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Nov. 2, 1981

Dear Alan,

I have reached the point where I am reasonably satisfied with the status of the 3C166 data, at least to the extent where I am ready for some detailed input from you. I am enclosing two items of relevance to 3C166.

First of all, I enclose an abstract of a poster paper I intend to present at the Boulder AAS meeting. You will notice that you are listed as a coauthor. I apologize profusely for the fact that I was unable to clear it with you prior to submission. I did make a bona fide effort to contact you several times, but was unable to connect. I finally had to make a decision to forego submitting an abstract, or to send one in unilaterally. I chose the latter option, and wrote what I hope is an inoffensively bland abstract.

Secondly, I enclose for your examination a report on the 3C166 observations. I felt that as long as I was going to write a formal report, I might as well do it as a first draft of a paper. I realize that you are probably deluged with pedagogical activities, but I would appreciate your commentary as soon as is convenient. Your Obedient Servant —

Stan S.

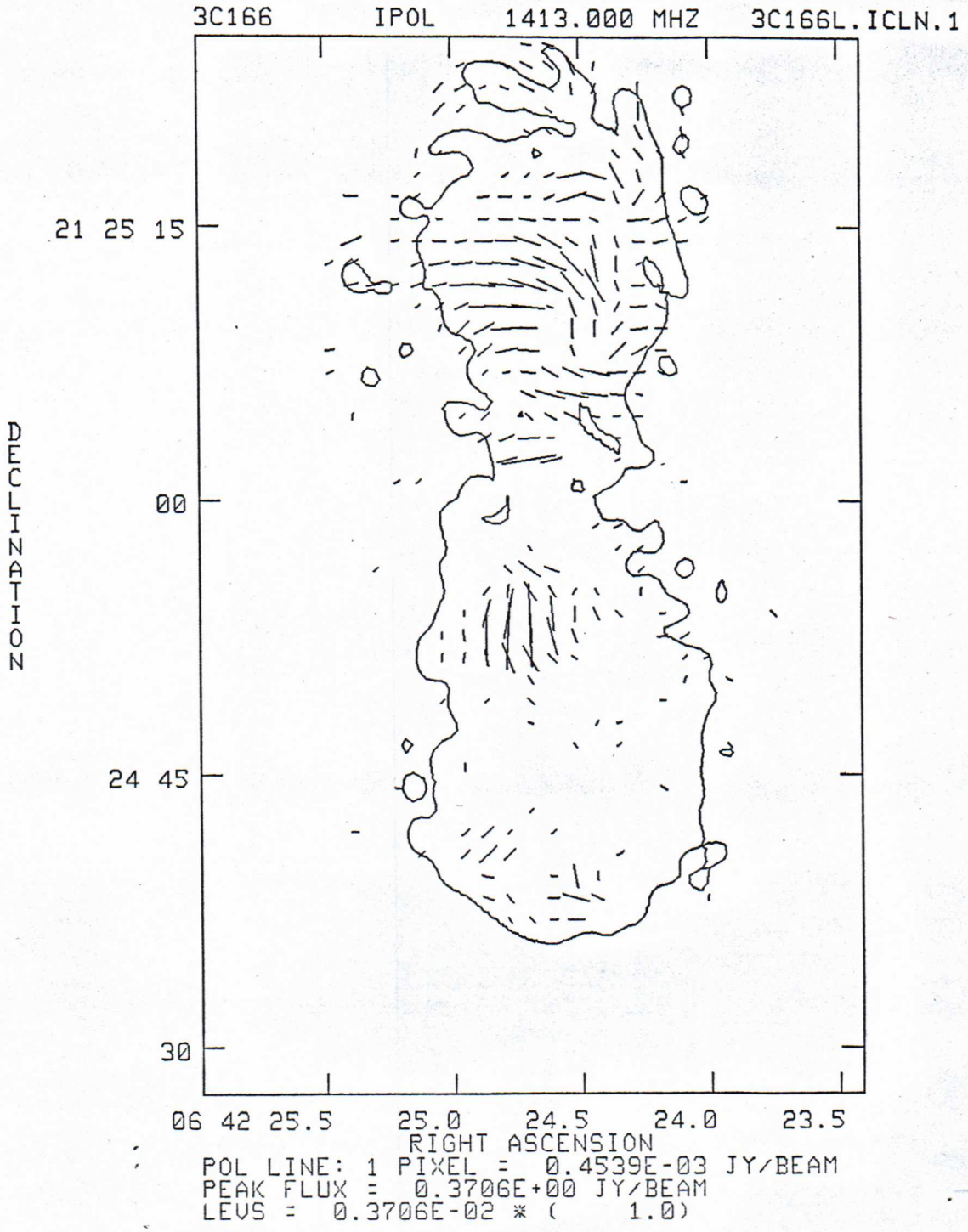


Fig.5

A plot in which the vectors are ∝ % pol. might be more useful as we show polarised intensity already as figs 3 and 4.

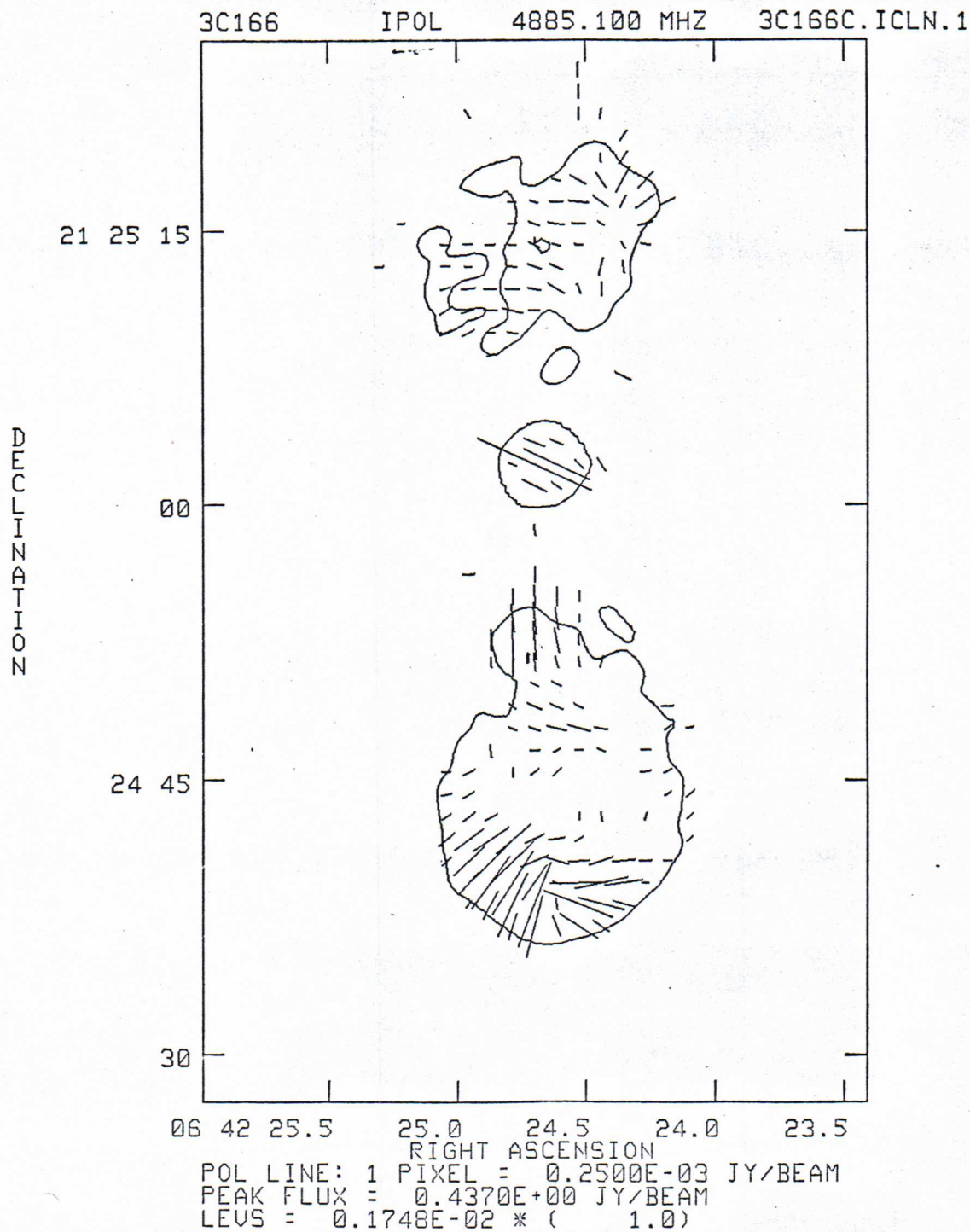


Fig. 6

% bdn here, too?

The plot below, which compares a cut through star #1 with a homogeneous cylinder model, shows that the true brightness distribution is considerably "jaggier" than the homogeneous cylinder model although the latter is not a bad approximation for rough calculation. Rough estimates indicate that the diameter does not greatly change through the remainder of the lobe, but stays about 6".

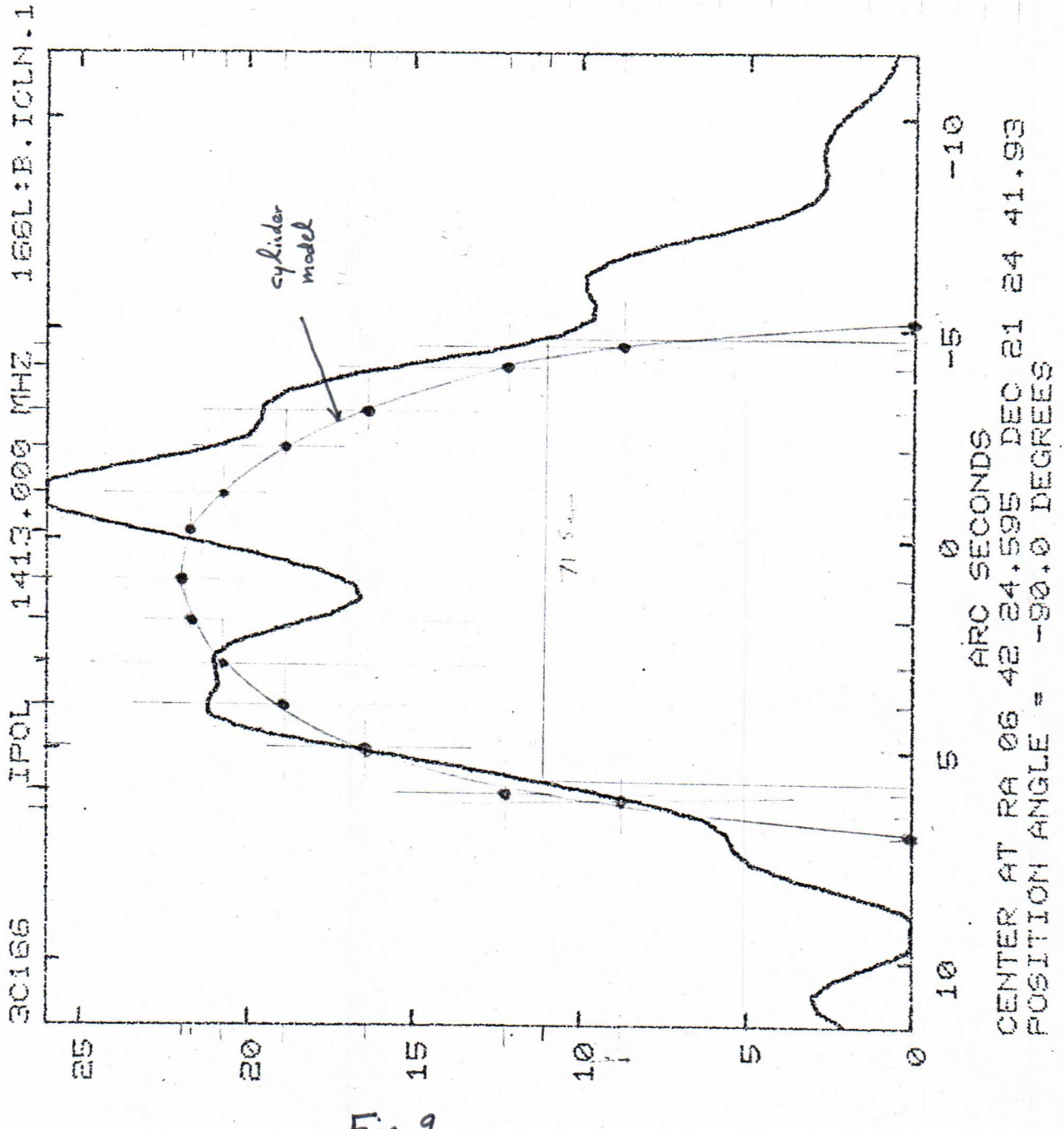
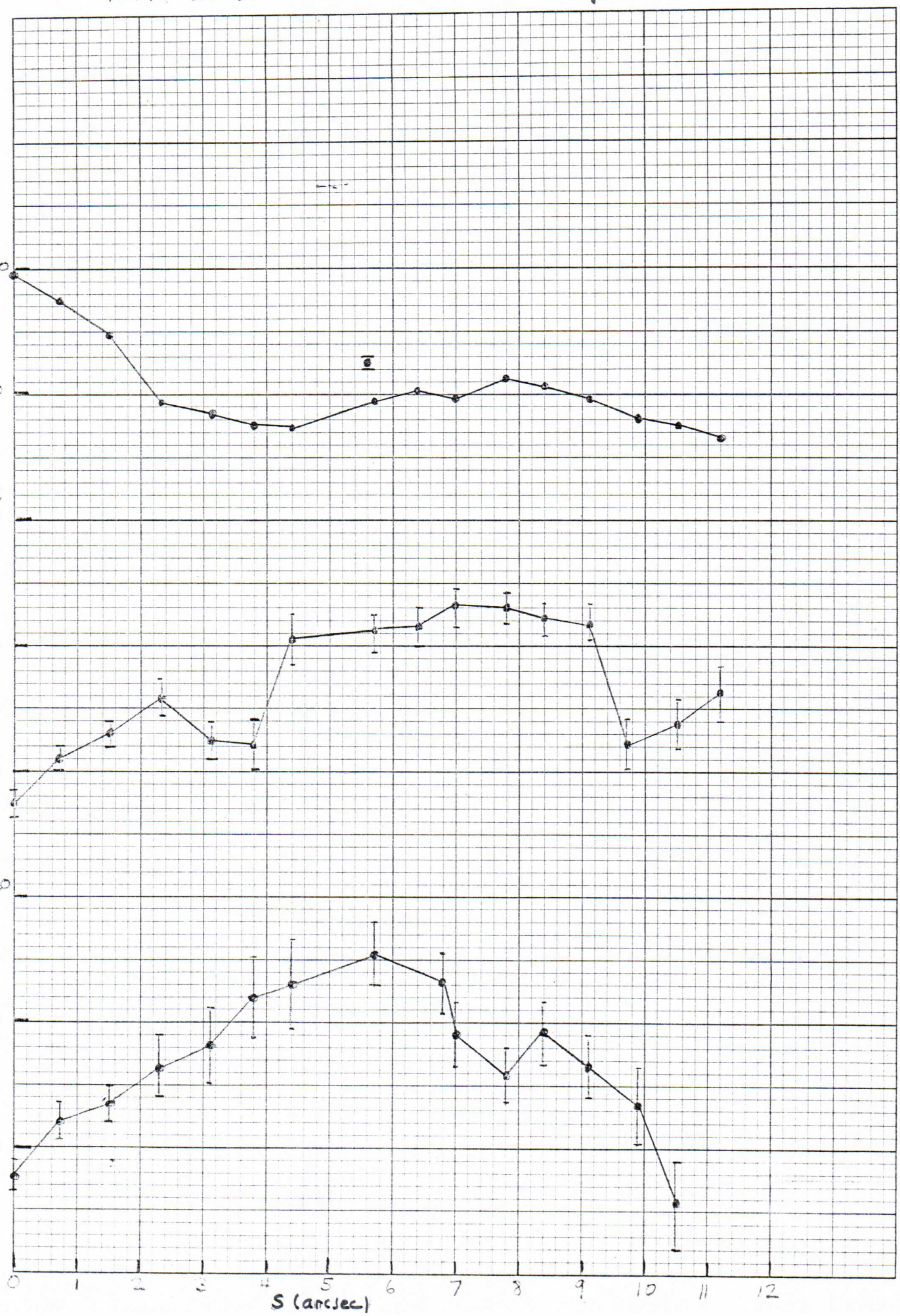


Fig. 9

NORTH LOBE SLICE

Fig. 10



10-22-81

An interesting idea — if the southern lobe of 3C166 is a shock phenomenon, and achieves a similarity solution, then one might expect the intensity of radiation to be a power law of distance since quantities such as $B_\perp$, H_ν etc are power laws. The data at L band from the E-W axis (data from p 321) are plotted below.

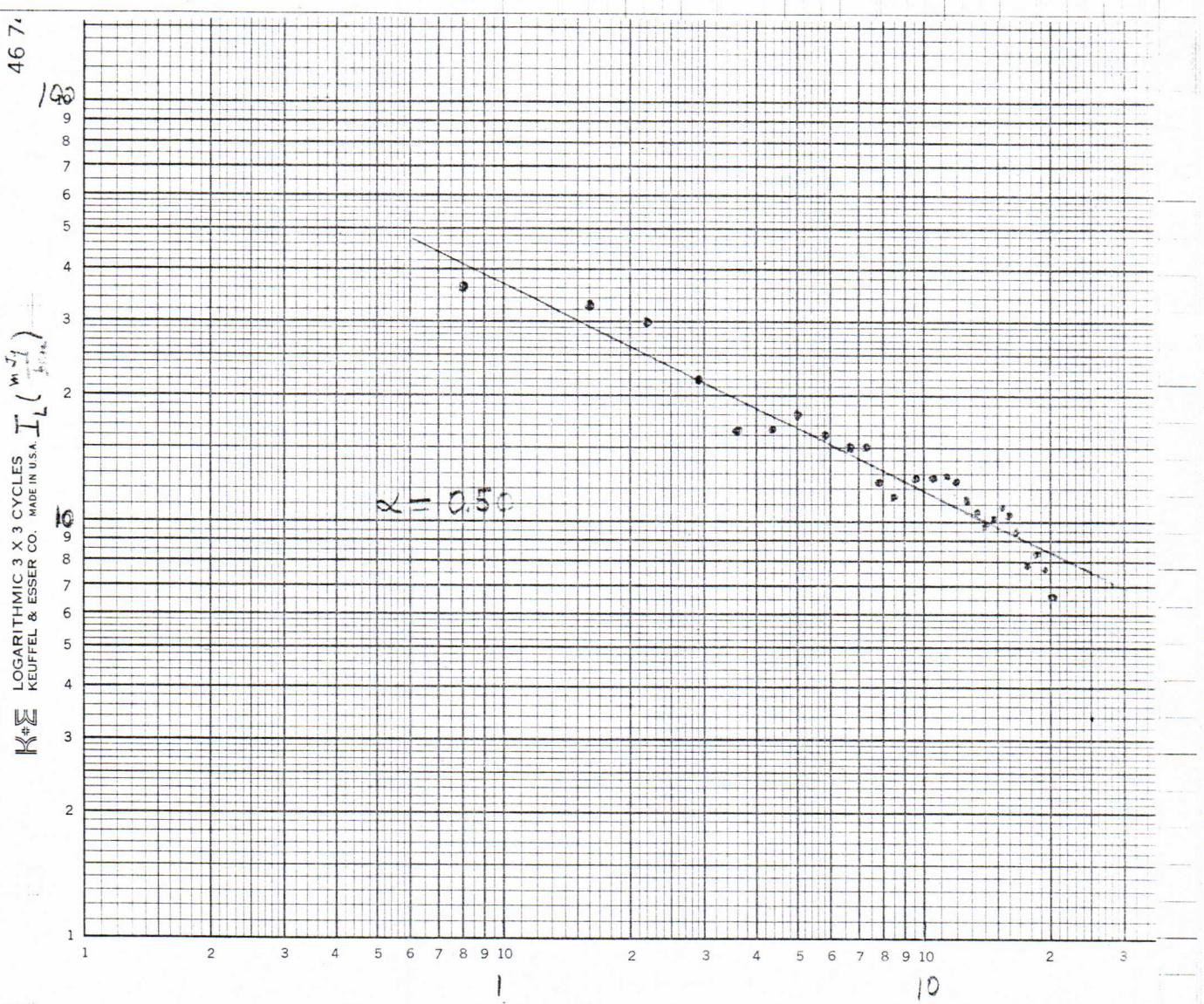


FIG. 12

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May 21, 1982

Dr. Norman Baker

The Astronomical Journal

Department of Astronomy

Columbia University

Pupin Building

New York, New York 10027

Dear Norman,

Enclosed are two copies of the revised manuscript "Dual-Frequency VLA Observations of the Extended Radio Galaxy 3C 166" by Alan Bridle and myself. We did not find the referee's comments particularly helpful. After careful examination of the paper, Alan and I concluded that we could not reduce the paper by anything like 30% without significant loss of scientific information, vital descriptions of technical procedure, and continuity with previous work. Nonetheless, we did delete or shorten certain sections which we felt were of lesser import, thus somewhat shortening the paper. Since the referee did not appear to have any scientific objections, we hope that the paper will now be judged suitable for publication.

Sincerely yours,

Steve

Steven R. Spangler

April 02, 1980

Dear Alan,

Thanks for your letter and the preprint. I was pleased to hear that you are interested in a collaboration on 3C166. My proposal for B array time was approved and will be scheduled in June. I have 6 hours to look at 3C166 and 3C327.1 (=1602+014), which is another steep spectrum variable from your 300' project. I am enclosing a copy of the proposal for your inspection.

I am particularly interested in the variability of the central components. I would think we would have a good chance of getting additional time in the future to study long term variability of these objects.

I am looking forward to working with you on this project.

Regards,

Stu

THE UNIVERSITY OF IOWA
IOWA CITY, IOWA 52240

Department of Physics and Astronomy
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(1)

Feb. 21, 1981

Dear Alan,

Thanks for sending me your map of 3C166. I find it amusing that our traditional predator–prey (or more likely, predator–scavenger) relationship has been reversed for once. In your letter you inquired about two points, (1) the agreement between your map and mine as regards the source morphology, and secondly the question of peak intensity on the map. I will address both of these below.

(1) Source morphology. After looking at your very fine map, my reaction is, aside from the obvious one that your map is of higher quality with a much better dynamic range, that the source morphology is the same. My experiment was intended to look for central components, with the nature of the extended structure being of secondary interest. Each map was made from only about four cuts worth of observation. The observations were made in July 1979, when the antennas were more randomly located than is presently the case, and the most distant antennas on the southeast and north arms were 1.95 and 0.55 km from the wye center, respectively. In addition, I had some fairly substantial problems with data quality, so quite a few antennas had to be edited out. All of this resulted in a dynamic range of about 20:1 on most of the maps. This was adequate for my

purposes of finding central components, but should be kept in mind when making detailed comparisons of the extended structure. If you display your maps starting at the 5% contour (as is the case for my published maps), I think the agreement is quite good as regards the position angles, angular extents, and gross morphologies of the North and South extensions. The map I published in the A.J. paper had a quite heavy taper, about 4 Km if I recall. Looking at your tapered maps, we see that the extended emission does seem to come all the way into the nucleus.

With regard to classifying this source, I suggested a "symmetric jet" nature on the basis of three observed properties. (1) The extended emission was linearly distributed and extended all the way back to the compact core. I think both of us would agree with this. (2) On the basis of my map, I felt that the transverse extent of the extended emission was small compared with the longitudinal extent. I expected that higher dynamic range observations would show the same transvene extent. We can see from your map that this is not the case, and that the source is much fatter at lower intensity levels. (3) It was my impression that your lower resolution GB interferometer map (which you published with Ed Fomalont in A.J. 1973, 83, 704) showed an even more extended lobe further north than the northern "jet" on the VLA map. Upon re examining your GB interferometer map however, it looks to me like the northern lobe on your GB map apparently is the feature we see on the VLA maps.

In summary, I think your description of "asymmetric double" probably is a better description than "dual jet". It is nonetheless interesting that the emission does seem to extend all the way from the lobe back to the central component. I wonder if this source (whose luminosity falls between the Class I and Class II sources) represents a transition between the low luminosity "jet galaxies" and the high luminosity doubles in which jets are not seen.

(2) Intensity scales on the maps. From my observations, I obtained a very "solid" number of the central component flux, which was 0.45 Jy at L band and 0.41 Jy at C band. These numbers result from both calibrated visibility plots and untaped maps (which showed essentially nothing but the central component). If you are getting central component fluxes significantly lower than this, it might be very good evidence for variability of the central component, as seems indicated by the results of your 2.7 GHz variability survey. I think this question should be pursued further. Don't be confused by the 0.29 Jy peak flux listed in the Figure caption to the map. I recall that there was a good reason for the difference between this and the 0.45 Jy number quoted above. I do not immediately remember what the reason was (without inspecting the old data), but it might have been that the cell size used in the map was a significant fraction of the synthesized beam size. At any rate the number you should use for comparison with your observations is 0.45 Jy for the L band central component flux. In Table I of my paper I list the error as 0.05 Jy, but I believe the number is actually much better than that.

With regard to the availability of my data, the data base is "backed up" (a charming phrase) on tape. All of the line printer output, maps, visibility plots, etc., are also in a stack labeled "AS36 Data" in the bookshelf in my office at the site. Feel free to rummage through it.

Finally, I have a proposition for you. I currently have pending a proposal for 2nd quarter (AS74) to make L, C, and K band observations of 3C166 and another source from the Spangler and Cook paper, 3C327.1 = 1602+014. Would you be interested in collaborating? I would very much enjoy working with you, and we could pool our data and ideas, especially with regard to 3C166. I am especially interested in pursuing this matter of the variability of central components in extended sources, and 3C166 appears to be an outstanding candidate. If my proposal gets on, we would have 3 VLA observations over a two year period, plus a 2.7 GHz measurement from your GB interferometer observations. That should tell us something. I will be returning to Socorro on June 1, so we should have time to work together. Let me know what your feeling is.

Regards,

Steve

P.S. — Could you get a Comptel image of your 3C166 map

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5 February 1981

Dear Steve,

I finally got a couple of hours to look at 3C166 = 0642+214 in the A configuration, and enclose some preliminary maps. These raise two questions which you may be able to help answer.

First, the source no longer looks as though it contains a symmetrical jet structure. Rather, it appears to be an asymmetrical double with elongated lobes extending back to the core. I'd like to make a map at the same resolution as that published in Spangler and Cook, AJ, 85, 659 (1980). Could you tell me what resolution that map had?

Second, the peak intensity on these maps is very significantly higher than the 0.29 Jy peak intensity quoted on your map (fig. 1 of your paper), but slightly lower than the value (0.45 Jy) you give for the "correlated flux" on your longest baselines, and for the "core flux" in your Table I. Is there anything in your data set which might account for these differences? I would be interested in comparing your visibility data with mine directly if you have yours in a convenient form. It would obviously be of interest to check for variability of the core by comparing the data sets.

Best wishes,

Alan