THE ASTRONOMICAL JOURNAL

Department of Astronomy, FM-20
University of Washington
Seattle, Washington 98195

(206) 545-2150

June 3, 1986

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

Dear Dr. Bridle:

Thank you for your letter of May 27. Thank you for sending a copy of the form to Dr. Henriksen at Queen's University in Ontario. Thank you also for sending your addition to the list of references directly to Hal Henglein, who is now more directly involved with the publication of your paper, "Collimation and Polarization of the Jets in 3C 219."

Yours sincerely,

Paul Hodge

Paul Hodge
Editor

PH:jcs

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

May 27, 1986

Dr. Paul Hodge
Editor, *The Astronomical Journal*
Department of Astronomy, FM-20
University of Washington
Seattle, WA 98195

Dear Dr. Hodge,

I am pleased that the paper "Collimation and Polarization of the Jets in 3C219" by A.H.Bridle, R.A.Perley and R.N.Henriksen has been accepted for publication in *The Astronomical Journal.*

Your letter to me of 22 May 1986 did not state whether your office had sent a copy of the page charge authorization and reprint order forms separately to Dr. Henriksen as I suggested. I have therefore forwarded a copy of the forms to him from here. As mail to Canada is slow, there may be some delay in your receipt of his completed forms if you did not in fact mail them to him directly.

Since submitting the revised version of the paper, I noticed that a reference is missing from the reference list. The following should be added to the reference list:

Schmidt, M. (1965). Astrophys. J. **141**, 1.

I am sorry for this oversight on our part. I have sent a note mentioning this addition to Mr. Henglein at AIP in parallel with this letter.

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

May 27, 1986

Mr. Hal Henglein
American Institute of Physics
335 East 45th Street
New York, NY 10017

Dear Mr. Henglein,

This concerns the paper "Collimation and Polarization of the Jets in 3C219" by A.H.Bridle, R.A.Perley and R.N.Henriksen which has been accepted for publication in *The Astronomical Journal* and is now scheduled for the September 1986 issue.

Since submitting the paper, I have noticed that a reference is missing from the reference list. Please add the following to the reference list:

Schmidt, M. (1965). Astrophys. J. **141**, 1.

I am sorry for this oversight on our part.

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

May 27, 1986

Prof. R. N. Henriksen
Department of Physics
Queen's University at Kingston
Ont. K7L 3N6
CANADA

Dear Dick,

The A.J. has accepted our paper, and they sent what appears to be the only publication charge and reprint form to me here, contrary to what I had asked them to do. Here are blank copies of the form, plus a copy of the order that NRAO will be placing, for your information.

The paper is tentatively scheduled for the September 1986 issue.

I now plan to be around in Kingston during the first week of August; a fixed point is John and Paula's mortgage-burning (and John's 50th birthday) celebration at the log house on the first weekend of August. Will keep in touch on our plans as they develop. I am definitely interested in staying in close contact with your jet emission modeling work, having developed a fair bit of code in that area myself. It might also be profitable for your student to have access to our new "minisupercomputer" (Convex C-1) at NRAO for some of that work. It is about 40 times faster than a Vax for scalar code, and has a vectorizing, optimizing compiler that accepts Vax standard FORTRAN and makes it run like a bat out of hell. (Top speed is about one-tenth of a Cray X-MP). So I could offer access to a fast machine as well as my thoughts on what the critical problems are. It would also be interesting to interface the code to AIPS so that we could use the full range of display and smoothing algorithms that have been developed for radio astronomy data processing, in order to view the models in a way that matches what observers actually do.

I look forward to talking with you both about all this.

Best wishes,

Alan

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Stirling Hall
Kingston, ON K7L 3N6 Canada

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Date Signature of Authorized Agent

THE ASTRONOMICAL JOURNAL

Department of Astronomy, FM–20
University of Washington
Seattle, Washington 98195

(206) 545–2150

May 22, 1986

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

Dear Dr. Bridle:

We are happy to inform you that your manuscript, "Collimation and Polarization of the Jets in 3C219," written with Richard A. Perley and Richard N. Henriksen, has been accepted and is tentatively scheduled for the September 1986 issue of The Astronomical Journal.

We have not specified to our Production Editor at AIP to make any of the seven figures plates. If you had intended Figure 4 to be a plate, would you please let Hal Henglein know right away?

We will make every effort to see to it that your article appears in this issue. If you are planning to be out of town within the next few weeks, it would be helpful if you could make arrangements for the proofs to be forwarded or otherwise appropriately handled in your absence. It is important, of course, that they not be delayed.

Please consult the enclosed blue sheet of guidelines to answer any questions you might have about the next steps in the process of publishing your paper. The yellow form is to be sent to the AIP offices in New York as soon as possible.

I look forward to seeing your paper in the Journal.

Yours sincerely,

Paul Hodge

Paul Hodge
Editor

PH:jcs
Enclosures

National Radio Astronomy Observatory
EDGEMONT ROAD, CHARLOTTESVILLE
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Dr. A.H.BRIDLE
tel. [804]296-0375 TWX 910-997-0174

May 16, 1986

Dr. Paul Hodge
Editor, *The Astronomical Journal*
Department of Astronomy, FM-20
University of Washington
Seattle, WA 98195

Dear Dr. Hodge,

I enclose two copies of a revised version of the manuscript of the paper "Collimation and Polarization of the Jets in 3C219" by A.H.Bridle, R.A.Perley and R.N.Henriksen.

Our responses to the referee's points are as follows:

1. We believe that both Figures are needed, as they emphasize different aspects of the data. Figure 4 is the clearest display we have found for the degree of polarization variations while Figure 5 emphaiszes the magnetic field direction and orderliness.

2. We now quote the error on the 58% polarization (±6%) on page 7. The referee's question about the three-dimensional structure of the field can be answered only by making the observations that we outline in § VI (pages 18-21).

3. This comment, and discussions with several of our colleagues, have prompted us to elaborate the relativistic jet models in more detail. § VII has therefore been broken into four subsections. The first three sections (pages 21 to 24) now explicitly discuss the chain of argument that leads to the class of model that we adopted here, and also show why simpler models would fail. We believe that this expanded discussion should answer the referee's main comment.

The referee's parenthetical remark invokes a wide range of possibilities that cannot be constrained *ab initio*. However, since submitting the original version of this paper we have realised that one possibility (that of reducing the jet sidedness ratio by disordering the jet velocity field near shocks), is particularly attractive in the context of relativistic jet models. We now discuss this possibility explicitly, and have modified our critique of the relativistic jet models in §§ VII, VIII and IX in the light of it. Our main conclusions are unaltered, however, as they depend on the fact that the main jet does not reach the lobe, but terminates in a feature whose properties resemble those of a lobe hot spot.

§VII (p. 25-26) and § IX (p.30) also now mention a second possibility related to the referee's third comment—namely that interactions between a jet and ambient clouds could produce intrinsic asymmetries (whether or not the jet has a relativistic velocity).

4. We now use "kpc" throughout.

We have also updated the references, and have stated more clearly (on page 13) the assumptions that are conventionally made by the model we used to describe the hot cluster atmosphere—namely that both the velocity dispersion and the gas temperature are constant throughout the cluster. This approximation (common in the X-ray literature) is inexact, but is adequate for our purposes.

I hope the paper will now be accepted for publication in the *Astronomical Journal.* As I mentioned in my original letter, two-thirds of the publication costs will be borne by NRAO, and one-third by Dr. Henriksen. It will expedite obtaining signatures for the page charge approval if you will send copies of the reprint order and page charge authorization simultaneously to me and to Dr. Henriksen at Queen's University. All other correspondence in connection with the paper should be sent to me at NRAO, except for any which will arrive during the period June 9 to June 27 1986, for which my address will be: c/o Aspen Center for Physics, Astrophysics Program, P.O. Box 1208, Aspen, CO 81612.

Yours sincerely,

Alan H. Bridle

THE ASTRONOMICAL JOURNAL

Department of Astronomy, FM–20
University of Washington
Seattle, Washington 98195

(206) 545–2150

April 8, 1986

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

Dear Dr. Bridle:

Your interesting paper, "Collimation and Polarization of the Jets in 3C219," written with Richard A. Perley and Richard N. Henriksen, has been reviewed carefully, and the reader's report is enclosed.

I encourage you to consider the reviewer's recommendations for your manuscript and to return it after you have made appropriate changes. Please send us two copies of your revised paper. It would be very helpful to have a detailed listing of your responses to each of the referee's points.

Thank you for submitting your manuscript to The Astronomical Journal.

Yours sincerely,

Paul Hodge

Paul Hodge
Editor

PH:jcs
Enclosures

THE ASTRONOMICAL JOURNAL

Review of the paper "Collimation and Polarization of the Jets in 3C 219"

by Alan H. Bridle, Richard A. Perley, and Richard N. Henriksen

I am happy to recommend this paper for publication. It reports on some important observations of the source 3C219 (which contains one of the first radio jets discovered). While I do not agree with all of the emphases, the discussion is clear and thoughtful. I only have minor suggestions whose implementation (or not) I am happy to leave to the authors.

1. Do you need both Figures 4 and 5?

2. The 58 percent maximum polarization is striking. What's the error? Do the authors believe that it is compatible with a sheared, but tangled field or does it have to be unidirectional?

3. Don't the difficulties with relativistic beaming disappear if you stop trying to associate features in the jet with the counterjet? (Also, there is no reason that there only be one speed in the source.)

4. I know it's illogical but surely it's kpc not Kpc.

The referee's report on 3C219 came yesterday. It says:

I am happy to recommend this paper for publication. It reports on some important observations of the source 3C219 (which contains one of the first radio jets discovered). While I do not agree with all of the emphases, the discussion is clear and thoughtful. I have only minor suggestions whose implementation (or not) I am happy to leave to the authors.

1. Do you need both Figures 4 and 5 ? Yes.

2. The 58 per cent maximum polarization is striking. What's the error ? Do the authors believe that it is compatible with a sheared but tangled field or does it have to be unidirectional ?

3. Don't the difficulties with relativistic beaming disappear if you stop trying to associate features in the jet with the counterjet ? (Also, there is no reason to suppose that there be only one speed in the source). Expand §VII

4. I know it's illogical but surely its kpc not Kpc.

==========

We have much more significant suggestions for revision internally !

I propose to add some discussion of the fact that IF the counterjet is the redshifted counterpart of the main jet and both are freshly burrowing their way out into the cocoon, then the brightness ratio can indeed be anything we like just at the tip, because this is where the velocity field gets disordered by shocks and the beaming geometry no longer applies. This penny dropped during a discussion with Steve Gull over beer and steaks at Datil, so I propose to thank Steve in the acknowledgments (note that the existing ones are made even more relevant by this). I think the relativtic jet picture is made much more viable by this, and propose that we say so.

We should emphasise more that the approximation to the isothermal sphere holds only over a few core radii, but that this is where most of the X-ray emission comes from, so it is valid for our purposes. Geoff Bicknell was somewhat misled by this, though our calculations are in fact valid in the context we are applying them. We might also give some more emphasis to the fact that the X-ray luminosity measured at 3C219 would be rather high for an individual galaxy. This strengthens everything we are saying about a cluster being involved. We might also emphasise the fact that it makes a very plausible story that we see the jet brightening because it is a previously free jet hitting a cluster component of the pressure at about a clister core radius (we imply this but we might say it more explicitly so that the casual reader doesn't lose it).

==========

Any comments on the referee's comments, or FINAL ones on the text ? I'll try to send this back this week despite the imminent arrival of Bob Sanders in C'ville to work on the 157-source alignment mapping !

From: CVAX::BRIDLE "Alan Bridle" 15-MAY-1986 15:44
To: VAX3::RICK,CVAX::ST%"lokenc@qucdn.bitnet",BRIDLE
Subj: FINAL edits requested by RAP/RNH to 219

You both sent me some last-minute suggestions. Here's what I have done with them:

==========

In the subsection VIIb and c titles: Add "symmetric" before "relativistic flows"

==========

Following the paragraph on "why it isn't all due to expansion" in Sect. VIId:

A bend in the jets could also contribute to the decrease in ${\cal S}$ at the counter-jet knot. A bend in the counter-jet similar to the observed $11\circ}$ ``kink'' in the main jet could make ${\cal S}$ larger near the core by increasing $(v_j/c)\sin\iota$ there.

Finally, we note that moving shocks may also arise if the jet entrains, or collides with, ambient clouds (e.g., Blandford and K\"onigl 1979a,b; Henriksen 1984a). If this occurs, the shocks in the jet and counter-jet could be intrinsically asymmetric around the core. If moreover a real collision occurs between the jet and a cloud (i.e., if the mean free path in the cloud for a jet particle is less than the cloud scale) then initially a strong shock should appear. This could be a most effective means of decelerating a counter-jet and of removing an unfavorable beaming from it. In time, however, this situation would translate itself into an accelerating terminal shock. If moving shocks are introduced into relativistic jets by such jet-cloud interactions, the asymmetries in the observed knot locations would be governed by randomness in the cloud geometry rather than by expression (2) above. As there is indeed evidence for such cloud-jet interactions in some radio galaxies (e.g., van Breugel \etal\ 1985), we emphasize that the above interpretations of knot asymmetries in terms of {\it symmetric} relativistic jets are not mandatory.

We conclude that a relativistic jet containing symmetric fast-moving shocks could readily display apparent asymmetries similar to those we have observed in 3C219 ....

==========

New penultimate paragraph in the Conclusions:

The jet/counter-jet asymmetries might also be the result of strong shock formation in the both jets by jet-cloud collisions at random distances from the core; in this case the appearance of the jets would be a manifestation of circumgalactic ``weather'' rather than a consequence of bulk relativistic motions.

==========

It goes back in the mail tomorrow unless I hear from one of you to the contrary. I have added the necessary references and updated some of those that were preprints when we submitted, with their journal pages.

Let's F*I*N*I*S*H it tomorrow !!

==========

From: CVAX::BRIDLE "Alan Bridle" 14-MAY-1986 17:45
To: VAX3::RICK,BRIDLE
Subj: RE: 3C219

OK -- in the sontext of smooth jets there's nothing to measure the geometric asymmetry with, so I'm happy to leave that as it is. You need some kind of discontinuity in order to be able to see whether or not anything is moving. And the shocks are the thing we're using as the discontinuity.

The bending sentences are back in, as you requested. I just dropped them for brevity, but who needs to be that brief ?

From: VAX3::RICK 14-MAY-1986 16:49
To: CVAX::BRIDLE
Subj: RE: 3C219

The angle asymmetry is the theta(1)/theta(2) relationship for identifiable knots moving relativistically, after having been 'launched' at the same time, same velocity, opposite angles, etc. That is, your equation (2). The relation holds for smooth jets, except that there are no features to track. In section a), you stated the only asymmetries visible is the brightness asymmetry, which is true for a smooth flow, as you have stated. My suggestion was to point out the difference in angular expansion which would be noted, if there were any features to follow. Your developmer has this point being brought out later, when discussion the shock propagatic model. I was only wondering if the two effects - the brightness asymmetry, and the angular expansion asymmetry, may better be mentioned together. This is not an important point by any means.

The bending argument is a rather obvious one, at least to 'beamers' (defined as those who truly believe in Doppler favoritism), and I think it would be appropriate to at least mention it in passing - perhaps in the sense that it does give another way of explaining the observed structures.

More MAIL to follow on other subjects...

From: CVAX::BRIDLE "Alan Bridle" 14-MAY-1986 14:24
To: VAX3::RICK,BRIDLE
Subj: RE: 3C219

I'll add "symmetric" to the subsection titles.

I have always used the angle from the plane of the sky; was just trying to be consistent.

Which sizxe asymmetry doi you mean for the smooth flow ? I'm not clear what you are really driving at, here.

The error in the polarization came from the noise estimates and the actual values of P and I in the part of the jet where we measured 58%. (I did that by "hand").

Glad you like the new version; I think it is a lot clearer than the old one.

I did drop the bending argument from the new version; this was because we now had a much better explanation for the brightness ratio, I thought. The bending business could, of course, still contribute to the ratio we see. Would you like me to reinstate it (I still have a copy of the old text around somewhere, so it's easy to put it back in).

Something else I have thought about but not added is that the observer sees a relativistic jet spread at c_s/gamma (it spreads at c_s in its frame). This of course counter-acts the inclination effect on thspreading a to make jets in strong sources look narrower than those in weak ones. But it applies to the free parts of the jet, which is not what we are concentrat on here.

From: VAX3::RICK 14-MAY-1986 14:10
To: CVAX::BRIDLE
Subj: 3C219

I hope it's not too late for comments. On the other hand, none of the following are important.

In subsection (d), concerning the apparent termination of the main jet, I had thought that we once discussed a change in jet direction, and hen in Doppler boost, as a viable explanation. Is this no longer held? (It mig not work unless jet velocity is very near c). [In the above, I mean Section VIII, not subsection (d)]. Is this idea mentioned elsewhere?

The new discussion, prior to section VIII deals only with perfectly symmetric flows, I believe. This is stated in the first heading (\heading (\it a) Smooth, Symmtric ...)\endheading, but not in subsequent headings. While it is fairly clear that these subsequent discussions deal with symmetr flows, there is a (probably small) chance of confusion. Do you think that inserting the word 'symmetric' is over-kill?

I have always wondered why you prefer to use \iota, rather than angle from the line of sight, \theta, favored by (I think) all the VLB commu

In the very first changed section, a), you have not stated the size asymmetry that occurs in relativistic flows, presumably because you have also stated 'smooth'. Is it worth noting that identifiable, symmetrically produced features would show this angular asymmetry, so as to intoduce the moving shock picture?

I like this new version.

Oh, by the way, where did the error in degree of polarization come from?

From: CVAX::BRIDLE "Alan Bridle" 9-MAY-1986 15:06
To: VAX3::RICK,BRIDLE
Subj: 1 of 2 on 219

I have now completed the revisions to the 219 paper to my own satisfaction. I have done the following:

1. Added the error (+/- 6%) to the 58% polarization.

2. Made the assumptions of the King-law model more explicit (i.e. everything, not just T, constant through the cluster). Geoff Bicknell points out that this is not consistent with the potential associated with the mass distribution, but it's standard in the X-ray literature and does not affect our conclusions at all. At least I am making the fact that it is an approximation more explicit now.

3. Pointing out that the luminosity of the 219 source is also typical of a cluster, and reinforcing the idea that 10 Kpc may be where the jet "lights up" as a result of feeling this atmosphere as opposed to 3C219's (Geoff B. had not fully appreciated this from reading the earlier version).

4. I have substantially rewritten Sections VII and VIII to epound more clearly on the version of the relativistic-jet model we were using. This is partly in response to the referee's comments and partly the result of the discussions I had with Steve Gull. Its worth spelling out the differences between the moving-shock model and the simpler alternatives that people often describe. (We were describing a moving-shock model all along, but I have written a much clearer description of that fact in this version). I have also written in the discussion of the brightness ratio changes at shocks, and have rewritten the segment on the adiabatic expansion evidence accordingly. I now think the change-direction issue is an un-necessary complication and have dropped it from this section. I have drawn a clearer distinction between Section VII and the unsteady-jet model (Section VIII). I think it is all much clearer as a result of this. I hope the changes are not so large that they will want to re-referee the paper now !

5. I have made small changes in the abstract and conclusions to reflect the changed tone of the discussion in Sections VII and VIII.

6. I have added acknowledgements to Geoff for his comments and to Steve for the many discussions he and I had over beer and steak at the Eagle Guest Ranch last month.

7. I will tell the editors that we do indeed need all the Figures.

I will follow this with a mail message containing the revised Sections VII, VIII and conclusions.

I will resubmit the paper next week, and this time send out preprints, unless I hear from you to the contrary by the middle of next week.

From: VAX3::RICK 21-APR-1986 18:38
To: CVAX::BRIDLE
Subj: RE: 3C219 referee report

The referee's comments are certainly minor, and the two revision you plans are certainly o.k. with me. So do it!

From: VAX3::BRIDLE "Alan Bridle" 30-MAR-1986 11:50
To: CVAX::BRIDLE
Subj: Hot Gas

From: VAX3::PEDLAR 21-FEB-1986 16:12
To: VAX3::BRIDLE
Subj: hotgas

Alan,Joel,

I'm interested in your hot gas meeting, especially the role of active galactic nuclei in the heating.

As I'm working on NGC1275 I thought I'd like to throw a few ideas into the meeting...if only to have them shot down!

The first question I have is whether we really do have pressure dri inflow in the hot halo. The radio halo has a size of ~100kpc and a radio flu of ~ 6jy at 1.4 Mhz which implies an average pressure of ~ 10e-12 dynes/cm-2 using the minimum energy assumption. The 30" component (Miley & Perola 1978 (10kpc) has a pressure of 10e-10 dynes/cm. From Fabian et al Ap J 248 47 (1 density at 3arcmin is 10e-2cm-3 and the temperature 5e7K which will give a pressure of~10e-11 dynes/cm-3. In view of the somewhat flaky assumptions on both sides, it seems possible that the radio emitting gas could have a significant influence on the pressure driven inflow. In fact if the r has expanded to 100kpc radius then wouldn't it have to be at a higher pressu than the xray halo?.

If the energy density in the relativistic plasma is approximately the same as the Xray gas,is the assumption that the inner gas will radiatively cool still valid? wouldnt it be reheated by shocks from the expanding radio plasma? or shocks which generate the relativistic electrons? The current energy in relativistic particles works out to be about 10e59ergs which is comparable to the thermal energy of the Hot gas over the same volume..

In the case of ngc1275 the Xray gas seems to be concentrated around this galaxy rather than the centre of the cluster (see Braduardi-Raymont ApJ 248 55 fig 1) I guess this could be error in galaxy counts but could it be that some of the Xray gas is generated by ngc1275, rather th being a consequence of dynamic heating by galaxies in the cluster?

It seems to me that a good fraction of the heating could be due to relativistic plasma expanding and shock heating the ambient medium. I this expands ~800km/s then it will shock heat the gas to 10e7K which will ha a cooling time › 2e8 yrs if ne~0.01cm-3. This could be much longer than the lifetime of the radio source and hence the Xray emitting gas may be still be present after the radio source has diminished.

The long term energetics are my main concern as if the cluster gas has a mass of 10e13 Mo then if the thermal velocity is 400km/s this gives a total energy of ~10e61 ergs. However in view of the long cooling time this could be due to a number of AGN events integrated over 10e8-10e9 yrs.

These ideas are highly biassed towards ngc1275, I must admit Ive not looked into much detail on other sources. Ive sent some pictures, figures et over with Rick Perley.

cheers Alan Pedlar

THE ASTRONOMICAL JOURNAL

Department of Astronomy, FM–20
University of Washington
Seattle, Washington 98195

(206) 545–2150

March 18, 1986

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

Dear Dr. Bridle:

We have received your manuscript, "Collimation and Polarization of the Jets in 3C 219," written with Richard A. Perley and Richard N. Henriksen, and it will be given prompt attention.

I am enclosing a copy of the American Astronomical Society's Transfer of Copyright Agreement. Please complete this form and return it to our office as soon as possible, so that there will be no delay in handling your manuscript.

Yours sincerely,

Paul Hodge

Paul Hodge
Editor

PH:jcs
Enclosure

From: CVAX::BRIDLE "Alan Bridle" 17-MAR-1986 18:12
To: VAX3::RICK,BRIDLE
Subj: RE: 3C219 paper

We stated that it is an approximation to the isothermal sphere. 90% of the luminosity comes from within the 3 core radii, I think. We also are consistent in using the Jones and Forman beta with the law that they used to fit the data.

The paper has been sent off, of course. Your copy of the version as submitted is in the mail.

From: VAX3::RICK 17-MAR-1986 18:09
To: CVAX::BRIDLE
Subj: 3C219 paper

Geoff Bicknell is here, and is reading the 3C219 paper. He has a number of comments, one of which is of more than a minor nature. He has been engaged with a correspondence with Jones and Forman, correcting an errors of theirs. The discussion revolves around the phrase 'King Law'. Geoff points out that this 'law' is really King's approximation to the isothermal sphere, and is invalid beyond about 3 core radii. What is important is that apparently J and F have not accounted for the discrepancy beyond this limit in their derivation of the parameter beta. When proper account is taken, the mean of 2/3 becomes a mean of 1 (because the density law should go as radius**-2 rather than radius**-3, as the 'King Law' would have it).

I suggested we talk about this, and other things next week. Do you have a comment to pass on at this time?

You may be interested to know that Steve Gull will be arriving next week, to stay for 2 months.

Going skiing tomorrow (Winter has arrived, finally, though rather late).

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

March 12, 1986

Dr. Paul Hodge
Editor, *The Astronomical Journal*
Department of Astronomy, FM-20
University of Washington
Seattle, WA 98195

Dear Dr. Hodge,

I enclose two copies of the manuscript, with glossies for seven figures, for an article entitled "Collimation and Polarization of the Jets in 3C219" by A.H.Bridle, R.A.Perley and R.N.Henriksen. I hope that this will be suitable for publication in the *Astronomical Journal.*

Please note that one copy of the manuscript is marked with Figure and Table locations, and with identifiers for unusual symbols. This copy is intended for use in your office, while the other, unmarked copy is intended for use by the referee.

If the article is accepted for publication, two-thirds of the publication costs will be borne by NRAO, and one-third by Dr. Henriksen. It would expedite obtaining signatures for the page charge approval if copies of the reprint order and page charge authorization could be sent simultaneously to me at NRAO and to Dr. Henriksen at Queen's University. All other correspondence in connection with the article should be sent to me at NRAO.

Yours sincerely,

Alan H. Bridle

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From: CVAX::BRIDLE "Alan Bridle" 7-MAR-1986 18:01
To: VAX3::RICK,BRIDLE
Subj: Dick H. and 3C219

I have now received, by BitNet, Dick's comments on the version of the paper I sent to him by BitNet. His main comment is to point out that the KC model is not the only one than could produce an inverse correlation between polarization and total intensity, but that any model which bases particle acceleration on turbulence would have this general property also. This leads to a general statement that all of the effects predicted by the KC model could have alternate interpretations in terms of other phenomena, so it is particularly important to search for the quantitative relationships KC predicted. I have added statements to these effects.

I have also decided to quote at greater length from Mattthews et al. at the start of the paper as they explicitly mention (a) the companion galaxy and (b) the fact that 3C219 is much brighter than the other cluster members. I wonder in fact how sure we can be that 3C219 is IN the cluster and not just projected against it, and have embellished the section where we talk about observations needed to test thermal confinement to make this point -- i.e. I point out that the radial velocities of the galaxies are needed to establish cluster membership as well as to measure the velocity dispersion.

I now have a double-spaced version ready to go (though it's after mail time and so won't go today).

I will JUST have this out of the door as I get buried in administrivia again.

Fred and I have gone over the cover art and title page of the Workshop together. I hoipe I can produce the Table of Contents at home this weekend.

From: CVAX::ST%"lokenc@qucdn.bitnet@Hamlet.Caltech.Edu" 6-MAR-1986 22:02
To: BRIDLE
Subj:

Received: from Hamlet.Caltech.Edu by CVax with DECNET ; Thu, 6 Mar 86 22:01:
Received: from QUCDN.BITNET by Hamlet.Bitnet with BSMTP via BITNET ;
Thu, 6 Mar 86 19:02:12 PST
Received: From QUCDN(LOKENC) by HAMLET with RSCS id 8850
for MAILER@HAMLET; Thu, 6-MAR-1986 19:02 PST
Date: Thu, 6 Mar 86 19:02:12 PST
From: lokenc @ qucdn.bitnet
To: bridle @ cvax

ALAN
OK! Now to business. For some deeply mysterious reason I have received only the first part of the edited manuscript. That is down to the first mention of fig. 3 under "polarized intensity". Therefore I am more than usually in the dark. However one comment is: We should insert "turbulently" before viscous when describing the Henriksen confinement model.
Moreover, when discussing KC paper, note that the inverse correlation betwee polarization and intensity is expected in any model which appeals to a turbu t acceleration mechanism. I would like to see the rest of the revised text i it is possible. I will check the magic net again tomorrow. My grant can stan the strain. Greetings from vacuum land.
Dick
QUIT

From: CVAX::BRIDLE "Alan Bridle" 4-MAR-1986 19:30
To: VAX3::RICK,BRIDLE
Subj: 3C219

I have put in a bit about the Konigl-Choudhuri model (reference for magnetically assisted collimation).

They predict oblique field segments with a spacing 5R, while what we "see" is a spacing 10R (if R = Phi/2, the assumption we made for NGC6251).

Note that they also predict an anticorrelation between %P and I (a rather nice feature, as it is qualitatively seen in many jets). I may insert a sentence pointing this out, as we had independently drawn attention to the anticorrelation in this case.

We might also note that the high degrees of polarization seen at marginal transverse resolution imply a high degree of polarization at the edges of the jet. Perhaps this is obvious, but people may not do the beam convolutionm in their heads.

I am presuming that we will send this to A.J.

lokenc@queens cdn

BITNET

3C219"

Profile of jet sensitive to conductivity (how you scrap the energy around). Prandtl # is sensitive to how the viscosity cuts across the jet.

Critical # (mixture of electron mobility + Mach number) determines whether the jet synchrotron maps flow parameters.

Does the synchrotron jet map the flow?

Clumpy background → cooling flows?

Sidney vdB M87 C.C.D. jet + debris around nucleus in Hα.
Ford/Butcher filaments.

Shocks. Restriction must c) how is reconfining
b) discontinuity in medium needed. → M_j R_j scale
fine structure in the medium.

Puzzle. Why no more evidence for magnetic confinement?

Hi degree of polarization even at low transverse width.

Center current and center-fields

Plasmoids intermediate + jets flip-flop elongated population could not be confined at far

$\rho v^2 \sim$ pint at far.

From: CVAX::BRIDLE "Alan Bridle" 29-AUG-1985 13:51
To: VAX3::RICK,BRIDLE
Subj: RE: 3C219

Sounds like some confusion here. I had likewise told them that we had done 1 20cm and 1 6cm frequency and had shown them our maps. They seemed at that time to think that they did not have much to add (this was while we were all at the Taos meeting). In terms of VLA usage the best thing might be for them to do two more 20cm frequencies and then combine their data with ours ? Why don't we go ahead on that in collaboration with them ?

From: VAX3::RICK 29-AUG-1985 12:25
To: CVAX::BRIDLE
Subj: 3C219

Jack Burns and Dave Clark sent me a proposal to observe two radio galaxies with the intention of looking for differential rotation measures around jets. One of their choices, 3C401, was badly chosen - too small, forcing them to observe at 6 and 2cm, where the effect they're looking for would probably be unobservable. I inquired into why they had not chosen 3C219, as in my mind, this object is ideal for such a search. They replied that you had said we had already done the experiment. This surprises me, since we have only observed at 1 20-cm frequency, and 1 6-cm frequency. Their proposal calls for multiple frequencies at 20cm, which is certainly the right way to go for the experiment. I recall that we had looked for evidence of differential RM in 3C219, and the result was inconclusive. With the limited data we have, I would feel a little uneasy attempting to fit RMs. So, why shouldn't they use this source for this search? They stated t 3C219 is easily the best candidate they found in their search of those sourc with known jets, and I think they should have a crack at it, especially sinc our analysis is (essentially) going nowhere. On the basis of all the above, I strongly recommended to them that they throw out 3C401, and insert 3C219. you have any objections, please inform me soonest.

From: CVAX::BRIDLE "Alan Bridle" 3-FEB-1986 12:12
To: VAX3::RICK,BRIDLE
Subj: 3C219 readers

I think the time may have come to get some external readers for the 3C219 paper. I would give it to Chris O'Dea and Neil Killeen here. How about sending a copy to Jack Burns, and using it to prompt him for some information about whether or not he did include us in the 3C219 follow-up proposal ?

Diagrams are on their way to you by (slow) mail.

National Radio Astronomy Observatory

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Dr. A.H.BRIDLE
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January 31, 1986

Prof. R. N. Henriksen
Department of Physics
Queens University
Stirling Hall
Kingston, ON K7L 3N6
CANADA

Dear Dick,

I have been trying to raise you by telephone for some time without success – you're a hard guy to get hold of these days.

Here is my redraft of the 3C219 jet observations and interpretation, now aimed at publication in *A.J.* Given that we cannot settle the confinement question, I don't think it's *Ap.J.* stuff. The main changes from the earlier version are that I have concluded that, as $L_X \propto r_c^3$, we must take a more cautious approach to estimating r_c. The new version is more conservative, and allows for the possibility that we are looking at a shocked region of the jet just downstream from where it recollimated. The result is that confinement by an intracluster atmosphere now looks quite reasonable; confinement by the galactic atmosphere requires too much L_X still, and rather unlikely values of n_e and r_c, in my view.

I could illustrate the statements about jet emission models with plots from my own model zoo if necessary, but feel that the generalities drawn here are sufficiently obvious from the diagrams already published by Robert Laing, and from common sense, that it is not worth detailed elaboration.

Please decide as soon as possible (a) whether you still want to be associated with this, given that its theoretical part is not exactly heavy-duty—it will cost you one set of comments to be delivered soon and $\frac{1}{3}$ of the publication costs to be delivered later, and (b) whether you have have any pearls of additional wisdom to include. Rick is visiting me in Charlottesville from February 17 to 21 and we'd like to close it out at that point. Give me a call.

I am sending the plots to our graphics people straight away. They will use only what is inside the plot borders, and will do the usual job of NRAO-standard lettering around the edges. The AIPS-isms will also be removed (plot numbers, lists of contour levels and image names, etc.)

The library at Green Bank found it had some left-over reprints of our old *J.R.A.S.C.* article, so I am sending you on some with this.

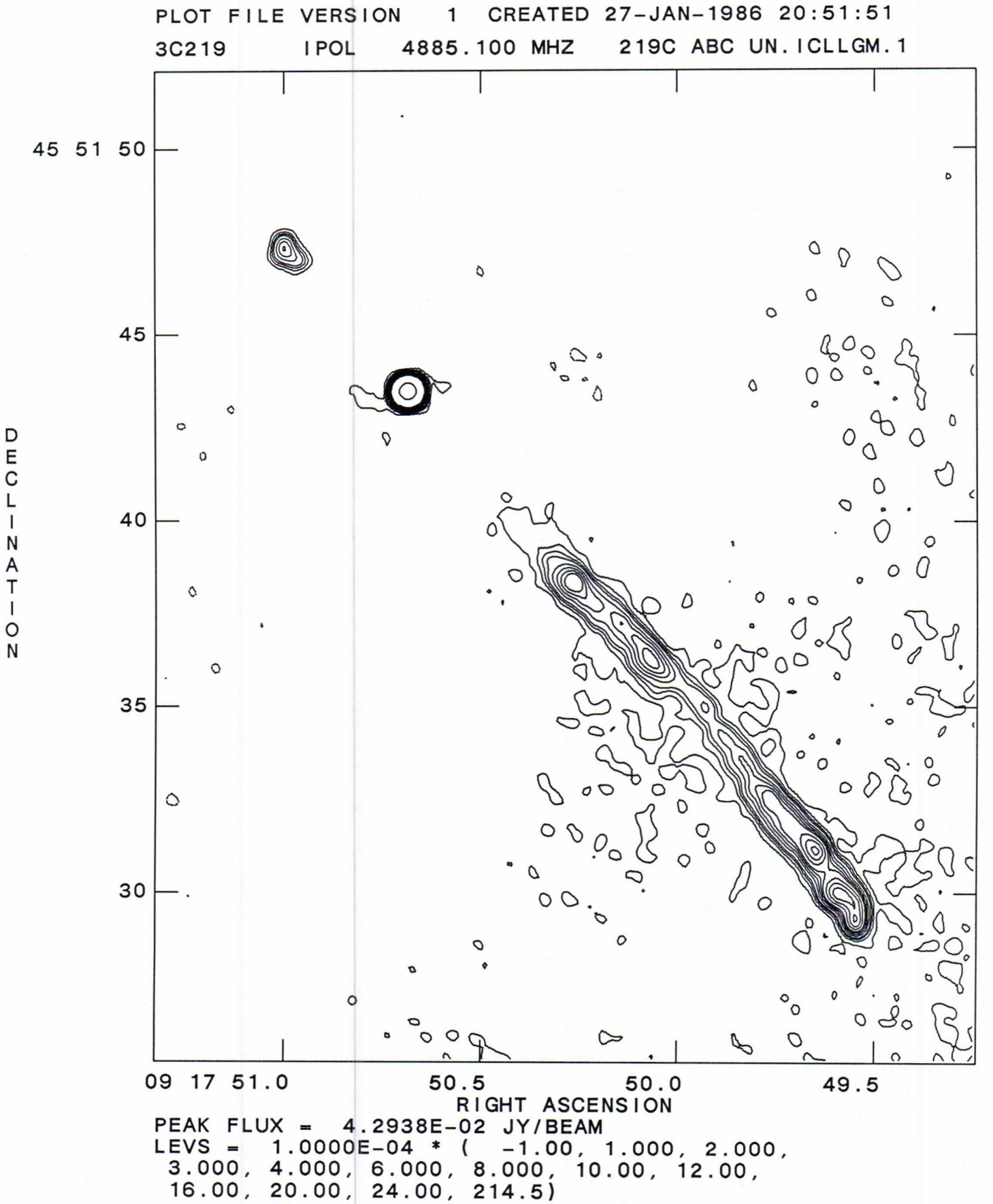
How's the CITA issue of *C.J.P.* coming ?

Best wishes to yourself, Wai and Kayll,

Alan

3C219 Collimation Data
0.35 arcsec resolution

THETA	IOBS	PHIOBS	IJET	PHIJET	LOGINRM	LOGPHI
6.60	1.19	0.640	1.420	0.5358	1.011253	-0.27098
7.50	0.53	0.800	0.589	0.7193	0.629080	-0.14304
9.76	0.99	0.660	1.167	0.5595	0.925905	-0.25215
12.88	0.39	0.540	0.511	0.4112	0.567729	-0.38592
14.70	1.13	0.680	1.317	0.5830	0.978500	-0.23432
15.40	1.02	0.810	1.130	0.7304	0.912151	-0.13639
16.32	2.00	0.580	2.506	0.4624	1.257796	-0.33489
17.16	0.95	0.780	1.062	0.6970	0.885205	-0.15672
18.14	2.03	0.620	2.457	0.5117	1.249335	-0.29093
-5.08	0.92	0.530	1.223	0.3979	0.946496	-0.40012


PLOT FILE VERSION 1 CREATED 27-JAN-1986 20:51:51
3C219 IPOL 4885.100 MHZ 219C ABC UN.ICLLGM.1
45 51 50
45
40
35
30
DECLINATION
09 17 51.0
50.5
50.0
49.5
RIGHT ASCENSION
PEAK FLUX = 4.2938E-02 JY/BEAM
LEVS = 1.0000E-04 * (-1.00, 1.000, 2.000,
3.000, 4.000, 6.000, 8.000, 10.00, 12.00,
16.00, 20.00, 24.00, 214.5)

From: VAX3::RICK 23-MAR-1984 11:37
To: CVAX::BRIDLE
Subj: RE: Meeting

I have in hand the measurements of jet widths. The beams are, in all places, listed as 1.4 to 1.5 arcsec, the half-power jet widths are all 1.7 to 2.1 arcsec (I'm rounding).

SHZT!!!!!!!

I have taken the original map, as plotted, and re-measured the jet width and beam. At the point of brightest emission in the jet (point 5 in our old, very wrong paper), the jet width is 1.27". I measure the beam to be, in the same position angle, 1.05". The deconvolved width is 0.7"!

Where did I go wrong? I think the answer lies in the use of SEEMAP. I clearly recall using the GO PROFILE part of this wretched program to produce the tracings. I would suspect that the interpolation used in this program is crude, and it is quite possible that it produced the approximately 30% expansion which appears to be common throughout the measurements.

Well, how are we going to explain this one away?

From: VAX3::RICK 23-MAR-1984 11:46
To: CVAX::BRIDLE
Subj: Errors in last MAILing

I mis-measured the maps leading to the conclusion in the last note. Getting the tik marks (i.e. the scale) correct, the new and correct jet width at point 5 is 1.55", the beam is 1.3", so the deconvolved width is 0.85". Thus our old numbers are not so far off.

From: VAX3::RICK 23-MAR-1984 11:52
To: CVAX::BRIDLE
Subj: Knot 2

I measured knot 2, with the correct scale. The width comes out to 1.9", with considerable room for measurement error, due to the poor S/N. The deconvolved width, of course, is still far too wide. Given the uncertainty due to poor S/N, and to the uncertain baseline, I think we can cover ourselves on the old paper. The only real error is in not trying to account for systematic errors in that paper. The error formulation I used presumed that the only errors were statistical ones in the measurement of widths.

There is a curious distribution of spectral index in the 3C219 lobes. The lowest indices are on the outward edges of the hot spots (the North rim of the shell, and the south rim of the L), then in the regions between these features and the lobe edges. The latter effect implies further outflow beyond the sharp features (making the conventional assumptions). Do you see anything like that in Cygnus or other doubles you have mapped ?

To Rick 12 Jan 88

From: VAX3::RICK 16-DEC-1983 10:54
To: CVAX::BRIDLE
Subj: RE: 3c219.

It might be possible that the jet widths can be slightly contaminated by the diffuse emission. It is easy to check on this, as the visibility function has a very sharp 'knee' around 10 kilolambda, and maps could be made excluding the extended emission. Otherwise, we could (manually) subtract off the background from the profiles, and then compare them.

I believe the old 'A' maps had the rings problem. The effect is really a very subtle one - the rings on the final maps had an amplitude only twice the noise - and only a few percent of the brightness of the jet.

From: VAX3::RICK 15-DEC-1983 19:18
To: CVAX::BRIDLE
Subj: RE: 3c219.

The problem with the 6cm data was inded peculiar. UVPLT showed at low of high fluxes at one (only) particular value of the baseline length. CLIP took out about 300 points (only). After remapping, all those annoying rings, which I had previously blamed on closure errors, which surrounded the phase center, had completely dissappeared! The new map is truly superb.

A couple of other neat things. The spectral index across the source acts exactly as in Cyg A. Values of about 0.6 (or less) in the southern hot spot and northern ring, with a semi-linear increase in spectral index towards the core, reaching about 1.5 to 2.0 when we run out of signal. The lack of good u-v coverage has caused the 20-cm maps to be a little nibbled, but this in no way bothers the trends stated above.

The percentage polarization of the jet is only about 40-50%, not up to 60% as we previously stated. The counter-knot has a much lower percentage polarization. I would say that there is no evidence for depolariz on (depolarization). The percentage polarization map is complicated, but similar, for both frequencies. I haven't attempted to make a rotation measure map, but I'm pretty sure that it is low - the Q and U maps are very similar for 20 and 6cm.

It's a nifty source, and the data is very good.

See you next week. By then, I'll have the best 3C390.3 maps the world has ever seen.

From: VAX3::RICK 6-DEC-1983 16:01
To: CVAX::BRIDLE
Subj: RE: Return of AHB

The system is dead easy. Put your map(s) on either VAX. Then use the TV verbs to disply the image on the screen exactly as you want to see it on film. Type in an OUTNAME, and then use the VLA-only verb 'T3'. This saves the image on disk. Next, go to DISPLAY, and run the program 'DICAPE'. This program reads the disk image and transfers it to the Dicomed.

That's it.

6 May 81

Dear Dick,

Here are the first maps reduced from our 3C219 proposal with Rick Perley. They are 6cm full-resolution maps (0".35) with the A configuration, made from the data taken while you were here in January.

Map A shows that the jet is now well resolved and has a lot of substructure. There is also a bright knot about 5" from the core in the direction opposite to the jet. This knot was not obvious on the earlier VLA maps (due to lower resolution and signal-to-noise). It appears to be elongated along the jet axis and may be part of the "counter jet".

Map B shows the polarized intensity distribution over the jet only; compare with map A for qyick look at degree of polarization. The usual vector plots will follow - we're having some computer problems this week.

Map C - the warm spot in the North-following lobe - looking more SNR-like than ever, and confirming what we said at lower resolution in the published paper. We need a good model for shell structures in radio lobes, rather than just edge-brightened hot spots. Could be tracks of wildly precessing beams, or explosive activity in the lobe, or shock fronts propagating away from the beam head ?

Map D - the warm spot in the South-preceding lobe. Note that it's not really edge brightened away from the core, consistent with its setback into the radio lobe ?

I think plasmoid models deserve some serious consideration in this source. Do jets get more and more sporadic as they get more luminosity to carry ? I'll soon send a first draft of our work on NGC 6251, in which the core is more luminous than those in NGC 315 and 3C 31, and the jet has much greater internal inhomogeneities, both in total intensity and magnetic field configuration. Extreme jet turbulence might be a good way to go to put all of this together.

Cheers

Alan

From: VAX3::RICK 25-APR-1984 18:58
To: CVAX::BRIDLE
Subj: Gas in lobes.

I don't think we are quite eye-to-eye on this question. Is it your intention to say that if the jet is confined we require either:
1) The lobes to be evacuated of external gas, and far out of equipartition,
2) The lobes full of external gas, similar to, or somewhat greater than, the presumed X-ray cluster pressure,
or 3) magnetic collimation, for which we see no evidence (yet).
A fourth possibility might be that the lobes did sweep out the external gas as they were forming, but the jet refilled them back so to produce the needed pressure.

I think this is how I see it. I see no reason why the lobes must have evacuated the external medium, although it is my distince impression that this has been assumed in all models.

Later: why not have thermal support throughout the lobes.
with mixed thermal/nonthermal pressures?

We are obviously not eye to eye (maybe we're eyeball to eyeball).

Running down your alternatives, let me suppose that we think jets are confined (from the evidence of the collimation data that we have provided the world with so much of). Then either:

(a) they are in contact with hot dense gas, which must therefore be adjacent to them, or

(b) they are not in contact with hot dense gas, and must be confined by some other pressure.

Let us take (a) first. This is the case we have been calculating when we try to do thermal bremsstrahlung checks. It is inconsistent with the first part of your option (1). If the lobes are evacuated, the gas ain't there. The second part of your option (1) then becomes moot. It is consistent with your option (2), though I don't think we need the "greater than" part of (2). It pre-empts your option (3). Your option (4) points to two sub-options of (a):

(a1) the hot dense gas came into the lobes from outside by some inhalation/ingestion process ferrying gas across the stand-off shock, or

(a2) the hot dense gas was pumped out along the jet, and is being recirculated back to the galaxy where it refuels the monster, getting repumped out the jet, etc. etc.

The difference between (a1) and (a2) is: does the radio galaxy recycle its own gas (a2), or keep ingesting gas from the outside (a1) ? The choice between them does not impact the confinement problem directly, or bear directly on the question of equipartition in the lobes. It is obvious;y an important ingredient of source evolution, however, and we have to try to make the choice somehow, some day, (far far away for most sources but maybe not for things like 3C449 and Cyg A).

My problem is that the lobes are seen to be full of synchrotron which, if it is in equipartition, is at a much lower pressure than the jets. What I am debating with myself (and you) is: what is the pressure balance between this stuff and the hot gas which coexists with it, according to (a - a1 or a2). Are we simply looking at a synchrotron emitting plasma that is uniformly mixed with the dense hot gas and is thus able to exist throughout it at a lower partial pressure, or should the dense hot gas be packing the synchrotron stuff into smaller volumes ("crushing it"). Or is the synchrotron stuff way out of equipartition (this one started out as your idea, not mine, remember !). [I don't attach the non-equipartition to option (1), as you do, but to option (2).]

Now what of option (b) ? We are forced to it if the thermal bremsstrahlung calculations come out wildly wrong, as I believe they do for Cyg A and for the big bright QSRs. Under it, your option (1) just postpones the problem - the out-of-equipartition lobe might confine the jet in the absence of dense hot gas in the lobe, but then what confines the lobe ? So I don't like (1) in the context of (b), either. Obviously (3) rears its head in the context of (b).

A further issue is -- if (3) is ever correct, then magnetic pressures may play a role in all jets, even if there is thermal gas out there as well. I recall that in the BCH paper in JRASC the jets most like what we saw in 3C219 in fact had BOTH thermal and magnetic pressures acting on them. Magnetic collimation itself tended to produce wild oscillations, but the magnetic/thermal partnership damped these oscillations down to an acceptable level.

On the evacuation question, most people assumed that the standoff shock was impregnable, for simplicity. It can also go unstable and let the stuff be ingested, although nobody to my knowledge has done anything predictive. The key question will be - how much gas is involved in the jet confinement ? If the mass required is small, then the recycling approach may work. If it is large, then it won't. Ingesting the gas rather than shocking it and pushing it back has the minor advantage that it cuts down on the total energy required to pump the source up. I

think we should compute gas masses in the models when we talk of confinement (I did this for 3C293 and NGC6251) - let's work it out also for 3C219 and Cyg A and see where that gets us.

From: VAX3::RICK 25-APR-1984 17:36
To: CVAX::BRIDLE
Subj: 219

I looked thru the new version (unfortunately, I don't know how to print it out down here). Some comments I have are as follows.
We have separated the 'extended' from the 'point' X-ray emission by using the mean relation in Fabbiano. I note in that paper that their best estimate of the size is a whopping <10.7'. That's pretty poor for an instrument with a 0.9' beam. Why is this so poor? They had rather little data on source, but on the other hand, the source is relatively strong.
I am a little worried that those who read the paper carelessly will quickly home in on the part on the top of page 8 which says that we can barely make thermal confinement work, without reading the preceding paragraphs carefully where we display the assumptions made about separating core from halo. I suggest we add, or repeat, the statement that we are presuming half the Fabbiano X-ray flux is from the nucleus.

You have added the discussion on the lack of Faraday rotation around the jet. Do you want to add that we have data on this, and will be 'reportin this in a later paper', or should we totally leave this out, as you now have. Certainly, between 6 and 20cm, there is no measureable RM gradient around the jet (Hmmm, this might be used to constrain the X-ray halo - maybe all of it is nuclear. The 3C449 RM gradients are easily explained in terms of the X-ray emission seen surrounding the galaxy).

More in a few minutes.

From: VAX3::RICK 25-APR-1984 16:57
To: CVAX::BRIDLE
Subj: Comments on 219.

O.K. Get ready. Here comes.

First off, I have not yet gotten the ratio of A array data to short array data - PRTUV only outputs to the 'line printer', and down here in Socorro, we ain't got none. Will do tomorrow.

I have derived the dependencies on h, and agree with you on all of them.

To continue our discussion about lobe/cocoon/thermal gas pressure, doesn't the problem arise because we have assumed that the lobes have pushed aside the cluster/halo thermal gas, and the minimum pressures of the halo are greatly less than those presumed in the external medium? If this is so, it seems certain that the halo pressure should 'crush' the lobe. Since this doesn't seem to be happening, the pressure in the lobes must be much greater than that given by equipartition. Either equipartition is way out (meaning the total energy rises greatly), or another pressure component is present. My hypothesis was that perhaps the same gas giving the X-rays is present in the lobes as well. This leads to plenty of predictable results, including depolarization (which may be occuring in Cyg A), and no notable X-ray images of radio lobes. The problem is how the gas gets in there. On MOnday, I mumbled something about processing it thru shocks, as the coccoo expands sideways (I mean the lobes) supersonically. This could result in significant heating, but only if the shock is strong. Let's suppose it is a weak shock - then the pressure/temperatur jump could be made small enough. On the other hand, maybe the internal pressure is a lot higher, due to a fairly strong shock - then we could really get rid of the confinement problem. Having an overpressured lobe is certainly no sin - we're sure it's expanding anyway. Another way to get gas in the lobes could be through turbulence - but I'm not too keen about this, the radio maps don't seem to lend much support to this (the radio contours are too smooth, all the way around).

This, I hope, explains what I got in my mind.

I am still working thru your new idea on expansion combined with sidedness. I think I see a flaw - but I haven't read carefully your note entitled 'the catch'. Will send stuff within a half hour,

.

From: VAX3::RICK 24-APR-1984 12:22
To: CVAX::BRIDLE
Subj: RE: 3C219 pol'n.

If the two of them occupied the same volume, this crushing would not occur. Suppose the lobes were due to shocks which propagated outwards from the 'jet' (or whatever), then the intergalactic material would be processed thru the shock. The shock would accelerate particles which should then find themselves in the thermal gas. The idea is that rather then pushing the IGM aside as the radio source expands, the expanding source uses the material along the way. This idea is closely related to those ideas we were tossing around last Wednesday nite - about infalling gas entering the lobe. I think we should have to take these ideas seriously (meaning external gas being present in the lobes) if the high gas density inside Cygnus A's lobes is confirmed by the new data.

Interoffice

National Radio Astronomy Observatory

Very Large Array
15 May 1981

To: RAP, RNH

From: AHB

Subject: 20cm maps of 3C 219

Here is the first set of untapered A configuration maps from our 20cm observations in January.

All have been CLEANed and restored with a 1".2 circular beam. The dynamic range is not wonderful and I think we may have some problems with aliasing. I propose to make and CLEAN 2048 x 2048 in the VAX with the sinc x exp convolving function to see if we can improve things. These maps were made in the usual fashion with pillbox convolution in the 11/70, using the maximum mapsize of 1024 x 1024.

Maps 1 and 2 - full field total intensity (IPOL) and polarized intensity (PPOL). Note the north-following blob in the jet is also detected here - also the considerable misalignment with the "shell" in the north-following lobe.

Map 3 - the "shell" with some surrounding lobe emission (IPOL). Maps 4,5,6 - on the same scale, IPOL, PPOL and IPOL with vectors proportional to PPOL and in PPOL's position angle. The radial E vectors and variation of degree of polarization reported in the pub-ished paper are nicely reproduced here.

Maps 7,8,9 - the same three displays as 4,5,6 but over the jet, core, "counter-jet" region. The core is less than 2% polarized. Note that we don't see rotation of the last vector at the end of the jet at 20cm, whereas we clearly did at 6cm. Possibly we will find evidence for some excess Faraday rotation at the end of the jet when we compare with 6cm at the same resolution ?

Maps 10,11,12 - the same three displays over most of the south-preceding lobe. Note the pattern in the E vectors over the extended emission relative to that over the warm spot. Also the regions of beam depolarization.

Maps 13,14,15 - the warm spot close-up.

%polarization maps will follow, when I have the VAX reductions done.