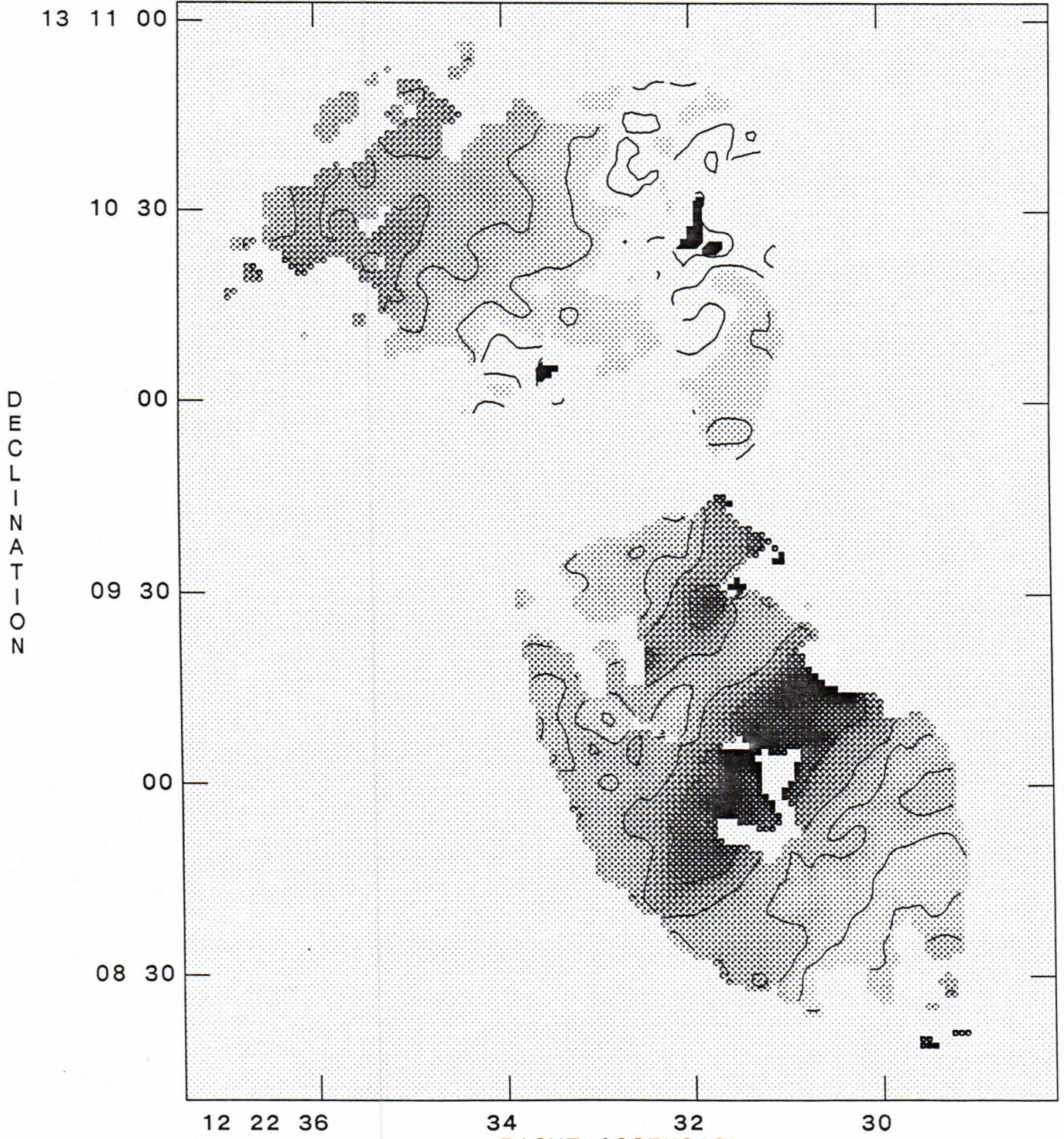
Alan

M84

PANG

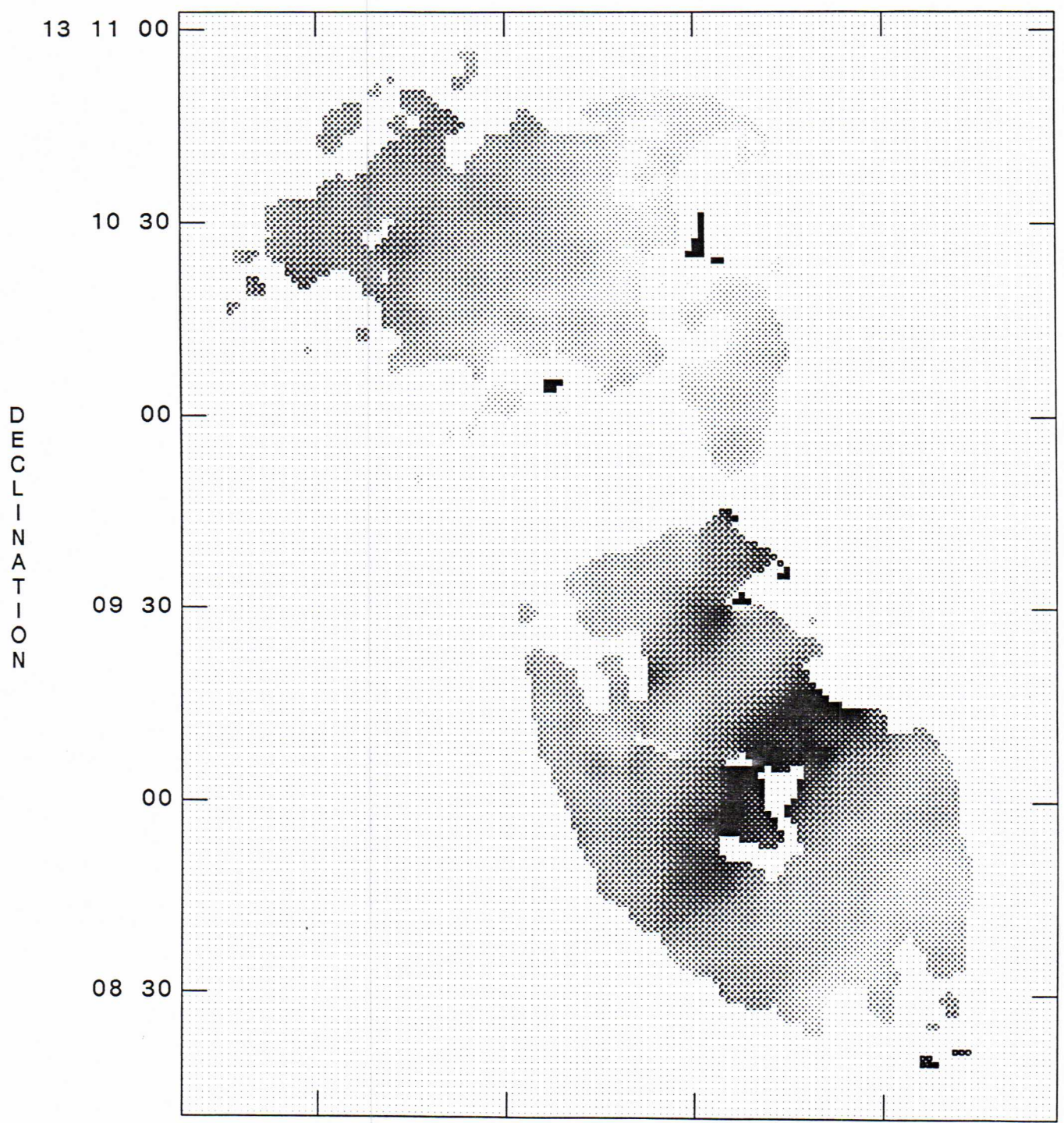
1413.000 MHZ

M84.RM+35.1



RIGHT ASCENSION
GREY SCALE FLUX RANGE= 1.0000E+01 5.9999E+01 RAD/M**2
PEAK CONTOUR FLUX = 7.2934E+01 RAD/M**2
LEVS = 1.0000E+00 * (0.000, 10.00, 20.00,
30.00, 40.00, 50.00, 60.00, 70.00)

M84 PANG 1413.000 MHZ M84.RM.4



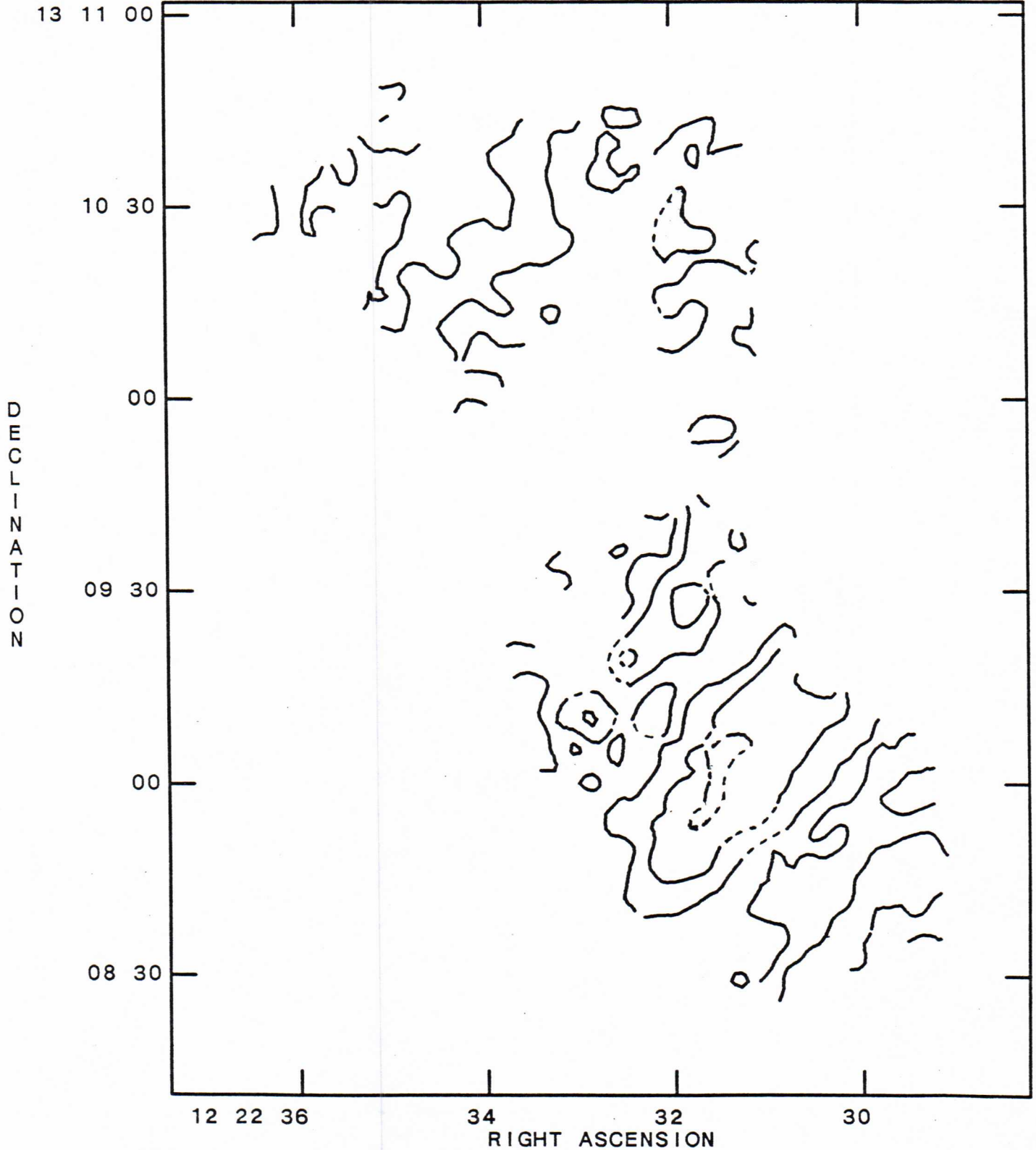
GREY SCALE FLUX RANGE= -2.5000E+01 2.4999E+01 RAD/M**2

M84

PANG

1413.000 MHZ

M84.RM+35.1



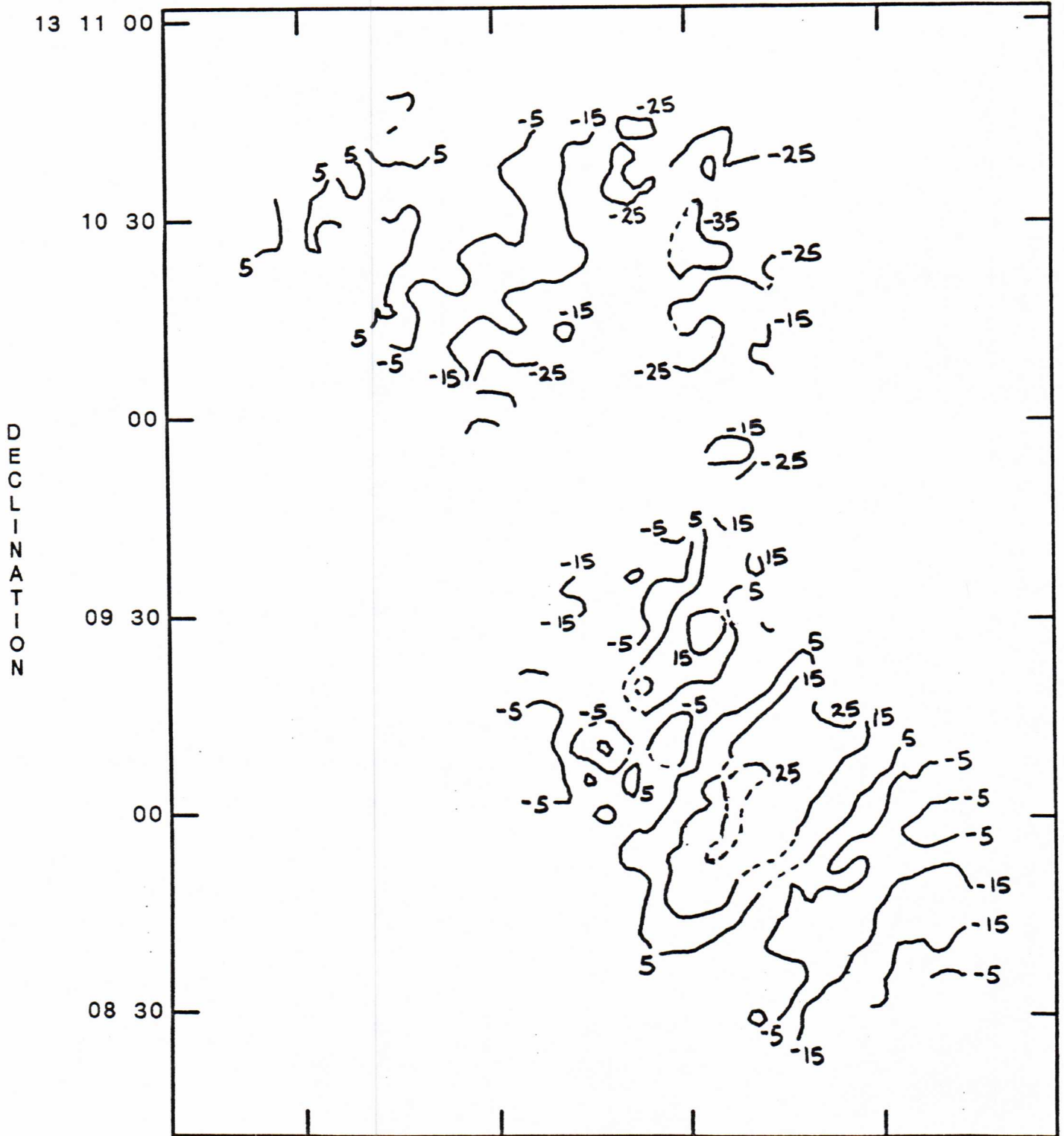
PEAK FLUX = 7.2934E+01 RAD/M**2
LEVS = 0.1000E+01 * (0.000, 10.00, 20.00,
30.00, 40.00, 50.00, 60.00, 70.00)

M84

PANG

1413.000 MHZ

M84.RM+35.1



12 22 36 34 32 30

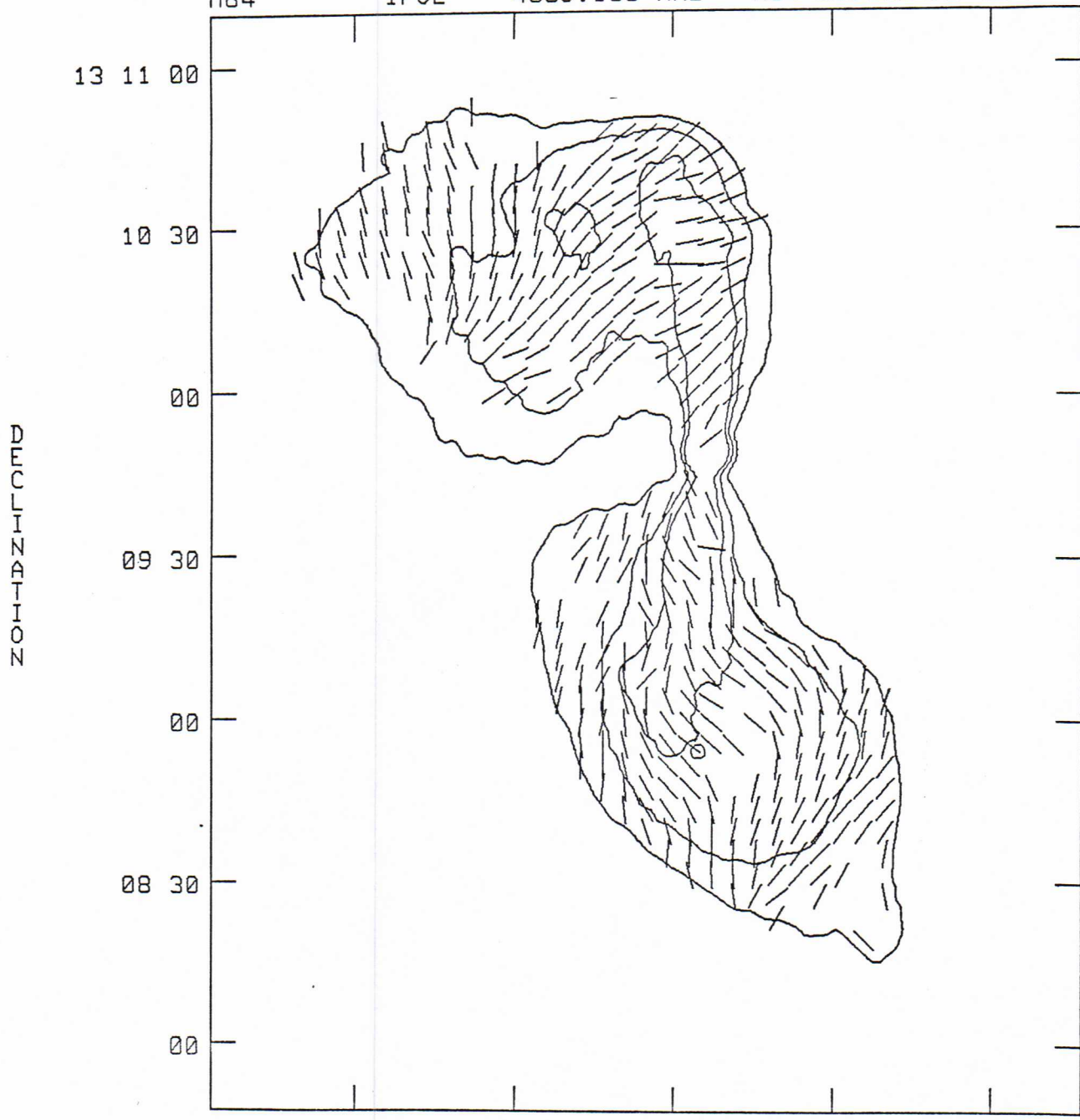
RIGHT ASCENSION

PEAK FLUX = 7.2934E+01 RAD/M**2

LEVS = 0.1000E+01 * (0.000, 10.00, 20.00,

30.00, 40.00, 50.00, 60.00, 70.00)

M84 IPOL 4885.100 MHZ M84 6C.ICLN.2 ΔPA



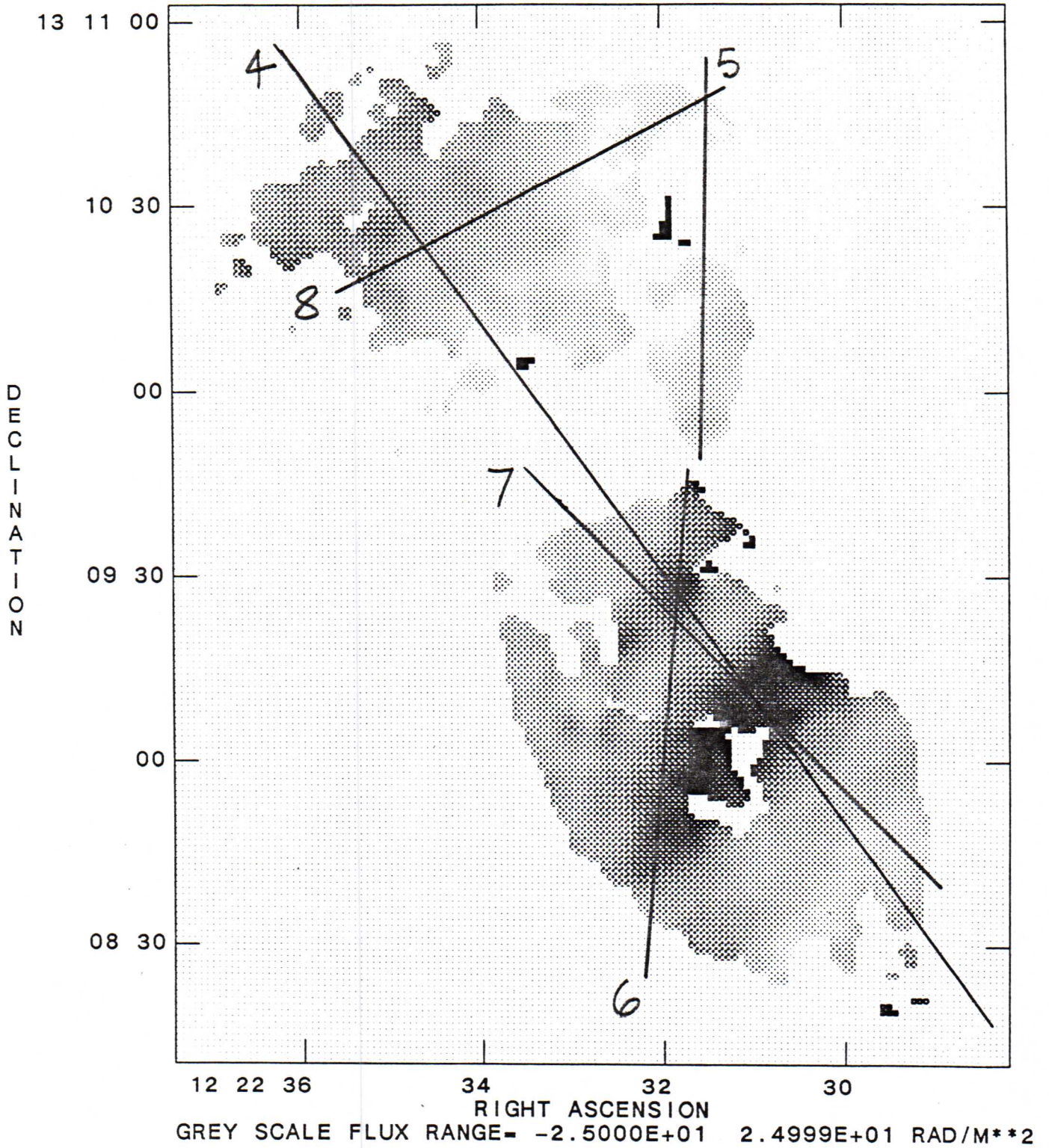
POL LINE 1 ARCSEC = 2.0000E-01 RATIO
PEAK FLUX = 0.1823E+00 JY/BEAM
LEVS = 0.2917E-03 * (3.0, 13.0, 30.0)

M84

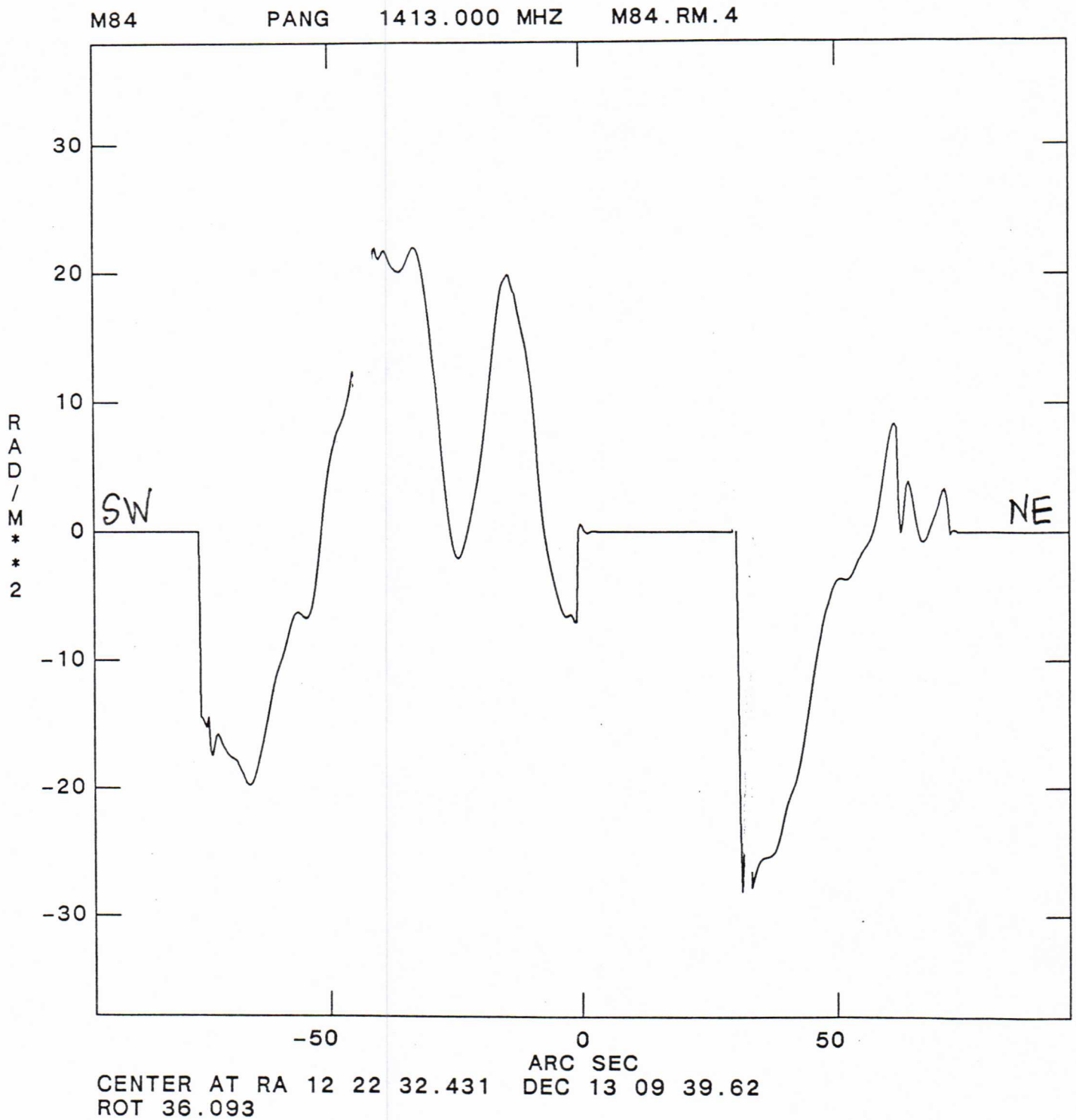
PANG

1413.000 MHZ

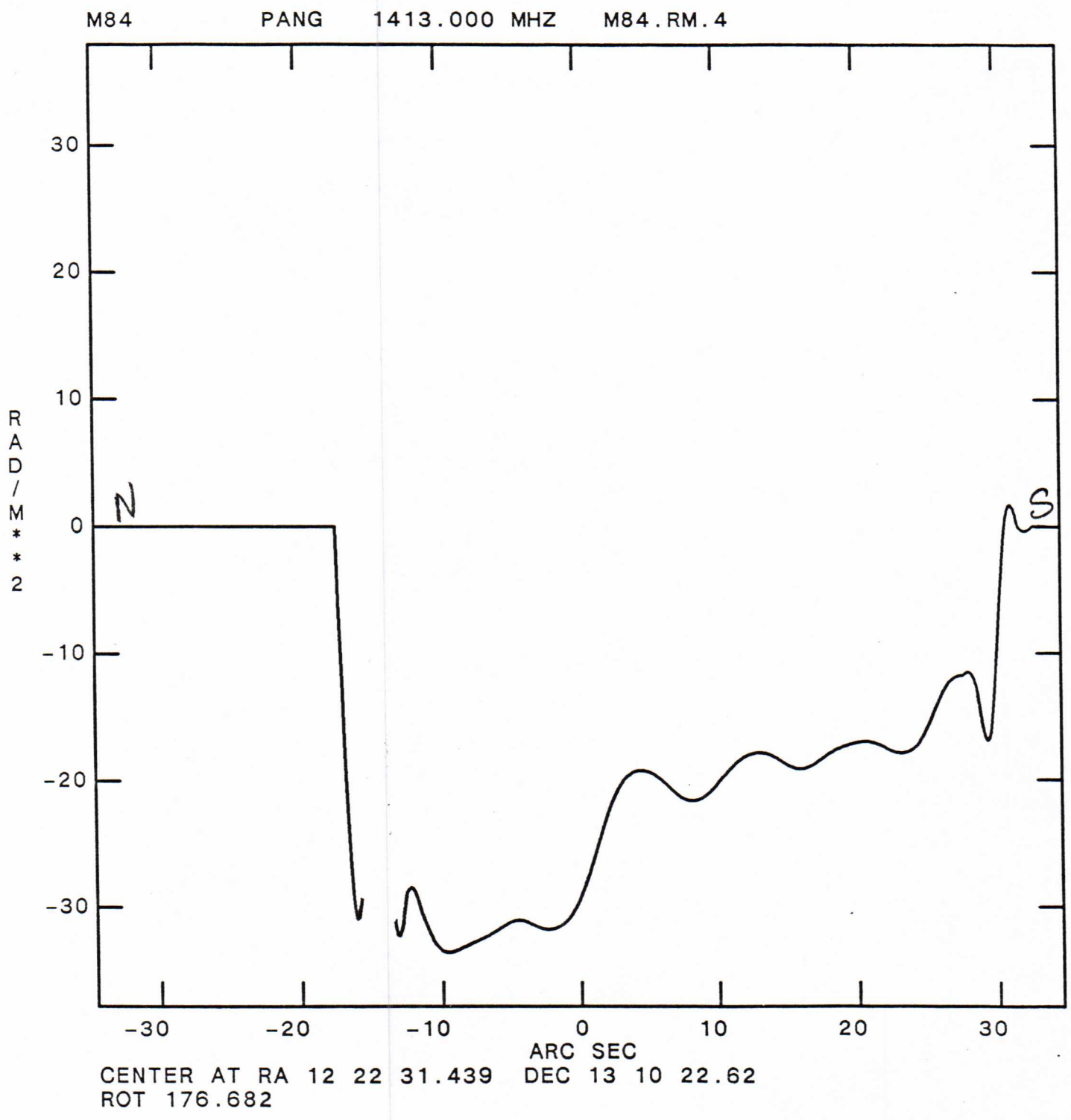
M84.RM.4



Slice 4 - "along" ext.
Source axis.



Slice 5 -
North jet

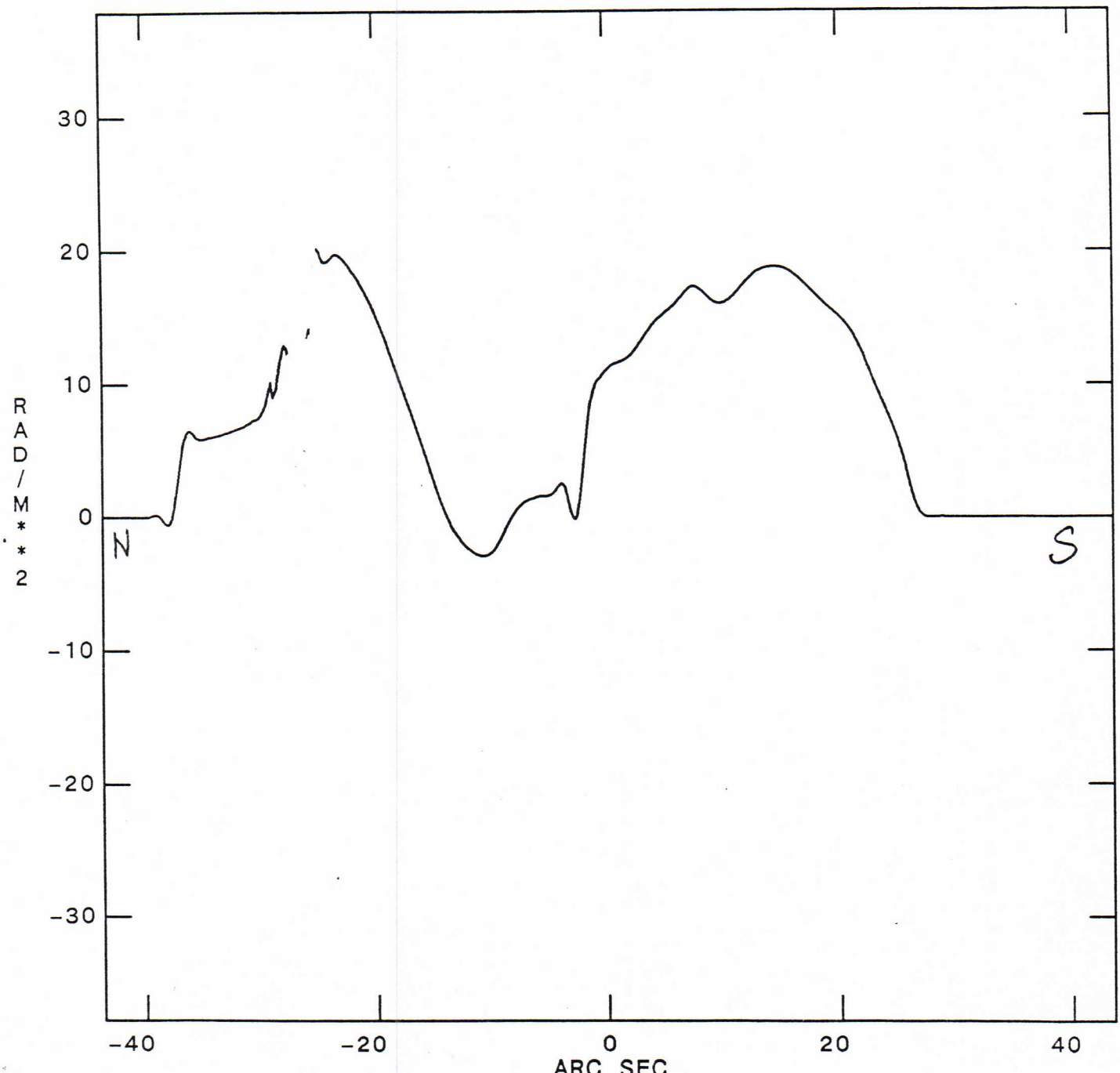


lobe
(5) North Jet

Core

Slice 6
South Jet

M84 PANG 1413.000 MHZ M84.RM.4

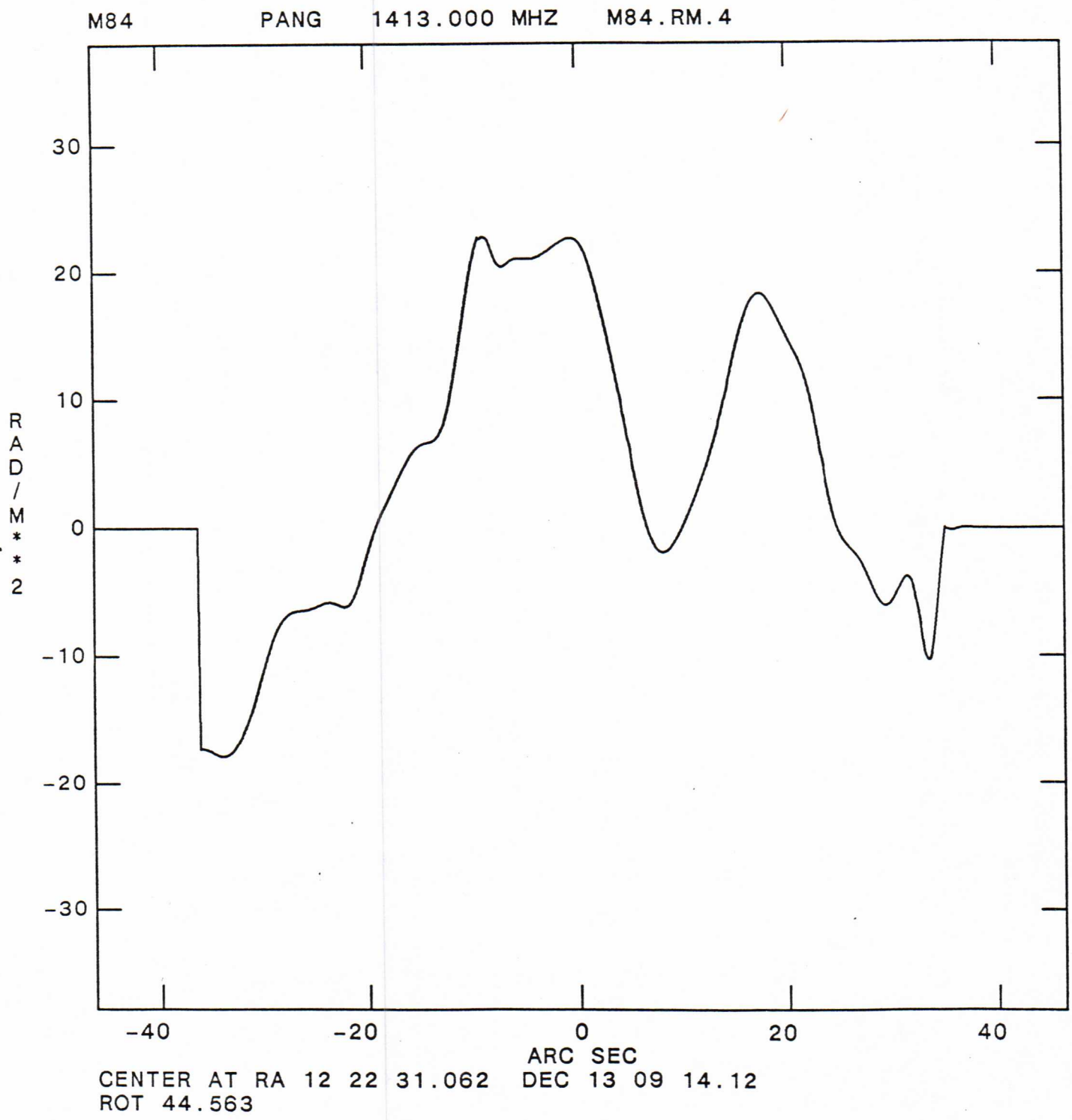


CENTER AT RA 12 22 31.986 DEC 13 09 04.62
ROT 176.055

Core

lobe

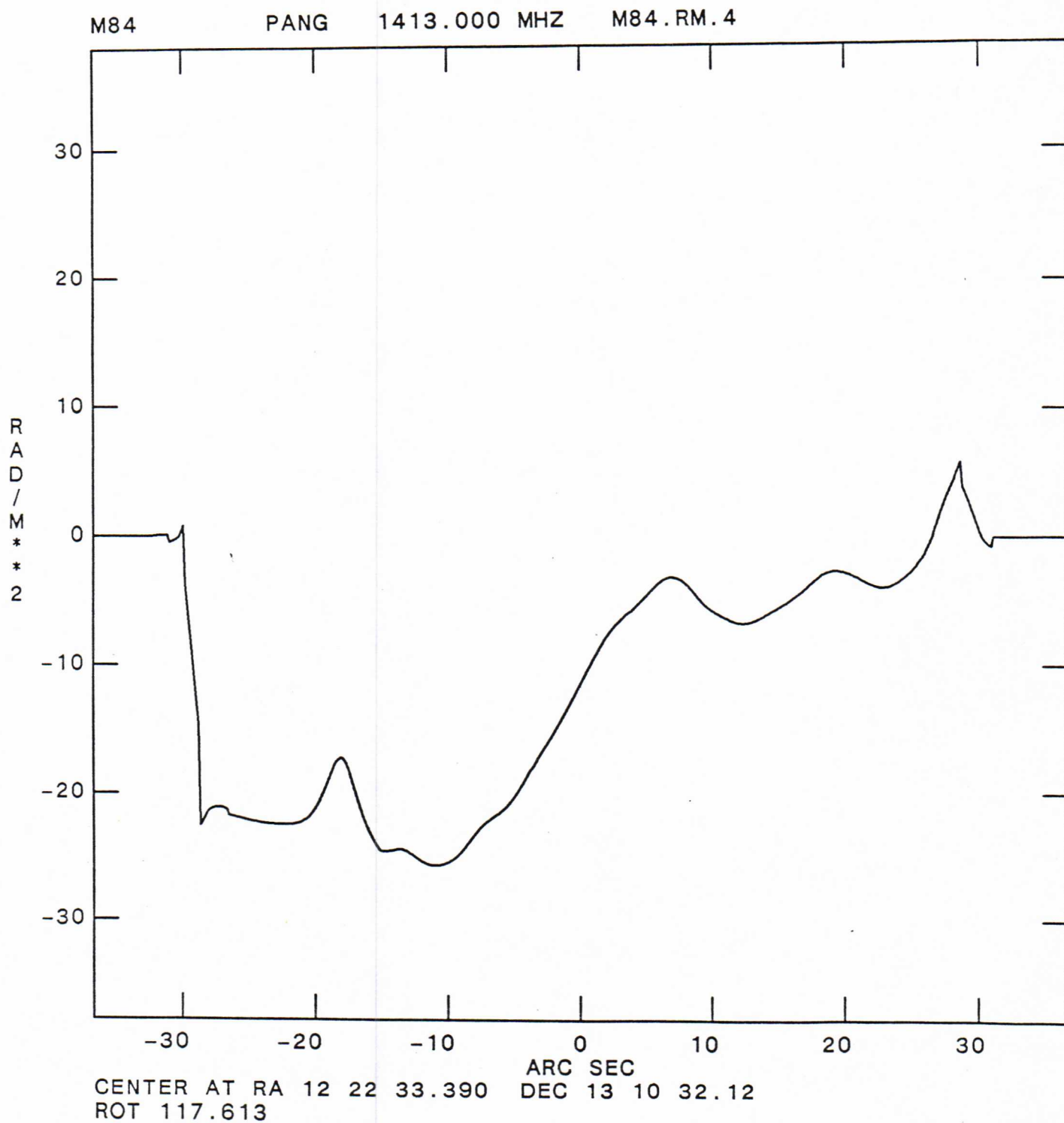
Slice 7 -
S lobe \perp to the
gradient in RM



SW
(7) (11.2 - 12.2) S lobe

NE

Slice 8 -
North lobe \perp to
RM gradient



ROYAL GREENWICH OBSERVATORY

Herstmonceux Castle, Hailsham, East Sussex, BN27 1RP

Telex 87451

Telephone and Telefax (0323) 833171

Your reference:

Our reference:

Date: September 6th 1985

Dear Alan,

Thanks for your letter. I was sorry to hear about your mother. Please let me know if there is any way I can help while you are in England (accommodation, transport, etc.)

I will be at RGO (or, at least, in Herstmonceux as I am due some time off) from September 21st - October 13th. Please visit whenever you can during that period. I would welcome a change from central software and both M84 status menu and 3C341/438 look possible. I warn you that running AFPS here is extremely frustrating (not consistent with the normal environment and no decent TV displays), but that should not matter.

Suggestions for M84 displays:

1. Can I continue
2. Will the "screened grayscale" reproduce? If so, it forms a good display except that: extraneous dots would need to be removed, a labelled wedge is needed and the "white" level is not legible.
3. The contours display looks reasonable to me. I would perhaps be slightly more ruthless in editing out fiddlinesses, but I cannot say that I put strongly about this.
4. The 'vector' display is of course in the workshop

* October 9th may not be possible.

proceedings: what was the response: is people find it
easy to understand? If not, then it is not worth including.
On the other hand, it may be the display of the future.
Could you consult public opinion? On balance, I think
that we should omit it if the grey-scale is printable.

5 Slides. Patch out numerous errors and typos, of
course. Mark positions on beam I contours. Also, it
would be worth adding some representative error bars or,
perhaps better, an indication in one corner of the
minimum S/N allowed by the masking.

To me, the most interesting shells would be
7 (S lobe, along maximum gradient), its equivalent
(at a slightly different PA) in the N, and (for
both lobes) one or two shells in orthogonal directions
- i.e. along iso-RM lines.

I am not sure that the cuts along the jet tell
us anything new, as we are relying on the 2D
displays to emphasize the point that the
RM structure is not correlated with the total
emission. 4 misses most of the interesting bits in
the N lobe and 8's PA is not related to the gradients.

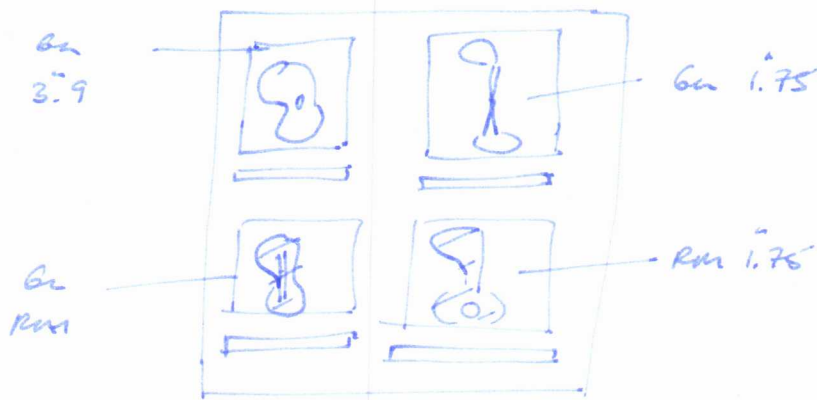
6. We should mark the "deprojected spot" on one of
the maps.

Have you looked at displays of the 1.75 maps?

I don't think that they add all that much,
except the reassurance that the results are not
restricted to one set of observations and that the
variations are well-sampled. Still, there is a fair
amount of signal. What do you think?

For a potential front cover, I would suggest panels with
beam I and RM side by side (or, given the journal format,
I alone the other). Rad/m² are essential, whatever EG says.

Would:



be a good idea, or am I making too much work? You have all the maps, I believe.

One recent result which may interest you: I have been looking at differential Faraday rotation / depolarization on opposite sides of sources with jets (high-luminosity quasars, mostly), both from my own data on a small number of jets and from Strom & Conroy's integrated component measurements. There appears to be a significant difference in the sense that the counter-jet side depolarizes faster. Various various models spring to mind, but I don't have any time to follow it up at the moment. It is possible that it would fit in (observationally that is) with a Jodrell thesis project, so Paddy Leahy & I may initiate some follow-up observations of a larger sample later in the year.

I shall be on La Palma from 9/9 - 20/9, in R60 (at least to the extent of picking up my post) thereafter. Please let me know what combination of transport, accommodation, food or other help I may provide. Your choices for the second of these are my spare bedroom* or Herbolmen Castle. We could no doubt extract some cash for Taylor for travelling expenses in return for an informal talk - but you may not want to bother.

I look forward to seeing you in October.

Regards to Mary,

* Probably more comfortable.

Robert



The University of Iowa

Department of Physics and Astronomy

Iowa City, Iowa 52242

319/353-4343

Feb. 01, 1983

Dear Alan,

Enclosed is a preliminary draft of a paper which I will probably submit to Ap.J. Letters (doubtless requiring elimination of some of the "boilerplate" in the present version). I would greatly appreciate it if you could tender comments.

I would also appreciate your advice on a related matter. Should I submit this paper with the present content, or should the M84 results be included in a longer version? My inclination is to submit this paper essentially as is, and prepare a second paper, with yourself and Robert as co-authors, describing the application of the results to M84. My reasons are twofold: (1) shorter papers tend to have more impact, and the M84 analysis would be a substantial undertaking in its own right, (2) a scientific alliance of Robert, yourself, and me would resemble a nocturnal congress of male cats, and a mutually agreeable result might be some time in coming.

Steve



The University of Iowa

Department of Physics and Astronomy

Iowa City, Iowa 52242

319/353-4343

Jan. 19, 1982

Dr. Alan Bridle
 National Radio Astronomy Observatory
 Edgemont Road
 Charlottesville, Virginia 22901

Dear Alan,

Thank you for copies of your excellent observations of M84. It is a good indication that one is washed up as an observer when he finds himself writing to colleagues for data decent enough to compare with theories!

The reason for my request is as follows. Largely at Robert's urging, I have reinvestigated the problem of synchrotron radiation transport in a random medium, this time assuming that the Laing model of the field being almost wholly random is correct. According to my analysis, the best place for studying such fluctuations is in areas where the mean polarization is very low. The reason for this curious result is that while the mean polarization is close to zero, the fluctuations in the polarization can be nearly as large as those in regions where the mean polarization is large. I will go

over the arguments for this during my anticipated trip to Charlottesville in March. At any rate, it turns out to be of considerable interest to study the fluctuations (or upper limits thereto) in Q and U in regions like the middle of the south lobe of M84.

The source M84 appears to be an outstanding candidate for such an analysis for two reasons. (1) It is one of the most compelling cases for Robert's sheared B field model. (2) Your 5 GHz observations with $1.5''$ resolution (map 'M84 GBC v3') has the best resolution across an extended object that I am aware of. Such high resolution is absolutely necessary if the type of analysis I have in mind is to have a prayer of succeeding.

The data I now request is in the form of three slices in the southern lobe of M84. I have indicated these as slices A, B, and C on the enclosed map. The slices should be made from the high resolution map, and should be plots of Q and U separately, not polarized intensity. The reason for this is that I need to calculate the variances and, if possible, the autocorrelation functions of Q and U independently. Since I intend to make measurements from these plots, they should be of good quality and scaled so as to facilitate measurement from them. I will also need values for the noise rms in Q and U .

I am most interested in analyzing the high-resolution 5 GHz map, but if it is convenient for you, I would also appreciate receiving the Q and U slices from the highest resolution 1.4 GHz map you have available.

I greatly appreciate your willingness to make your data available, and I am anxious to begin the analysis of these fluctuations.

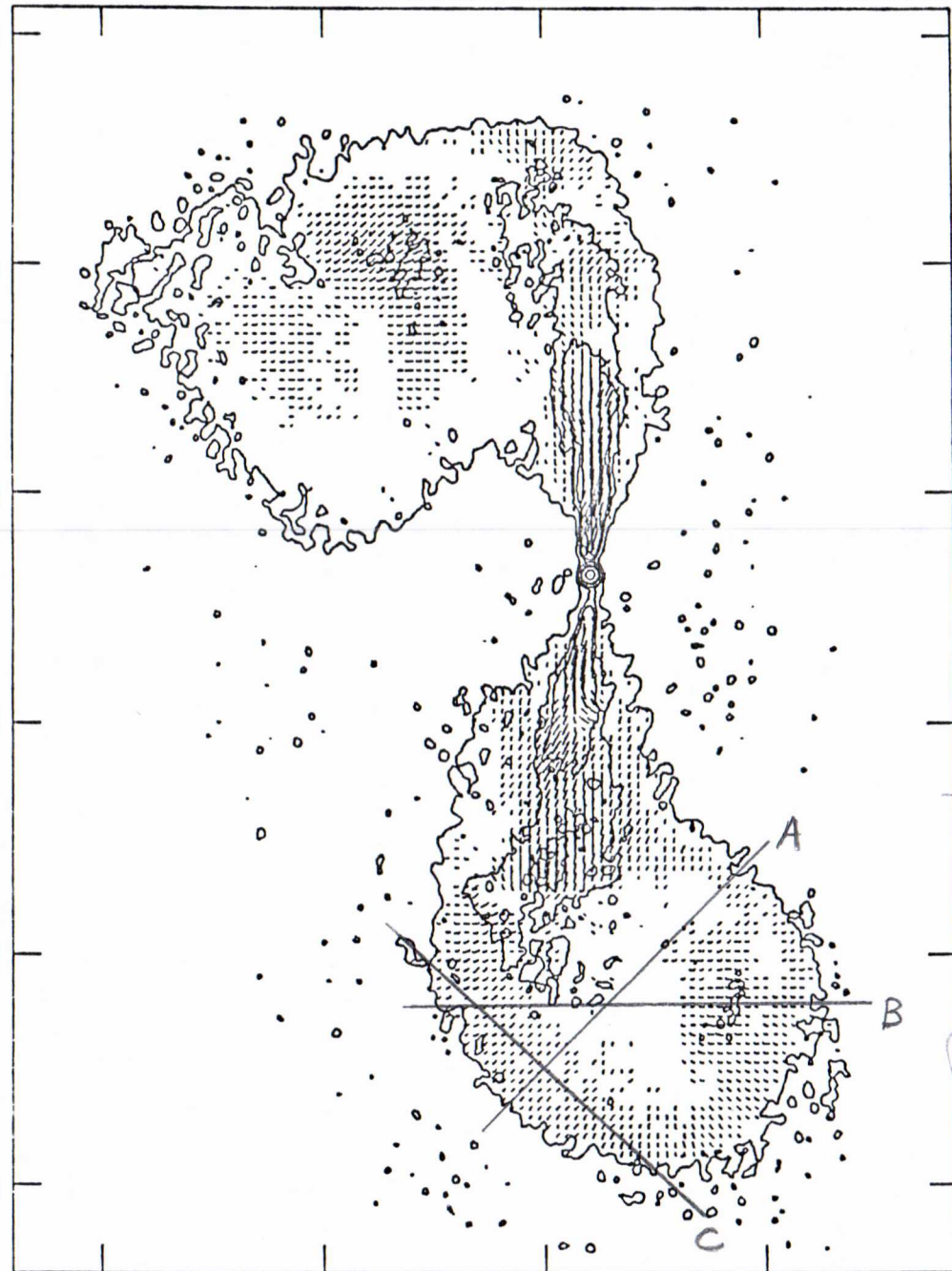
Regards,

Steve Spangler

E

13 11 00
 10 30
 00
 09 30
 00
 08 30

DECLINATION



"
 1.2
 $XIN = YIN = 4$ (1 μ beam)
 FACTOR = 20

How much signal in the center? If $< 1\%$ need small scale sizes, won't see fluctuations in the bright bit.

Please make Q and U slices along these lines

12 22 36 34 32 30
 RIGHT ASCENSION
 POL LINE 1 ARCSEC = 3.7904E-04 JY/BEAM
 PEAK FLUX = 1.8010E-01 JY/BEAM
 LEVS = 0.1801E-03 * (-1.0, 1.0, 5.0, 10.0, 50.0, 100.0, 500.0, 1000.0)