From root Thu Sep 1 14:57:40 1994
From: WARDLE1@BINAH.CC.BRANDEIS.EDU
To: abridle@NRAO.EDU
Subject: jets
Date: Thu, 1 Sep 1994 14:52 EDT

Dear Alan,

What a beautiful paper in today's AJ! I wish I had contributed more.

Scott Aaron and I are writing up (at last) the definitive (we hope) analysis of the jet/counter jet ratios with much new stuff in it. CAN YOU BRING ME UP TO DATE ON THE LAING-GARRINGTON EFFECT FOR THESE SOURCES? I have a preprint from Bridle, Laing, Scheuer, and Turner on "jet side versus spectral index in quasars." You appear to have compiled data on the effect for several sources, but I don't know which ones (it is missing a page or two). Is it in print yet?

What we have done is (following Peter's suggestion) explore various degrees of intrinsic asymmetry in the jets. Obviously if the distribution of intrinsic asymmetry is equal to the observed jet/cj distribution, then beta_jet = 0. But then half the jets we see are pointing backwards. Between Laing-Garrington and the correlation with core prominence etc, we can limit this severely and end up with pretty fast jets (>.6c) even if they aren't very symmetrical.

cheers,

john

NATIONAL RADIO ASTRONOMY OBSERVATORY

520 EDGEMONT ROAD, CHARLOTTESVILLE, VIRGINIA 22903-2475

Dr. ALAN H. BRIDLE

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June 27, 1994

Editorial Supervisor,
The Astronomical Journal,
American Institute of Physics,
500 Sunnyside Blvd.,
Woodbury, NY 11797-2999
U.S.A.

Re: 002409anj

I enclose the corrected laser proof, manuscript and supporting forms for this paper.

First, let me thank you for an excellent job of editing and composition that was remarkably error-free compared with others I have seen.

In answer to your explicit questions:

(a) the sentence on ms. page 87 suffered from a misplaced parenthesis, the correction is shown in the proof page 52,

(b) the "missing references" are all from the camera-ready Notes to Table 1, not from the text; no action is needed.

The authors' E-mail addresses (also written on the proofs) are:

abridle@nrao.edu
dhough@physics.trinity.edu
cjl@wells.haystack.edu
jburns@nmsu.edu
rl@mail.ast.cam.ac.uk

I presume that the many places in which a decimal point appears under a superscript symbol will be correctly aligned in the final printed copy. At present all such occurrences in the laser proof have the decimal point centered higher than its correct position at the bottom edge of each line.

I have a number of questions or suggestions re layout that I have not made in the proofs as their disposition depends on your willingness to make changes of this nature. They are:

1. It is unclear how Figure 14(b) is to be located in the text as there is room only for Figure 14(a) in the space above the caption. Will there be a further insert page showing only Figure 14(b). If so, how will this be referenced to the Figure caption?

2. The Figures become seriously staggered from the text that refers to them from proof page 14 onwards. This makes the latter part of Section 4 hard to follow. The situation might be improved if Figure 16 is moved onto proof page 14, Figure 17 onto proof page 15, and Figures 18 and 19 to proof page 16.

3. In Figure 20's caption it is not clear to me why you have asked for MEM to be in roman caps but CLEAN in small caps. Both refer to algorithms, and neither is the name of an AIPS task, and the split-font result you have specified looks odd. Would you consider making MEM small caps throughout the paper for consistency at this point?

4. Figure 24 was missing from the proofs. It is also desirable that it not be removed too far from the text at the start of Section 4.10 (proof page 18). Could it be placed closer to this text if the Figure changes mentioned above were made?

5. The captions of Figures 25 and 26 are in a font that is so small as to be barely readable. Was this intended?

6. Table 5 should be placed closer to where it is referenced (proof pages 27-28, not page 30 as now). Moving Figures 34 and/or 35 forward to pages 27 or 28 would also help, as the text describing Table 5 would then appear later.

7. The readability of Section 5.2.2 would be improved if Figure 37 was placed on page 31 and Table 7 on page 32 rather than the other way round, as now.

8. Figure 45 is probably the most important one in the paper, and should be moved as close as possible to the text describing it. Please move it from proof page 44 to proof page 41 if you can.

9. Several references given in our manuscript as "in preparation" or "private communication" have been recast as "Author 19xx" and moved into the reference list. (See ms. pp 38, 47, 61, 93). While this may be standard practice for the Astronomical Journal, I would like to point out that it is annoying for readers who turn to the end of a long paper only to discover that there is no published reference! We would greatly prefer to revert to our original manuscript copy for all of these cases. Also, our original form was consistent with the style used in the References to Table 1, which contain several private communications.

If editorial policy requires you to move these items in the reference list, however, the following corrections should be made:

> Bridle 19xx moved from manuscript page 47 (proof page 33) cannot be the same as Bridle 19xx moved from manuscript page 93 (proof page 54). If you must turn these both into references they should be separate items labeled Bridle 1995a and Bridle 1995b
>
> Hough 19xx, unpublished should be Hough 1994, unpublished and follow Hough 1986, not precede it as you indicated.

Hough, Vermeulen and Readhead 19xx should be dated 1994 and precede Hough, Zensus, et al. 1993 not follow it as you indicated.

I have not marked any of these (#9) corrections in the proofs as they depend on whether you can revert to our usage in the original manuscript.

Finally, I realized on seeing the proof that a minor revision to Figure 47 (adding symbol labels to the X-axes) was inadvertently omitted when we resubmitted the manuscript. If it is now too late to reprocess the graphic for Figure 47 and meet your deadlines, please proceed with the present version. If, however, the revised Figure could still easily be substituted, please use the version enclosed herewith.

Thank you once again for such an error-free job. I trust the remaining details can be sorted out without much difficulty.

Yours sincerely,

Alan H. Bridle

From abridle Thu Mar 24 18:32:44 1994
From: abridle (Alan Bridle)
To: dhough@physics.Trinity.EDU, cjl@wells.haystack.edu, jburns@nmsu.edu, rl@rgosc.ast.cam.ac.uk
Subject: Short cut
Date: Thu, 24 Mar 1994 18:31:51 -0500

Here's where there are significant changes to the text in the redraft. The page numbers are for the _new_ version, .ps file recently E-sent. Note that we gained about 3 pages by moving stuff to the Table captions, but we lost almost exactly that again answering the referee's questions and spelling out the assumptions in the prominence analysis more clearly. So it's _still_ 115 pages of MS!

p.6/7 End of sec.1 attempts to clarify that summary of empirical results is in section 6, and that what follows is mostly discussion re models.

p.12 first para has added refs re Ricean bias and COMB

p.18/19 Fig.8 now only one panel

p.27 Figure 22c deleted (note this is one more than in draft of letter I sent you, this was the next one on Jack's and my short list and when I went through again I decided it fell into same category as 8b/8c and could go ...)

p.34 middle para, new text re errors in positions for 3C351 central feature and QSO

p.37 middle para, new text re errors in positions

p.39 first whole para. Added sentence re none of cj candidates meeting jethood criteria (this is first of several, to make this point inescapable no matter how readers jump!)

p.41 top line now refers to hook-like features by their labels in Figures, to cross-reference

p.42 line 5 from end now says counterjet "candidate"

p.43 New Figure 37 inserted at start of 5.2.2. (all subsequent Figures renumbered). I prefer Dave's draft of this Figure to my own, and the one job remaining is to computerize his picture to a .ps file by the way).

p.45 Major deletion in text moves Table 8 description to caption.

p.48 Ditto for Table 9

p.50 Ditto for Table 11

p.53 Ditto for Table 12

p.55 Ditto for Table 14 (end of gains in space ....)

p.60 Correction to first sentence of 5.5.1, as pointed out by Colin and Jennifer Carson we deleted the candidates from the lobes as well when doing the prominence analysis.

p.62 5.5.2, line 1 now refers to Figure 45a

p.69 New material at start of 6.1 -- "no clear cj's" again!

p.70 more use of word "candidate" where appropriate

p.71 line 6, state explicitly we mean ratios too when "assessing"

p.72 first full para, added ref to FRI/II transition with one number and some new text. Slight rewording at end of para also for efficiency.

p.75 New 6.5.1 on detection rates. Just so we can hit the reader who starts by reading this section with the basic detection statistics. Old 6.5.1 and 6.5.2 follow unchanged but renumbered.

p.78 Reworded and expanded to deal with the small-intrinsic-scatter assumption (Colin's concern).

p.78/79 Para. that spans these two has been rewritten to emphasize (a) the result from the angle rangle 20-70 that we were anyway quoting for the jet/counterjet ratio fit, and which does a better job of matching the observed prominence range. Also to give an example of how the conclusion changes if we use an angular range that is close to the line of sight. I hope this and the preceding change handle all of our own "corrections" to the paper based on further thinking about the prominence-prominence interpretation. This is probably the section that everyone should reread most carefully!

p.89 Start of 8.1 now tries to tell the reader who jumps in at this point that some summarizing has already occurred elsewhere! Also to set up this section as a "where are we going" section rather than "where have we been"?

p.90 Dave had suggested we also try to deal with the referee's "where are the conclusions"? comment by expanding our rediscussion of the counterjet detections in what is now last para. on this page. I've tried that, tell me how it reads ...

p.92 Last 3 lines, I've changed the wording to suggest that we don't necessarily want to fit the slope of the linear relationship when looking at the correlation. With bigger samples, we should probably fit models directly rather than work through fitted slopes, as Colin and dave discussed in their E-mail.

References. New ones are Leahy & fernini, Owen 1993, Vinokur and Wardle & Kronberg. (Latter is a concession to fact that the classic Vinokur paper is in French, but note that W. & K. refer you on to it at the crucial point!).

Figure captions.

All of the polarimetry captions have been identically reworded to save space, given that we defined p on p.12.

Added material identifying crucial bits of sources:

Figs 1,3,5,8,10,12,14,16,20,22,24,25,26,27,30,32,34,36.

any more needed?

New caption for 37, all others relabeled.

I think this is _the_ last draft (miGawd!), so any and all comments are appropriate, no matter how large/small ......

Cheers, A.

From root Tue Apr 5 11:17:13 1994
From: dhough@physics.Trinity.EDU (David Hough)
To: abridle@polaris.cv.nrao.edu
Subject: "x" correlation
Date: Tue, 5 Apr 94 09:58:37 CDT

Alan,
OK, now that you've reminded me, I recall your position on this quite some time ago. It's all right with me to leave things as they are, I suppose, since we already point out how sensitive "jx" is to 68.1/351, so I guess there's not an urgent need to point out that they also affect "cjx". With this said, I still would not object to the addition of a sentence on p. 62 that suggests the weaking of "cjx" is apparently due to this sordid pair. Yes, I hope you hear from Colin & Robert soon as well!

-Dave

P.S.: of course, that should be "weakening" a couple lines up.

From root Sat Apr 2 18:52:21 1994
From: dhough@physics.Trinity.EDU (David Hough)
To: abridle@polaris.cv.nrao.edu, cjl@dopey.haystack.edu
Subject: Prominence correlation AGAIN!
Date: Sat, 2 Apr 94 17:34:32 CST

San Antonio, TX
April 2, 1994

Alan, Colin:

You've probably both been aware of this for a long time, and if you told me about it, sorry it slipped my mind. But I've just spent an inordinate amount of time worrying about the different results for the central feature-straight jet prominence correlation when normalizing by jetted lobe extended emission vs. counterjetted lobe extended emission.

After tracing through the shift in every data point at each step in the process (i.e., observed flux -> rest frame flux -> log rest frame flux -> log prominence), it became abundantly clear that 3C68.1 and 3C351 play disproportionate roles in BOTH of the prominence correlations (i.e., "jx" and "cjx").

The "jx" case, of course, we're all aware of and have discussed carefully in the paper. But I've personally never understood, or been comfortable with, the strong weakening (near destruction?) of the correlation in the "cjx" case. Recall that the linear-correlation coefficient r=0.83 for "jx", 0.65 for "cjx". As we note in the paper, dropping 68.1/351 lowers r to 0.67 for "jx". However, it is also interesting that dropping 68.1/351 RAISES r to 0.76 for "cjx", which is a more "favorable" result than for "jx"!

This is because these two sources undergo the largest shifts when going from log rest frame flux -> log prominence in BOTH cases. Their strong jetted lobes give them unusually low prominences that pull them "down to the left", away from the other sources, enhancing the correlation. But their feeble counterjetted lobes give them more middle-range prominences, that push them "up to the right" to a region OFF the main line of the other sources, weakening the correlation.

By the way, the correlation of the log rest frame fluxes, BEFORE any normalization to prominences, is quite good for all sources (r=0.77) and even without 68.1/351 (r=0.75).

And as one more check, the rest frame fluxes themselves (NOT logs) have r=0.62 (similar to 0.63 for the observed fluxes quoted in the paper). I was a little worried that taking logs seemed to enhance the correlation. But I think we're safe, because the correlation of the actual prominences (NOT logs) yields r=0.76 for all sources, r=0.72 w/o 68.1/351. So it's there, no matter which way you look at it.

Finally, and perhaps most importantly, depending on your point of view, one should ask what happens if the total extended emission = "jx" + "cjx" = "x" is used. For all sources, r=0.77; w/o 68.1/351, r=0.70 (about as one would expect). An advantage of this is that the huge lobe flux ratios in 68.1/351 are downplayed, and a "legitimate" correlation emerges for all 13 sources. The fitted slope with York's method for all 13 sources is 0.55 +/-0.13.

WHAT'S THE BOTTOM LINE? What the astute observer would have concluded from a quick inspection of Figs. 45a&b in the paper. The two points responsible for by far the largest differences between these two figures are 3C68.1 & 3C351, because of their extremely unequal lobe flux ratios. Should we

(a) add a sentence or two saying this on p. 62; ✓
(b) switch to total ("x") extended emission to mitigate against extreme effects of these two sources; or
(c) just ignore it?

-Dave

P.S.: The "x" plot PostScript file follows; I KNOW it's a bit ridiculous, but I can't help noticing on this plot that the only source "out of line" is 3C47, and I'll bet it's not a bad guess that upon resolution comparable to the other 12 sources (rather than 4 times poorer), 3C47 would conform more closely to the line. In fact -- don't hit me! -- r=0.91 w/o 3C47 (but w/ 3C68.1 & 351)!

From root Mon Mar 28 00:26:29 1994
From: dhough@physics.Trinity.EDU (David Hough)
To: abridle@polaris.cv.nrao.edu
Subject: Hough's FINAL comments
Date: Sun, 27 Mar 94 23:09:20 CST

San Antonio, TX
March 27, 1994

Alan,

I went through all 115 pages this weekend, and have only the following comments:

(1) p. 24 vs. p. 32: in the first case, he's "R. Barvainis"; in the second case, "Richard Barvainis". This is an example of pursuing ridiculous consistency! ✓

(2) p. 46, Sec. 5.2.4, 1st par., 3rd sent.: might read "Instead, PHI/THETA, which roughly measures the (projected) angular {\it collimation} of the knot, is often larger at small THETA." Not essential, but I just noticed this got dropped in transferring table descriptions from the text to notes in the tables.

(3) p. 58, line 3: "I(x,y)" should be italicized in this first line following the equation. ✓

(4) p. 58, line 5: "S" in "S(lx,ly)" should be italicized. ✓

(5) p. 69, Sec. 6.1, 1st line: you probably mean "Sections 4 and 5.2.1". ✓

(6) p. 72, line 7: without any local context, is it clear to all readers that the superscript "5" in the total power expression means 5 GHz? ✓

(7) p. 98, Laing 1993 ref.: there should be no period at end. ✓

(8) Figs. 1 and 20 Captions: should we not point out, for consistency, that {\bf A} contains the counterjetted hot spot in 3C9 and 3C249.1? All other sources that have cjhs's, not merely failed candidates, get some mention of them. ✓

(9) Fig. 26 Caption: the "(D and the ridge linking it to C)" at the end looks like a word-processor carry-over that should go. ✓

(10) Fig. 36 Caption: "A contains the JETTED hot spot." ✓

(11) Fig. 37 Caption: is this brief one OK, or should we make a few simple remarks - e.g., cf=central feature, hs=hot spot, open circles represent jet knots, "c" subscript is centrally-referenced, "l" subscript is locally-referenced (and for "l", the dashed lines indicate a fit to the feature positions)? ✓

That's it - doesn't amount to a hill of beans, does it? That's because it REALLY looks to be in beautiful shape. I also felt, upon reading it after essentially a 3-month layoff, that it's even BETTER than I thought before. Thanks again for the wonderful job of pulling it all together and polishing it to virtual perfection.

-Dave

From root Wed Mar 23 18:09:16 1994
From: dhough@physics.Trinity.EDU (David Hough)
To: abridle@polaris.cv.nrao.edu, cjl@dopey.haystack.edu
Subject: Angle ranges
Date: Wed, 23 Mar 94 16:47:47 CST

Alan,
Yes, I agree with you that choosing the angle range to fit the observed prominence range is fine for our intrinsically similar sources. However, I still think we should be careful to point out that various combinations of intrinsic prominence and angle ranges obviously could also be made to work (but without bothering to go into any specific details or examples). But you're right, the gamma_j will hardly change at all, and will always stay around ~2.

The best choice of angle range may not be 10-60 degrees, as Colin has pointed out. One argument (and Colin may have additional ones) has to do with the smaller linear size sources (i.e., those w/ LAS < 10") we've excluded in our sample of 13. These have to be squeezed in somewhere, and fitting some reasonable fraction of them between 0 and 10 degrees might be hard (this all assumes, of course, that projected linear size has anything at all to do with orientation). In this case, a simple alternative might be to use a 20-70 degree range, which will still handle the prominence range and imply a somewhat lower gamma_j (maybe about 1.6). Colin, any further thoughts on this?

Perhaps I've placed too much emphasis on 10-60 degrees as "the best" in my recent messages; it IS for gamma_j=2, but if we let gamma_j be <2 ranges like 20-70, etc., also "fit".

-Dave

c: Colin

From abridle Wed Mar 23 16:11:40 1994
From: abridle (Alan Bridle)
To: dhough@physics.Trinity.EDU
Subject: Angle Range
Date: Wed, 23 Mar 1994 16:10:56 -0500

While attempting to put the actual words into Section 7.1 (p.79) of the paper as submitted, it occurs to me

(a) that we have already stated twice that we are assuming the jets and central features have a uniform distribution of intrinsic properties, to be intrinsically similar, in which case

(b) why don't we just do the calculation of the inferred gamma from an angular range that approximates the observed range of prominence (10 to 60) saying that this is how we chose the range. Doesn't the 20 to 50 range come pretty much out of thin air on p.79 anyway?

Given that we're rounding the answer to gamma ~ 2 in the end, couldn't we just use 10 to 60 at this point? I presume this would move our 1.8 up to 1.9, and so what?

I'd feel more comfortable with this approach unless you think it conflicts with something we've said elsewhere.

A.

From abridle Wed Mar 23 10:58:43 1994
From: abridle (Alan Bridle)
To: dhough@physics.Trinity.EDU, cjl@wells.haystack.edu, jburns@nmsu.edu, rl@rgosc.ast.cam.ac.uk
Subject: Draft of resubmission letter
Date: Wed, 23 Mar 1994 10:57:26 -0500

Here's my draft of the resubmission letter.

It should help you check changes in the text, to follow, and comments on it directly are of course welcome..

I suppose I should add a line about choosing to spell out our assumptions more clearly in the prominence analysis, without changing results?

===

I enclose a revised version of the manuscript "Deep VLA Imaging of Twelve Extended 3CR Quasars" by A.H.Bridle, D.H.Hough, C.J.Lonsdale, J.O.Burns and R.A.Laing (Paper Number 940023).

We thank the referee for a careful reading of a long paper and for supporting its publication. The referee points to several places where we also see room for improvement and have made changes. The few cases where we disagree with the referee and have not followed the comments are minor and are identified in our detailed response summary below.

Because the paper's length may deter some potential readers, we were receptive to detailed suggestions for shortening it. The referee, like us, could only find ways to "tinker on the margins" rather than to achieve substantial compression, however. We conclude that its length is driven by its content so we have restricted ourselves to dealing with the referee's explicit points, rather than with the generality that the paper is long. It is ironic that several of the referee's suggestions require small increases, not decreases, in length!

Introduction and Conclusions: We agree that this connection deserves to be strengthened, and have added text to the Conclusions to do so.

Counterjet Candidates: We agree with the referee that the point made in the abstract must be made elsewhere; this was an important oversight! We do not share the referee's "impression" that the correlation with jet bending adds to the case that counterjet candidates are real, however. Their reality as features of the radio sky depends on the integrity of our image processing, and their reality as counterjets must be judged by further observations that can distinguish them from lobe filaments, etc. Only then can model-making enter the picture productively. We therefore confine this revision to clarifying why we use the term counterjet "candidate" so extensively throughout the paper.

Redshift and angular scale conversion: The only redshift that is duplicated is that of 3C9, as we note a minor discordance in the literature. We wish to retain this. We also wish to describe the linear scales of features as we mention them throughout the text. The only way to respond to the referee's comment would therefore be to eliminate the "linear size" column in Table 1. This Table is the only place where the linear sizes are collected, and the space saved by this omission would be minimal. We therefore decline to follow this suggestion.

Relabeling knots: We do not wish to label jet knots "J1, J2" etc. We believe that the labeling should not prejudge what is a jet knot and what is a hot spot, or (for 3C68.1) which is jet and which is counterjet. We sympathize with the referee's wish to locate features in the contour maps without referring to the text, however. This can be achieved less prejudicially by brief additions to the Figure captions, and we have done so.

Omitting Images: We have carefully reviewed the need for every contour display. We disagree with the suggestion to omit Figure 7: we wish to record the slight possibility, alluded to in Section 4.4, that there is weak extended emission near the counterjet path in 3C175. In case there should be a rapidly-expanding counterjet in this source (as has been claimed for a few FR Class I galaxies) our low-resolution data from Figure 7 should be accessible to later observers. In some other Figures, we show whole-field contour maps to display the gross structure or the knot identifications but these maps suffer from contour crowding. We therefore used enlargements both to eliminate this crowding and to provide total intensity contours at the same scale as polarization vector plots. We agree with the referee that Figures 8b and 8c are the least crucial of these, and are willing to drop them. We do not wish drop any other maps, however.

Table Captions: We agree with this suggestion and have moved as much material as possible into Table captions.

AIPS tasks: We share the referee's lament that there is no comprehensive reference for all of AIPS. Our strategy has been to describe the functionality of the tasks briefly so that our image processing methods can be followed without detailed knowledge of AIPS. But it is also useful to refer to certain AIPS ingredients by name, as this will speed comprehension by any of the (several thousand) AIPS users who may read the paper in detail. The referee has caught one case (we believe it is the only one) where we failed to describe the functionality and gave only an AIPS task name. The problem is correction of polarimetry for Ricean bias. The referee's reference (a paper on optical photometry) will not clarify the issue much, however. We have instead added the standard reference to the statistical problem. We feel that we should still tell the specialist reader which debiasing-estimator we used (AIPS has several) by naming the AIPS routine explicitly. We have added the only explicit reference that we can find to the AIPS polarization correction algorithms and their merits. (We do not wish to discuss the algorithms at length in this paper). This reference is a VLA Scientific Memorandum, available from the NRAO, not to a published paper. This serves the referee's intent at minimal cost in added length.

drawspec: We see merit in telling the specialist readers that we did not use the (somewhat crude and inaccurate) one-dimensional fitting routines from AIPS to derive our jet-collimation plots and their errors. It costs little to state explicitly that we used the drawspec routines and we also wish to thank Dr. Liszt for supporting them. (Possibly the referee does not realize that drawspec is distributed and used outside the NRAO?).

Table 5: We now state the global error budget for the optical and radio measurements, and the individual errors in the one case where they are

crucial (3C351). As we can only give global errors for the radio data (beyond our statement of the reference positions in Table 3), we do not feel that it is worth enlarging and complicating Table 5 by restating the published optical errors individually.

Added figure: We agree with this suggestion and have provided an additional(!) figure.

Page 58, r: The referee was mistaken, the quantity was defined on p.55.

Page 63: We have followed the referee's final suggestion, that we refer directly to the Figure.

Page 66: We agree, and have added relevant words to the Conclusions.

Page 72: We feel that the term "assessment" generally includes the concept of limits as well as measurements, but we have added a few words to make this quite explicit.

Page 73: We agree, and have now given the numbers explicitly.

Yours sincerely,

==

From root Fri Mar 18 09:16:52 1994
From: Colin Lonsdale <cjl@dopey.haystack.edu>
To: abridle@NRAO.EDU, dhough@physics.Trinity.EDU
Cc: jec@dopey.haystack.edu
Subject: Prominences
Date: Fri, 18 Mar 94 9:15:23 EST

Gee, we've got some serious duplication of effort going on here! The plot you sent is a virtual copy of some of the ones Jennifer and I have been playing around with (albeit with a much bigger dataset).

My gut reaction is that this plot is a useful way, but by no means the only way, to show what kind of beaming parameters are needed to explain the correlation as presented. How credible is the postulated theta range of 10-60? Given that we have excluded small sources, and that our sources are lobe-dominated as a rule, do we really think the distribution extends all the way to 10 degrees? I thought angles like that were reserved for 3C345 and its kin. As you know, most of the prominence range for the core comes from the range of angles from 10 to 30. Some may feel that simply extending the range to 10 is "cheating" in a sense, to get the model to fit. What we really want is some independent way of guesstimating the theta range. Barthelization seems to be a way of constraining the upper end of the theta distribution. Perhaps the core dominated sources provide a constraint on the other end, on the basis that they must be much closer to the line of sight than any of our sources (in the beaming picture we are testing, at least).

I hope you see what I'm getting at. Theta is a powerful free parameter that should be wielded with caution. I believe some mention of the prominence range and its implications for the beaming hypothesis should go into the paper. I'm not yet convinced that beautified plots that appear to be an excellent fit best highlight the real issue, though might be persuaded if a justification for the 10-60 degree range other than "it fits" can be found.

Personally, I don't see what's wrong with saying that while beaming may well be the dominant cause of this correlation, it's probable that there is a substantial intrinsic prominence range in both quantities (correlated or not, can't tell). More data are needed ...

Cheers,
Colin

From root Thu Mar 10 08:53:21 1994
From: Colin Lonsdale <cjl@dopey.haystack.edu>
To: dhough@physics.Trinity.EDU
Cc: abridle@NRAO.EDU
Subject: Re: Prominence Problems?
Date: Thu, 10 Mar 94 8:48:40 EST

Dave, that rather puts things in perspective, I agree. If the intrinsic prominences of the core and straight jet are linked, as seems very plausible, a significant contribution to the prominence range from intrinsic scatter need not destroy the (very good) correlation. It may, however, adjust the slope significantly. Given the ranges you have calculated for the 13-source sample, it seems clear that we can make a reasonable case for doing our analysis, with beaming as the possibly dominant cause of the correlation. I would shut off the alarm bells as regards this paper. However, I don't think it'll fly in the long run because the prominence range is much bigger in your extended sample, as well as in mine. Eventually, any discussion will have to acknowledge that the intrinsic prominence range is large, probably dominant in some samples (Robert's narrow-line/broad-line comparison data, with the large core prominence range in the narrow-line objects, reinforces this view).

By the way, the approach Jennifer and I are taking on this one is to compare the data to model predictions instead of doing a simple regression (using York or anybody else's method) in log-log space. We think this will be a more powerful diagnostic tool. We should have something to show fairly soon, and I'll be sure to let you know what we find.

Colin

From root Thu Mar 10 00:36:08 1994
From: dhough@physics.Trinity.EDU (David Hough)
To: abridle@polaris.cv.nrao.edu, cjl@wells.haystack.edu, jburns@nmsu.edu, rl@rgosc.ast.cam.ac.uk
Subject: Prominence Problems?
Date: Wed, 9 Mar 94 23:20:14 CST

March 9, 1994
San Antonio, TX

Hello all,

My response to the issue Colin raised, and Robert commented on, concerning the range of prominences: there probably isn't really a whole lot to worry about for the paper.

Some numbers to consider: the range of central feature prominences (the real thing, not the log) is 284, and for the straight jets it's 44. Given this, the predicted ranges for simple beaming, and the accompanying ranges of intrinsic prominence that would be needed to match the observations, should be examined for various assumptions about orientations:

Table of Ranges

Orientation	Central gamma=5	feature intrinsic	Straight Jet gamma=2	intrinsic
20-50	22	13	10	4
10-50	113	2.5	18	2.5
20-60	41	7	18	2.5
10-60	214	1.3	33	1.3
20-90	160	1.8	79	none

So what do we want to unify? If you'd like central features and straight jets to have the same attractively small range of intrinsic prominence, you might vote for 10-50 or 10-60 degrees. The inner bound of 20 degrees forces one to accept more intrinsic scatter, and in the worst case (20-50) this does about half the work beaming does to match the observed ranges. I'm not terribly uncomfortable with this.

Implications for derived jet gamma? Well, for gamma_c=5 and gamma_j=2, some quick calculations show the following slopes for the log prominence plot:

Orientation	Mean Slope
20-50	0.73
10-50	0.60
20-60	0.78
10-60	0.65
20-90	0.86

Since these are all >~ the observed slope of 0.63, this suggests that for all these orientation ranges gamma_j <~2.

One more quick thing: yes, Robert, I had some thoughts like yours about ruling out high gammas. Here's a quick table of the predicted beaming ranges for undecelerated (gamma_j=5) straight jets:

Orientation	Beaming Range
20-50	55
10-50	464
20-60	126
10-60	1067
20-90	730

So if you've some reason to believe in an orientation range other than the one we're presently using in the paper, then undecelerated large-scale jets can be ruled out. But for 20-50 degrees, it looks like beaming alone could do the job (remember, we're after an observed range of 44).

Oh, I can't forget this: mind you in all of this that a gamma_j=3 always seems to buy you about the same beaming range as a gamma_c=5 (since alpha_j=0.6 and alpha_c=0). Just a handy reference point.

COMMENTS, SUGGESTIONS, OBJECTIONS?

-Dave

From root Wed Mar 9 17:40:09 1994
From: dhough@physics.Trinity.EDU (David Hough)
To: abridle@polaris.cv.nrao.edu, cjl@wells.haystack.edu, jburns@nmsu.edu, rl@rgosc.ast.cam.ac.uk
Subject: Hough's comments on AHB's proposed revisions of AJ "megapaper"
Date: Wed, 9 Mar 94 16:24:11 CST

March 9, 1994
San Antonio, TX

Hello all,

OK, here are my very brief comments on Alan's proposed response to the AJ referee on the "megapaper", as Colin has taken to calling it. I agree with nearly all that Alan has said, with the following minor adjustments:

(1) Intro. vs. Conclusions

I'm not sure calling Sec. 8 "Consequences" works, since Sec. 7 is already called that! I think it's simple to expand the first full par. on p. 91 (Sec. 8.1) by a sentence or so to be clear that (a) NO clear counterjets were detected; (b) counterjets are more likely to appear in sources with larger jet bends; and (c) NO counterjets are seen opposite straight, uninterrupted straight jet segments (all this, rather than the murky "circumstances" we now employ). So, still call it "Conclusions".

✓ excellent!

(2) Drop some images?

NO! Keep 'em all as you say, and defend to AJ.

(3) Optical & Radio position errors in Table 5?

NO! Do as you say, giving error only for 3C351 explicitly. For the Clements (1983) optical position given in Table 5, the errors are +/-0.011 s in RA and +/-0.08" in DEC (seems to easily nail down D as coincident with optical ID, but NOT C).

(4) P. 66 Last Par.

Again, return to my (1) above: just restate that jet-counterjet anti-correlation evidence is completely lacking, against what beaming predicts, in first full par., p.91 (Sec. 8.1).

(5) Appendix?

Nah, I don't see the benefits are worth the costs. Jack's right that maybe if we had done it this way originally it might have been somewhat preferable, but not at this point.

(6) Lastly, Colin's "new issue"

As I told Colin earlier, I played around with ranges of prominences, etc., preparing for the Socorro workshop last month. So I will have something to say about his and Robert's comments, but I've got to go now. I hope I can get to it tonight; if not, tomorrow morning.

-Dave

From root Wed Mar 9 14:40:04 1994
From: rl@mail.ast.cam.ac.uk (Robert Laing)
To: abridle@polaris.cv.nrao.edu
Subject: Re: Another idea
Date: Wed, 9 Mar 94 19:39 GMT

That's an interesting idea. It might create additional confusion because there are bound to be references to individual sources in the discussion, some of which presuppose that the reader has ploughed its way through Section 4. On balance, I think it would probably be a mistake, but I'm not very certain.

R

From root Wed Mar 9 14:35:36 1994
From: rl@mail.ast.cam.ac.uk (Robert Laing)
To: abridle@polaris.cv.nrao.edu
Subject: Re: AJ referee responses?
Date: Wed, 9 Mar 94 19:34 GMT

Dear Alan,
Just a brief note to say that I am entirely happy with the response you and Dave have suggested. Do you have the 3C 351 optical position errors, by the way.

Re Colin's point: I need to think about this. The intrinsic prominence is bound to have a scatter, and I think that we would need to ask how large a scatter would be needed to destroy the correlation, rather than saying that a range in prominence automatically implies a high gamma. A very small range in prominence would be more useful, because you could then rule out large gammas regardless of intrinsic scatter. One point which might be relevant: I looked at the core prominences for a complete subset of 3CR with z < 0.88 for which I had complete spectroscopic information, and found that the median values for high-excitation narrow-line and broad-line objects differed by about a factor of 10, despite the fact that the spread within each class was more like 100. A naive model would suggest that the narrow-line class should have a very narrow range of core prominence, so I think that much of it must be intrinsic.

Cheers, Robert

From abridle Wed Mar 9 14:19:02 1994
From: abridle (Alan Bridle)
To: dhough@physics.Trinity.EDU, cjl@wells.haystack.edu, jburns@nmsu.edu, rl@rgosc.ast.cam.ac.uk
Subject: AJ referee responses?
Date: Wed, 9 Mar 1994 14:17:36 -0500

Proposed responses to the referee's comments (not including Colin's point yet, we need to discuss that):

1. Introduction versus Conclusions

This is a fair comment versus Section 8 alone, as we went out of our way to summarize the purely observational "facts" earlier (in Sections 5 and 6). We could gather some more material into Section 8 as the referee suggests, but this may well lengthen the paper or break the earlier flow. Maybe a better alternative is: (a) to emphasize at the end of the Introduction that Section 6 summarizes our main observational results while sections 7 and 8 are mainly about interpretation and (b) to rename Section 8 "Consequences" rather than "Conclusions".

There is an argument for doing it the referee's way as this is a paper in which most people are particularly likely to read only the Introduction and the Conclusions!

2. Counterjet detection.

The referee's first point is a good one, we have not emphasized enough in the main text that the counterjet candidate sample as a whole fails to meet the BP jethood standard (we have discussed it ad nauseam for the individual sources, but it is indeed summarized only in the abstract at the moment). A logical place to add this is at the end of 5.2.1, and to emphasize that we use the term "candidate" throughout for this reason. We might also re-emphasize this at the start of Section 6.1.

The referee's second point is a bad one. We are being careful to distinguish purely observational "facts" and correlations from interpretation-dependent conclusions and I don't think we should say anything at all about whether the correlation with bending increases our confidence in the reality of the features themselves!

3. Suggestions for shortening:

3.1 (some repetitions in text)

(a) redshifts in text and table? I think this is true only for 3C9, and this is in the context of mentioning a small discordance in the published redshifts. I think this is o.k. as is, and will not shorten the paper by more than a sentence anyway.

(b) angular scale conversion to linear scale. It makes sense to give the linear scale when a signficant feature is mentioned in the text, i.e. to do the sum for the reader "in-line". We could eliminate one column of the Table by not giving the LLS in kpc but I don't see much gain from that.

(c) contour selection for polarization maps: unfortunately we cannot make one statement serve all because of the various subtleties of configuration-mixing and deconvolution - the present statements correctly identify the contours in each image, and I think this is worth doing so that the occasional careful reader does not find something inexplicable.

I therefore propose to ignore all three suggestions, but we can tell A.J. why in the covering letter. Overall problem if length of paper cannot be solved by tinkering at this level, we might as well keep the details accurate.

3.2 (relabeling all the components in the Figures).

Absolutely no way! This would not only involve redoing all the handwork on the diagrams but would also involve reworking the tables and dozens of references throughout the text. The referee's explicit suggestion would also prejudge which features in 3C68.1 are counterjet and which are jet, which in 3C351 is the central feature, etc. The referee's _point_ is one to which I am sympathetic, but the only solution I feel comfortable with is adding a sentence to each Figure caption to identify which knots we have considered to be part of the jet or counterjet, and which is our final "central feature".

3.3 (some images are not necessary for the message of the paper)

What has been done is to provide an I image at the same scale as every P image, and occasionally another I image to show the labeling. Where the I data are repeated, is it because the "labeling" image crowds contours or to keep a 'same-scale" I image next to the P data. We could toss out all the duplications, i.e. 8b, 8c (suggested by ref), 17a, 22b, 22c (suggested by Dave) with little effort but slight loss of clarity. Going through these with Jack, I felt the best cases for keeping in were 17a, which really helps the reading of 17b, and 22b, which greatly clarifies the knot labeling at the east end of the source). If we drop 22b, then we need to redo 22a to make the contouring less crowded and to allow the room to label feature J. 22a is already very congested as it is, and its main value is to show the whole source at once rather than to show the knot ID's.

Figure 7 is explicitly described in the text, the point being that it showed marginal evidence for a counterjet candidate while the full-resolution image did not. Dropping it would amount to dropping the slim possibility that there is extended emission along the counterjet track in 3C175. Not a big deal either way, but as it stands we leave the question a little bit open for any future 3C175 observers to deal with. I'm happy to defend that to AJ if necessary.

3.4 (Table captions)

We've provided quite a few but I'm happy to go along with the referee's suggestion that we move as much as possible into these captions. I need to redo the tables anyway as they are not in AJ's standard camera-ready format.

3.5 (references to AIPS, drawspec)

We suspect that the referee uses IRAF! The lack of references to AIPS' methods outside of the AIPS on-line help is a problem, I feel we have described the AIPS functionality briefly everywhere but the one case (COMB), and also that referring to the AIPS items briefly by name will help any of the (few thousand or so) AIPS users who might want to read the analysis section in detail. But the item on p.12 was indeed obscure: the reason being that we need to explain that we did not use the maximum-likelihood estimator (from AIPS task POLCO) but the actual method used in COMB is not the published Wardle-Kronberg correction either! The only place I can find the COMB correction documented outside AIPS is a VLA Scientific Memo by Leahy and Fernini (which points out that the AIPS correction is non-ideal for depolarization work).

I suggest that we refer to the standard work by Vinokur for the Ricean bias, and to Leahy and Fernini for the COMB correction. The Clarke and Stewart reference is to polarization correction in optical photometry, and wouldn't help the reader figure out what we did to deal with the problem.

I think it's helpful to assert that we did not use AIPS' brain-dead slice-fitting programs or their unbelievable error estimates, but instead used a particular software package (drawspec) that (a) does a better job, (b) is officially distributed, with documentation, from the NRAO -- just like AIPS, and (c) gives us the opportunity to thank a fellow scientist (Harvey Liszt) explicitly for his contribution. I don't think people who write software get thanked enough when we use the fruits of their toil, and I don't feel at all inhibited about pointing out where software has made a particular contribution to the detail in a paper.

4. Table 5

We discussed putting the errors in, but it makes the table messy and cannot be done on an individual-source basis for the radio data. Jack and I feel the best approach is to mention the global error budget for Table 5 in the text, and to give the optical position error for 3C351 (the only one where any science depends on it) in the text.

5. Sketch defining angles

This had come up before, and I think it is a good idea. Requires drafting a new Figure and will lengthen the paper, but I agree with the referee that this is worth it.

6. Using r before it's defined

Dave is right, referee is wrong. (No surprise!)

7. Refer to Figure 44a, not Table for correlation.

I agree with referee.

8. Page 66 last para.

   I think the referee means Abstract. not Introduction. It may have a little more prominence than it deserves by being in the abstract, given the problem of false correlation. Again possibly the problem is calling Section 8 "Conclusions"?

9. Page 72, last para.

   "assess" was supposed to embrace the idea of limits, not just measurements, we can say so explicitly.

10. Page 73, 3rd para.

    Good idea.

From root Wed Mar 9 17:40:09 1994
From: dhough@physics.Trinity.EDU (David Hough)
To: abridle@polaris.cv.nrao.edu, cjl@wells.haystack.edu, jburns@nmsu.edu, rl@rgosc.ast.cam.ac.uk
Subject: Hough's comments on AHB's proposed revisions of AJ "megapaper"
Date: Wed, 9 Mar 94 16:24:11 CST

March 9, 1994
San Antonio, TX

Hello all,

OK, here are my very brief comments on Alan's proposed response to the AJ referee on the "megapaper", as Colin has taken to calling it. I agree with nearly all that Alan has said, with the following minor adjustments:

(1) Intro. vs. Conclusions

I'm not sure calling Sec. 8 "Consequences" works, since Sec. 7 is already called that! I think it's simple to expand the first full par. on p. 91 (Sec. 8.1) by a sentence or so to be clear that (a) NO clear counterjets were detected; (b) counterjets are more likely to appear in sources with larger jet bends; and (c) NO counterjets are seen opposite straight, uninterrupted straight jet segments (all this, rather than the murky "circumstances" we now employ). So, still call it "Conclusions".

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NO! Keep 'em all as you say, and defend to AJ.

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NO! Do as you say, giving error only for 3C351 explicitly. For the Clements (1983) optical position given in Table 5, the errors are +/-0.011 s in RA and +/-0.08" in DEC (seems to easily nail down D as coincident with optical ID, but NOT C).

(4) P. 66 Last Par.

Again, return to my (1) above: just restate that jet-counterjet anti-correlation evidence is completely lacking, against what beaming predicts, in first full par., p.91 (Sec. 8.1).

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(6) Lastly, Colin's "new issue"

As I told Colin earlier, I played around with ranges of prominences, etc., preparing for the Socorro workshop last month. So I will have something to say about his and Robert's comments, but I've got to go now. I hope I can get to it tonight; if not, tomorrow morning.

-Dave

From root Wed Mar 9 10:36:50 1994
From: Colin Lonsdale <cjl@dopey.haystack.edu>
To: abridle@polaris.cv.nrao.edu
Cc: dhough@physics.Trinity.EDU
Subject: Re: reminder
Date: Wed, 9 Mar 94 10:33:09 EST

Alan, I do not have much in the way of specific responses to the referee comments, and right now I'm buried. However, here is a note I threw together a while ago but consistently left at home on a floppy. Sorry I didn't get it to you earlier, but it may be useful to you now. I know this is a bad time to bring up such a subject, but better late than never ... . Let me know what you think.

Colin

Alan and Dave,

I want to bring both of you up to date on a piece of work my student, Jennifer Carson, and I have been doing. While working on this I came to a realization of some significance to the megapaper. We have been extracting core, bent jet and straight jet prominences from the high redshift sample I have (the one with Peter Barthel), for the purpose of expanding the sample available for investigation of the correlation we found in the megapaper. Most of the work has been in accounting for differences in sensitivity, resolution, and "missing flux" problems when making comparisons to the well-observed sample in the megapaper. Recently, we decided that we had a pretty good handle on these effects, and started to think a bit harder about what our results were telling us, and how we should go about analyzing the relationship between core and straight jet prominence.

It was at this point that I realized the relativistic beaming model we have used to derive an "expected" slope for this relationship also, of course, predicts an absolute range of prominences. The non-zero range of prominences for a finite range of assumed viewing angles is supposed to be the primary cause of the correlation. If the correlation and its slope has anything to do with beaming, the range of prominences generated by the range of viewing angles had better be able to generate the observed range of prominences. Therein lies the problem. Even in the megapaper sample, the range of prominences is a factor of 100 to 1000, yet we derive our "predicted" slopes using an orientation range of 20 to 50 degrees, and this implies MUCH bigger Lorentz factors than we have been considering. Either that, or the inner parts of sources (cores, straight jets) just vary more rapidly than the lobes, and go up and down (intrinsically) effectively in concert. Data from the high-z sample extend the prominence range significantly (as did the extra 9 sources in your contribution to the Socorro workshop, Dave).

When Jennifer and I put together our paper, we will of course discuss these issues in depth, but I thought it was wise to alert you to this point now, while there is still time to throw a key sentence or two into the megapaper. There is of course a chance that the referee will also catch it. I am puzzled as to why this did not occur to me before.

From root Fri Mar 4 16:02:01 1994
From: dhough@physics.Trinity.EDU (David Hough)
To: abridle@polaris.cv.nrao.edu, cjl@wells.haystack.edu, jburns@nmsu.edu, rl@rgosc.ast.cam.ac.uk
Subject: Hough's comments on AJ referee's report on "Deep Imaging" paper
Date: Fri, 4 Mar 94 14:45:54 CST

March 4, 1994
San Antonio, TX

Hi Alan,

Thanks for the copy of the AJ referee's report on the "Deep Imaging" paper. While he/she makes of number of good points, none of them seems to criticize the data, analysis, or interpretation in any way. Frankly, I'm quite surprised, but I'm not complaining!

So on to the matters of "style" and small errors noted by the referee. The only points I have any disagreement with or additional comment on are:

(1) I think it's too much trouble to relabel all the features on the images. Just leave 'em as is.

(2) Following the suggested cut of Figs. 7, 8b, & 8c, there might be a handful more in the same category. Could we cut Figs. 17a, 22b, & 22c as well?

(3) I don't think placing ALL the optical (and radio) errors in Table 5 does much for anybody, but I agree that quoting the optical errors for 3C351 in Sec. 4.12 where we're arguing for D to be the central feature makes sense.

(4) The referee is wrong that "r" was not defined by the top of Page 58; it was clearly defined on Page 55, under point (c).

Otherwise, I am not for any massive cutting or re-writing to shorten or better organize the paper. I think that except for some of the obvious redundancies he/she points out that will help chop things down a bit, I'd leave it alone.

Seems like we had a very meticulous, unbelievably fair referee!

-Dave

c: Colin, Jack, Robert

From root Fri Feb 11 11:24:36 1994
From: Jennifer Carson <jec@dopey.haystack.edu>
To: abridle@NRAO.EDU
Cc: cjl@wells.haystack.edu
Subject: suggestion for paper
Date: Fri, 11 Feb 94 11:23:54 -0500

Hello,

I pointed something out to Colin yesterday about which he suggested that I write you. In your recently submitted paper, table 12(c) lists "rejected candidates" for hot spots. On page 62, in the discussion of the choice of prominence parameters, you say:

"...we normalize prominence measures by the _extended_ flux density of one or both lobes, i.e., by the integrated flux density of the lobe(s) minus that of any hot spot(s)."

The lobe flux densities you used subtracted out the rejected hot spot candidates as well as the "approved" hot spots, although the above wording seems to suggest that the rejected candidates were not subtracted. It is clear to me why you decided to subtract out the rejected candidates as well, to ensure that you are not normalizing to possibly-beamed emission. Perhaps you could clarify this in the final version of the paper?

regards,
jennifer carson

From root Mon Jan 31 15:57:31 1994
From: rl@mail.ast.cam.ac.uk (Robert Laing)
To: abridle@polaris.cv.nrao.edu
Subject: Summary of 20cm map status – my best guess
Date: Mon, 31 Jan 94 20:57 GMT

Alan,
These are the objects in question, I believe. The only one about which I'm unsure is 3C 277.3, for which the published map appears to be A array only (it's too early to appear in the archive). I guess that you know more about this one than I do. The remaining data appear mostly to have B and C array observations. I think that the s/n and coverage are adequate, but it is a bit difficult to tell from some of the plots.

So, I'm fairly sure that there isn't anything much requiring B array, but please feel free to add things. In particular, I'd value advice on 3c 277.3.

Summary of data

3C 111	B+C	APRW	OK
3C 135	B+C	Current proposal	
3C 184.1	B+C	Leahy & Perley	OK - ?jet
3C 192	B+C	RAL	OK - ?jet
3C 223	B+C	Leahy & Perley	OK - ?jet
3C 223.1	B+C	Spangler	Needs C conf ?jet
3C 277.3	A+?	Van Breugel et al.	? need B
3C 285	B+C	APRW	OK
3C 303	A+B	Leahy & Perley	OK
3C 321	B+C	APRW	Can't see bridge
3C 382	B+C	Leahy & Perley	OK
3C 388	A+B	Roettiger et al.	OK
3C 390.3	B+C	Leahy & Perley	OK
3C 403	B+C	This proposal	
3C 405	A+B	Carilli et al.	OK
3C 445	Too big to map at 8 GHz with VLA - ignore		
3C 452	B+C	RAL	OK

6cm construction config. A, B.
21cm A only (4hrs

APRW: some of Leahy & Williams (1984), Alexander & Leahy (1987), Leahy, Pooley & Riley (1986), which use overlapping datasets. All appear to have some C configuration data. The table in Leahy, Pooley & Riley gives the clearest account of the observing configuration.

Cheers, Robert

From root Mon Jan 31 10:11:35 1994
From: rl@mail.ast.cam.ac.uk (Robert Laing)
To: abridle@polaris.cv.nrao.edu
Subject: Re: VLA proposal
Date: Mon, 31 Jan 94 15:11 GMT

As I recall, we were thinking about B-array data at 1.4 GHz. I have put together something along these lines. We reckoned that 3C 135 and 403 would need B array: has anyhting changed that might affect this (e.g. these objects no longer being thought to have jets?)

Cheers, Robert

P.S. Any feedback about 3C 31?

From root Thu Jan 27 14:26:34 1994
From: dhough@physics.Trinity.EDU (David Hough)
To: abridle@polaris.cv.nrao.edu
Subject: Possible Alternative with N=21
Date: Thu, 27 Jan 94 13:14:21 CST

Alan,
 I will NOT allow myself to fiddle with numbers I got late last night, when I just called 'em as I saw 'em and left it at that. However, one fair alternative to the previous plot I sent is one that drops 3C190 and 3C191. Both sources have angular size 5", and are thus the smallest in the entire sample. The small number of beams across these sources made them very difficult to work with, and I have the least confidence in the results for them. So a plot omitting them follows.

-Dave

From root Thu Jan 27 14:01:42 1994
From: dhough@physics.Trinity.EDU (David Hough)
To: abridle@polaris.cv.nrao.edu
Subject: Further Cent. Feature–Str. Jet Prominence Data
Date: Thu, 27 Jan 94 12:49:17 CST

Alan,
I finally sat down last night to try a messy task I wasn't sure would be too fruitful, but it may have been. I did admittedly CRUDE measurments of central feature and straight jet flux densities off of various maps of TEN additional sources in the 3CR complete sample of 25 lobe-dominated quasars. In some cases authors provided hard numbers roughly consistent with what I was eyeballing off the maps, so I don't think what I've done is totally off base. Anyway, I got the raw numbers, I hope without terrible bias or errors of any kind, so that a cf-jst prominence plot can be done with 23 of the 25 sources; the two stragglers, 3C14 & 3C181, are the only ones for which I've never seen a VLA map. The results follow in a .ps plot file for your amusement. You will note that I choose to plot jst "A" prominence vs. cf "B" prominence, because I'm becoming more convinced that the "B" - "B" plot has the potential for inducing false correlations near unit slope, IF you always take away about the same fraction of cf flux and the addition of this flux to the jet then DOMINATES the jet emission (there will be more of a tendency for this to happen in sources at small orientations, where larger beaming factors in the central features might occur).
I'm pretty convinced I'd like to bring this up in Socorro at the workshop next month.

-Dave

From root Tue Jan 25 12:01:18 1994
From: dhough@physics.Trinity.EDU (David Hough)
To: abridle@polaris.cv.nrao.edu
Subject: Gratuitous Reassurance
Date: Tue, 25 Jan 94 10:49:06 CST

SA, TX
1/25/94

Alan,
I was putting a few thoughts together for the Socorro Workshop next month when it occurred to me that we had abandoned one of your earlier ideas about measuring jet emission: using the flux density per unit length, or "linear flux density density(?)". A few minutes work shows that the "A" measure of straight jet prominence, "normalized" by the length of the straight jet, is highly correlated with the standard "B" measure of central feature prominence (both prominences relative to extended jetted lobe emission): r=0.7495, P(r)=0.0032. And York's slope is 0.53+/-0.13, consistent with our other results.

Further, there is no correlation of fractional straight jet length (length of straight jet/central feature-jet hot spot distance) with central feature prominence (Fcf,jx,B): r=0.4472, P(r)=0.1255.

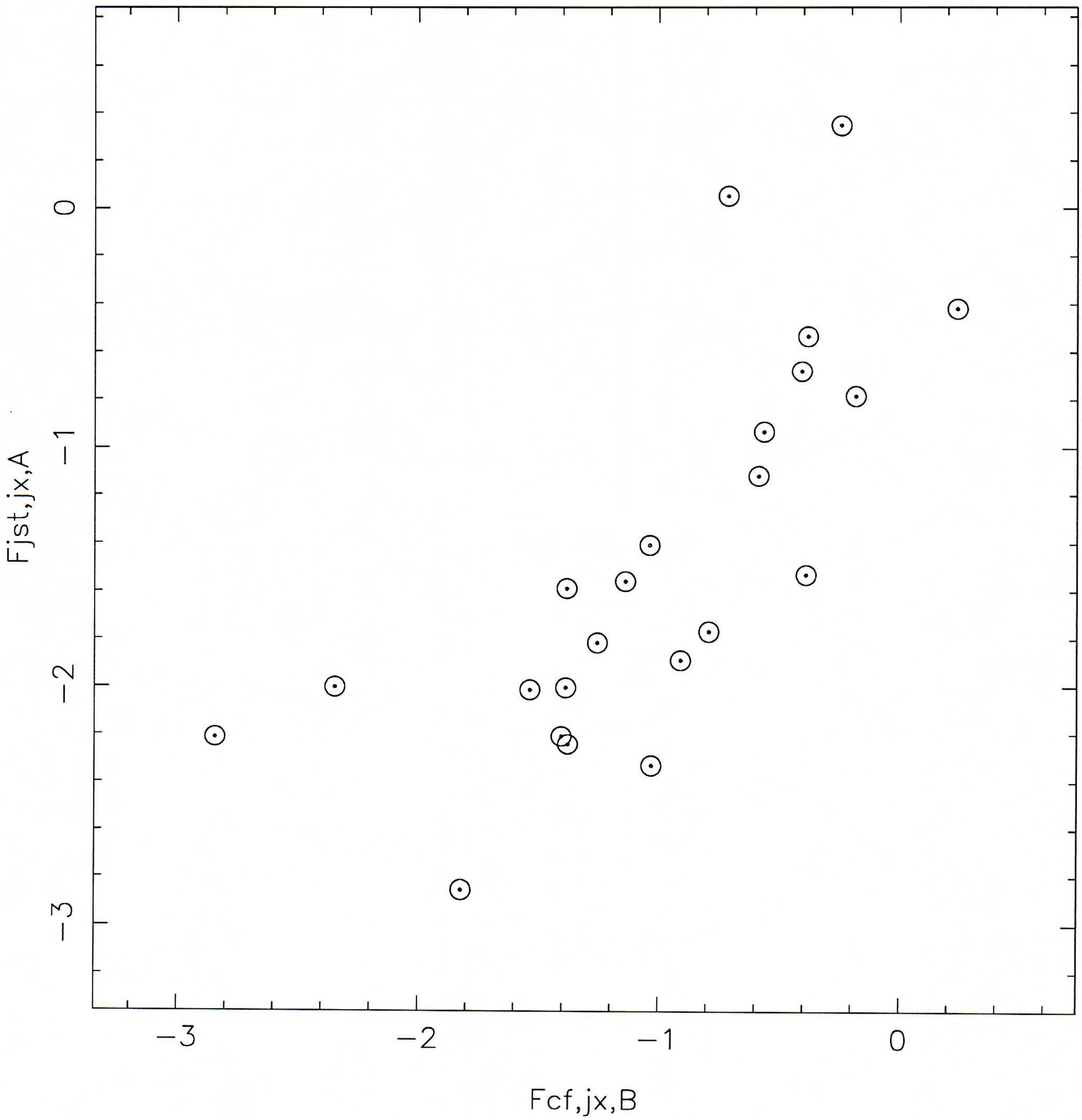
So my silly worry that prominent straight jets might be so by virtue of their lengths alone was unfounded. And we don't have to worry about mechanisms that might somehow have strong nuclei producing long jets, but in such a way that the length-normalized prominence remains ~constant. Such are the things that keep me awake at night!

Also for good measure, I note that Fcf,jx,B vs. Fjst,jx,A gives r=0.7500, P(r)=0.0032, and York slope 0.53+/-0.13. I point this out only because this test formally compares what we know to be on the mas-scale with what we know to be on the arcs-scale, and doesn't mess with the murky 10-100 mas stuff.

But none of the above really matters, so you might as well recycle this message.

-Dave

Prominence Plot, N=23, r=0.7352, P(r)=0.0001, York m=1.0+/-0.2



NATIONAL RADIO ASTRONOMY OBSERVATORY

520 EDGEMONT ROAD, CHARLOTTESVILLE, VIRGINIA 22903-2475

Dr. ALAN H. BRIDLE

TELEPHONE 804 296-0375 FAX 804 296-0278
INTERNET abridle@nrao.edu

January 17, 1994

Dr M.L.Norman,
Department of Astronomy,
University of Illinois,
1002 W. Green St.,
Urbana, IL 61801.

Dear Mike,

Here is the draft of the 3CR quasar imaging paper as it has been submitted to the Astronomical Journal. We will not be sending out general preprints until the paper has passed the refereeing stage, so please keep this version to yourself.

It was good to talk with you and Dinshaw at the AAS meeting. I am looking forward to seeing what can be done with the new relativistic-jet code. Maybe by the time of the meeting in Tuscaloosa you will be able to show how much of what we are speculating about here can really work!

I'll be in touch as soon as Mark Swain and I know when we will be getting the A-configuration 8-GHz data for 3C353. We will definitely plan to take up your suggestion of making the 8k by 8k images at Illinois!

With best wishes,

Alan

Move Appendix → original source discussion?

17a compare 17b was reason
22b, 22c drop 22c but not B?
Could expand fig captions to include some jet/cj knot details
— don't force the issue of where is jet/cj in C&I
Table 5 global error budget + 351 in detail

Referee's Report on Paper Number 940023

"Deep VLA Imaging of Twelve Extended 3CR Quasars"

by Bridle, Hough, Lonsdale, Burns and Laing

The authors present important new observational data concerning the nature of double radio sources, in particular dealing with the question of "one-sidedness" in quasar jets. Selection of objects, observations, data reduction are described in detail and the results are thoroughly discussed. I thus strongly support publication in the *Astronomical Journal* after revision as outlined below.

The paper is rather long and due to the wealth of details presented it is, however, not easy for the reader to focus on the important points. I thus urge the authors to shorten the text and also to make some changes concerning the structure of the paper.

- Introduction and Conclusions: Although I am perfectly happy with the Introduction, I believe the Conclusions do not pick up the points raised at the beginning (points a to c on page 6) sufficiently. In particular I miss a brief summary of the counterjet detections; the Conclusions should not only summarize the interpretation but also the observational results themselves.

(a) evidence of counterjets (b) jet detection rate & prom. statistics (c) systematic differences between jetted and cjetted lobes

- On the grounds of morphology **no** clear-cut counterjet was detected. This is said in the abstract but I think a similar remark in the text is missing. Also the discussion should touch that point. It is my impression that the strongest arguments for the reality of the counterjet *candidates* is the correlation found with jet bending (although it is not clear in general why a bent of the main jet should necessarily influence the opposite side).

the failure to meet the BP criteria is indeed not summarized elsewhere

I'm not sure we want to enlarge on this!

- In the following I would like to list a few things which to my opinion should help to shorten the text and make the paper more transparent for the reader:

  1. The descriptions of the individual sources repeat information, which is given in the table(s), e.g. redshifts, angular scale conversion to linear scale. This is not necessary. Also information

only for 3C9, and that's because it is slightly controversial

Not true but maybe Appendix would

not so, these are only in the text

$\sqrt{(p^2-\sigma^2)}$

but. not by doing anything about other ref is mentioning?

which is the same for each object does not have be repeated every time (e.g. polarization maps on *selected* contours). In general I think this part could be shortened quite a bit.

2. It would help the reader to compare maps without reference to the text if the different features in a source would be labeled similarly and not just in strict alphabetical order (e.g. why not always call the central feature **CF**, the jet knots $\mathbf{J}_1$ ...?). This would especially help to quickly find the counterjet candidates.

relabeling every figure, table and text ref is non on. Can identify them in the captions?

3. Some images are not necessary for the message of the paper and — in view of the large number of figures — should be omitted (e.g. Fig. 7 does not give more information than 8a, the enlargements 8b and 8c are also not necessary). All images should be checked in this respect.

Fig. 7 included because it showed a poss. c-jet feature. Could drop 8b & 8c.

4. The authors generally do not supply table captions. I think it would make the text more readable, if the descriptions of the individual columns in the text were cut out and put into table captions. This would also make the use of the tables easier, as one does not have to search the text for the interpretation of columns and symbols.

OK need to make Tables conform to AJ pattern anyway

Vinokur 1965 Ann d'Ap. 28, 412

Contains a reference for the Ricean bias, but not for how we deal with it

based on Wardle & Kronberg (1974) ApJ, 194, 249.

5. As long as AIPS and its commands is not documented in a way it can be referenced, I would not make it a prime reference in the data reduction part without further description. This is only helpful for insiders but frustrating for outsiders having no opportunity to find out what was happening. Naming the AIPS task in brackets should be sufficient for the insider. E.g. on page 12: for COMB one could give a general reference, e.g. Clarke, Stewart (1986) in Vistas in Astronomy **29**, pages 27–51. On the other hand commands like SLICE, TVSTAT, BLANK are sufficiently trivial, so mentioning them explicitly does not seem necessary. Similar arguments hold for S. Liszt's *drawspec* program.

we haven't, that's why there is always some extra description.

We are in each case mentioning task in course of describing an explicit data analysis strategy. The AIPS docs are only reference and how else to tell people where to look? or how to reproduce our work.

would remove approx ≡ one sentence from paper and simply make ~~exclude~~ harder for AIPS-literate readers to see what we have done!

Let me finally point out a few special things with reference to their location in the text:

- Table 5: Errors for the optical positions would help. E.g. in the case of 3C 351 the identification of feature D and not C with the central feature also involves knowledge of the accuracy of the optical position!

but not quite – see Leahy and Fernini VLA Scientific Memo. 161 (1989).

- A sketch showing the definition of the different angles η (and perhaps also Ψ and χ) would be helpful (Tables 7, 8 and 11). Although indices c and l are used in the text, they are not used in Table 7, causing probable confusion. ok?

- Page 58 top, r is not (yet) defined here. r was def. first on p. 55 (c)

- Page 63 last paragraph: Shouldn't it read "Table 19" instead of 20? In this case I was looking at the table first to find the strong correlation by eye. Only while reading on I saw the reference to the Figure. I guess this might happen to others also, so it may be better to start like " ... Figure 44a shows a strong correlation ...". ok

- Page 66 last paragraph: According to the Introduction (abstract?) this appears to be one of the major observational conclusions of the paper. As such it seems to me not to be stressed strong enough (e.g. not been repeated in the Conclusions).

  Add sentence to Conclusions

- Page 72 last paragraph: The first sentence sounds strange: If no counterjet is detected, no integrated flux ratio can be given (only lower limits as in Table 6).

  interpreted ratios or limits rather,

  "assess" included "set limits". Maybe we can reword.

- Page 73 third paragraph: Could a reference for the typical power in jets and lobes of FR I sources be given here?

  I we should give numbers here, too.

From root Tue Jan 11 17:09:10 1994
From: dhough@physics.Trinity.EDU (David Hough)
To: abridle@polaris.cv.nrao.edu
Subject: Page charges
Date: Tue, 11 Jan 94 15:58:08 CST

Alan,
Our dean says there is no problem in ULTIMATELY paying the $800 or so, but that there may well be a problem prior to June 1. So assuming that some arrangement, amongst ourselves or involving AJ, can be made that will not require Trinity's share until June 1, we're in business. Sorry I can't deliver the cash immediately.

-Dave

From root Sat Jan 1 16:48:28 1994
From: rl@mail.ast.cam.ac.uk (Robert Laing)
To: abridle@polaris.cv.nrao.edu, cjl@wells.haystack.edu,
dhough@physics.trinity.edu, jburns@nmsu.edu, rl@mail.ast.cam.ac.uk
Subject: FINAL comments
Date: Sat, 1 Jan 94 21:48 GMT

Gentlemen,

Sorry not to have replied earlier: I have been away and have just got the new version of the paper. You will doubtless be relieved to know that I only have a few minor quibbles. Other discussion can, as you say, be left for later.

Small bits and pieces:

last line of 5.1: delete respectively

p37, para 2: the reference to Laing (private communication) should be to Argue, A.N. & Kenworthy, C.M., 1972, MNRAS 160, 197.

p73, para 1: a more appropriate ref than Laing (in prep) is: check ref w. RAL

Begelman, M.C., 1993 in Jets in Extragalactic Radio Sources, eds R\"{o}ser H.-J. & Meisenheimer, K., Springer Lecture Notes on Physics 421, p 145.

Sec 6.3, 12. I prefer ... rapidly and then recollimate.

pp79 and 84, strictly speaking the formulae are only valid for emission that is isotropic in the rest frame, I suppose. Tends to strengthen the argument on p 79 if it isn't, since n increases. Probably not worth complicating matters.

Sec 7.1.1: reference to models of inhomogeneous jets: Laing 1993 in Astrophysical Jets, eds Burgarella, D., Livio, M. & O'Dea C.P., CUP: Cambridge, p 95.

p85, para 2. I'd prefer replacing "If the shocks ... are highly oblique" with "If relativistic flow is important at the hot spots", since I think the argument is more general than currently stated.

p92, para 3: "finitely asymmetric" sounds wrong to me: why not just "asymmetric"?

The only new bit of text about which I am doubtful is the argument at the top of p 89. I don't believe that the definition of "secondary fine structure" is precise enough to make a strong statement. I have never understood how you cope with the case when the hot-spot is too weak to qualify under our definition. For example, why isn't the whole of 3C 334 B+C "secondary fine structure" with the primary missing? The structure function analysis was done to try to get round this lack of precision. However, in the interests of finishing things off, can I suggest shading this paragraph a little, e.g.:

"A further difference that could be explained by this model is the ~~possible~~ strong trend of hot spots that are associated with secondary ~~fine structure to be~~ on the jetted side (Lonsdale 1989): the ongoing collimated .... would then occur only on the currently active side of the source."

I wouldn't be livid if the para went off as is, though.

I can't quarrel with the sentiments expressed in 8.1.

Perley et al p.258-260
Subarcsec RA ed R.J. Davis & R.S. Booth CUP 1993

Remaining comments are for later discussion and/or replies to Alan's requests for comments:

Pelletier/Sol reference: I'm not bothered, although they might be.

Depolarization asymmetry: I am not convinced by the counter-arguments, but since the point is a bit off the theme of the paper, I'm content to leave it out. I certainly don't believe that a thin layer outside the source is consistent with the Cygnus A data. You need very high densities (of which there is little sign in X-rays) and fields. In any case, I would expect the field to be amplified by compression, and it cannot be transverse to the line of sight everywhere in front of the lobe, so the depolarization could easily be more on the jet side. The sound speed argument is much stronger for internal thermal matter than for a thin layer, but still causes some trouble since the speed of restructuring of the layer has to be slow.

Oblique shock models of hot-spots: we have made the point that supersonic post-shock flow is allowed if the shock is oblique, and Norman & Balsara have shown that re-nozzling occurs. I'm not prepared to speculate on exactly how fast the flow will turn out to be, since my intuition isn't up to it, but there is no physical reason why relativistic flow is prohibited. Let's wait for Mike Norman to do things properly.

Compactness asymmetry vs core strength. Now that is a good argument. It connected with a train of thought about compact steep-spectrum galaxies. We found some evidence that these are heavily reddened quasars, in the sense that they have strong broad H alpha, but very weak broad H beta. 3C 68.1 is described as a "red quasar" somewhere in the literature, and there is evidence for a fair amount of reddening in 3C 351 too. What these objects have in common is that they have unusually weak radio central features for their optical spectral type (they really stand out on a histogram of core prominence), they are highly asymmetric in their radio structures (intensity, separation ratio, compactness or all of the above) and they are very red. They certainly don't fit well in Barthel's picture. Don't know what this means, but it seems worth following up. I might well sign up to intrinsic asymmetry's being dominant in these cases.

Happy New Year,
Robert

From root Thu Dec 30 10:53:31 1993
From: Colin Lonsdale <cjl@dopey.haystack.edu>
To: dhough@physics.Trinity.EDU (David Hough) (David Hough)
Cc: abridle@NRAO.EDU
Subject: Re: Final matters...
Date: Thu, 30 Dec 93 10:51:14 EST

Dave, I owe you a vote of thanks for having the energy and patience to follow through on my unquantified assertions. I am of course gratified that my eyeball assessment of the bent jet ratios is borne out by the real data. I fully expect that by using the two free parameters of changing angle to the line of sight in the bent jets, and correctly chosen values of jet deceleration with associated increases in beaming angles (with assumptions about our orientation distribution), one could explain all this away in a beaming model. I have to say, however, that this is one more piece of evidence (like the hot spot asymmetry) that fits completely naturally into an intrinsic model, but extremely awkwardly into a beaming model. I think your new numbers are hard enough to merit the last minute inclusion of a couple of sentences to that effect.

Colin

From root Thu Dec 30 10:33:11 1993
From: dhough@physics.Trinity.EDU (David Hough)
To: abridle@polaris.cv.nrao.edu, cjl@wells.haystack.edu
Subject: Final matters...
Date: Thu, 30 Dec 93 09:23:12 CST

SA, TX
12/30/93

Dear Alan:

(1) OK, I see the error of my ways yesterday on the bent jet-bent counterjet issue. Yes, the three detected bent counterjets are all fainter than the corresponding bent jets, and then obviously the ten non-detected ones are also fainter than the clearly detected bent jets opposite them! So it is indeed perfectly honest to say the bent jet is ALWAYS brighter than the bent counterjet. Jolly good that we're stating this point clearly now.

(2) Related to (1), Colin raised the issue of how strong a correlation we would expect between central feature and bent jet prominence if beaming were responsible for the bent jet/bent counterjet flux ratios. His hunch was that we'd expect a lot more than is actually seen (zilch), because he felt that the bent ratios are larger than the straight ratios. This is actually true! Now it's a little messy because of the well-known problem of just how to identify a counterjet, never mind its straight and bent pieces separately, but if I do a little approximating where necessary to get at least some crude measure of the bent ratio, I find:

not included

?	Straight Ratio	Bent Ratio	Bent Ratio Per Unit Length
Lowest	1.2	4.4	3.7
2nd lowest	3.3	5.1	5.1
Median	12	21	21
2nd highest	52	197	111
Highest	175	570	713

How much of this is upper limit ratio?

Note that the third column is necessary, since the bent lengths are not generally the same on both sides. What I make of all this is that the bent ratios are roughly something like a factor of two larger than the straight ratios, so indeed you would need stronger beaming factors to explain the bent ratios with beaming. So either the explanation of the bent ratios isn't beaming, or else somehow the bending of the beamed jets is messy enough to decouple their prominences from those of the central feature while still allowing them always to be brighter than the bent counterjets.

(3) In going through this, I realized a mismatch between counterjet fluxes for 3C9 in the text vs. Table 6. The text on p. 39, line -8, has 0.31 mJy, but this should be changed to 0.36 mJy to match the table.

(4) If you're going to standardize on "Source" for a column heading in all Tables, then Tables 15 & 16 need this as well (currently no heading at all for the source column).

(5) If you're going to put the h-dependences in Tables 10 & 13, do

we need to look at doing this for Table 1 (linear size) and Table 17 (power)? We could either (a) tack a note onto Table 17 saying Ho=100 was used as in Table 1; or (b) we could put the h^-1 and h^-2 in the Table 1 & 17 column headings, respectively, and alter the Table 1 note to say Ho=100h rather than simply 100.

OK, NOW I think that's all I have. Any word from Robert or Jack?

-Dave

c: Colin

From root Wed Dec 29 16:50:56 1993
From: dhough@physics.Trinity.EDU (David Hough)
To: abridle@polaris.cv.nrao.edu, cjl@wells.haystack.edu
Subject: Straight Counterjets
Date: Wed, 29 Dec 93 15:40:52 CST

SA, TX
12/29/93

Dear Alan:

OK, I've done some fiddling with the "straight counterjet" issue, and, as expected, don't really have anything alarming to report. But please note the following:

(1) The bent jet prominence actually correlates better with that of the straight counterjet than that of the integrated counterjet (e.g., for normalization by total extended emission, r=0.58 vs. 0.49).

(2) Nothing of significance for central feature (r=-0.04) or straight jet (r=0.05) vs. straight counterjet prominence.

(3) IF you normalize jet/counterjet side bits by their own lobes, THEN there are absolutely NO hints of any correlations. The only one we are partial to - bent jet vs. integrated counterjet - drops to r=0.27. MAY have some bearing on what we say in this regard at the end of the j-cj prominence paragraph.

(4) One more curiosity: I checked all the "self-prominences", i.e., I ran the prominence of each bit with extended jet lobe normalization against its prominence with extended counterjet lobe normalization. My list includes cf, jst, jbt, cjst, cj, jh, and cjh. All have a clear correlation with r>=0.68 (P<=0.01) EXCEPT:
  (i) cjst (straight counterjet), which misses slightly at r=0.63 (P=0.02) and doesn't worry me.
  (ii) jst (straight jet), which misses by a mile at r=0.47 (P=0.11) and DOES worry me. Are we being sent a signal of some kind by this?

With all this said, I'm NOT agitating for a single change in the paper as it stands. Your choice of the adverb "materially" lets us get away with item (3), I believe; and item (4-ii) may just be a fluke, one which we have really addressed in our discussion of the weakening of the cf-jst prominence correlation when switching from jx to cjx normalization.

THE BOTTOM LINE: DON'T CHANGE ANYTHING IN THE PAPER, UNLESS SOMETHING IN THE DETAILS ABOVE REALLY BOTHERS YOU!

-Dave

c: Colin

From root Wed Dec 29 12:04:45 1993
From: dhough@physics.Trinity.EDU (David Hough)
To: abridle@polaris.cv.nrao.edu
Subject: HOLD ON A MINUTE...
Date: Wed, 29 Dec 93 10:54:53 CST

One terribly obvious thing occurred to me just this morning. When discussing counterjet prominence, we have ALWAYS used the integrated flux density (or limit thereto) for the counterjet. But of course we should treat straight and bent counterjets separately, to the best of our ability. Now I don't think one new iota of astrophysics will come out of this, but let me do the checking today and send results to you for consideration.
This popped out at me, by the way, when I was re-reading some of Colin's e-mail and thinking about the new point that has been emphasized in the last draft concerning how the bent jet pieces tend to be brighter than the bent counterjet pieces in the same source. Yes, it's true, but there are only three sources with "detected bent counterjet candidates" with which to back this claim: 3C215, 3C334, & 3C336. Just don't want us going overboard on the basis of ONLY THREE SOURCES!!!

From abridle Tue Dec 28 12:52:04 1993
From: abridle (Alan Bridle)
To: dhough@physics.Trinity.EDU, cjl@wells.haystack.edu, jburns@nmsu.edu, rl@rgosc.ast.cam.ac.uk
Subject: Two more small things
Date: Tue, 28 Dec 1993 12:51:39 -0500

I have two more small changes to suggest.

(a) p.90, last sentence of middle paragraph.

Amend to "decouples their apparent emission from that of the inner, presumably straighter, parsec-scale jets".

(b) I mentioned previously that in Tables 10 and 13, I would add the h-dependencies of the derived quantities. I believe that these are:

B_{eq} --- $h^{2/7}$

p_{min} --- $h^{4/7}$

t_{syn} --- $h^{-3/7}$

xi --- $h^{-4/7}$

Please let me know a.s.a.p. if you think otherwise!

A.

From root Tue Dec 28 14:24:19 1993
From: dhough@physics.Trinity.EDU (David Hough)
To: abridle@polaris.cv.nrao.edu
Subject: Two more small things
Date: Tue, 28 Dec 93 13:14:34 CST

```
A.,
(a) If you meant p.80, not 90, then it's OK.
(b) Yep, I get exactly the same h-dependences!
                                                   -D.
```

From root Tue Dec 28 12:48:41 1993
From: dhough@physics.Trinity.EDU (David Hough)
To: abridle@polaris.cv.nrao.edu, cjl@wells.haystack.edu, jburns@nmsu.edu, rl@rgosc.ast.cam.ac.uk
Subject: Submit the paper!
Date: Tue, 28 Dec 93 11:38:37 CST

SA, TX
12/28/93

Dear folks:

I've just read Colin's two messages and Alan's one. I agree with Colin in both of his: first, that the paper is ready to be submitted, and second, that his p. 92, line -6 correction is needed.

I've got a very short list of little things I caught on my all-nighter last night, but I don't feel I need to see yet another draft with these changes made - I'll leave it to Alan to tend to them or ignore them as he sees fit. And I second Colin's motion - outstanding job, Alan, pulling it all together!

The Very Short List
----

(1) p. 56, line -5: should be a blanket referral to Section 7, not just 7.2.

(2) p. 60, line +2: "filter in that IT depends only..." also Sobel-filter measure, fix 2nd "it"

(3) p. 65, line +7: "central" typo, as Alan caught.

(4) p. 65, line +10: again, I used the MEAN error ratio, not the median, to get the 0.63+/-0.12. If you really prefer the median strongly for some reason, we'd better re-calculate.

(5) p. 81, lines +7 & +9: just use "quasar(s)" instead of suddenly introducing "QSR(s)".

(6) p. 81, line +8: "Our estimate differs from these in that (a) it..."

(7) p. 84, lines +10-11: I still must insist that we change this to something like "If the fraction of the material flowing with speed parameter in the range BETA to BETA+dBETA is f(BETA)dBETA, then our estimate..."

(8) p. 85, line +6: "asymmetry" typo, as Alan caught.

(9) p. 90, line -8: "milliarcsecond" typo, as Alan caught.

(10) p. 92, line -6: "...may therefore need to introduce further free", as Colin caught.

(11) p. 99, Norman et al. ref.: no period at end.

(12) p. 104, Fig. 9 caption: Move the last sentence about the vector scale to just BEFORE (a) & (b), to be consistent with how it's done in all

other captions.

(13) Tables 10/13: pressure units don't bother me, h-dependences would be nice but I won't cry if you skip it.

(14) "B" flux density table: yep, it should be the new Table 17 and the present 17-21 become 18-22. DO!

THAT'S IT! I'll be around the rest of the week if any other major items of business arise, but as I said, I'm happy to see it go in before the year is out WITHOUT having to review a further draft.

Jack, Robert, will you have a chance to circulate your comments today or tomorrow?

-Dave Hough

From abridle Tue Dec 28 11:13:28 1993
From: abridle (Alan Bridle)
To: jburns@nmsu.edu, rl@rgosc.ast.cam.ac.uk
Subject: Minor language changes/corrections
Date: Tue, 28 Dec 1993 11:12:59 -0500

```
Minor language changes suggested by AHB:

p.36, Sec.5 line 3  "all of" --->  "all"

p.41, line 3  "This case is the opposite of" rather than
              "This situation is the opposite of that in"
      line 5 from end   Hyphenate "re-gridded"

p.68, full para. 2:

      Replace "The situation for the counterjet prominence" by
      "The dependence of counterjet prominence on jet deflection"

      Delete "presently" from next sentence

p.71, line 1  Replace "may, however, indicate" with "suggest, however"

p.73, line 1  Replace "An example of such a situation is"  with
              "Consider, for example,"

p.79, line 14  Replace "the slope of which" with "whose slope"

p.82, line 3  Replace "All of these"  with "These expectations all"

p.86, line 4  Replace "are initiated" with "originate"
      line 6  Replace "demonstrate" with "show"

p.87  full para.1, last sentence:
      Amend to "Our data support the idea that interactions with the
      large-scale environment indeed modify jet properties significantly.
      Models that attempt to ascribe the observed asymmetries
      entirely to {\it asymmetries} in these interactions must however
      seek to explain this coupling to parsec scales."

p.88, line 7  Replace "as observed in many cases" by "as often observed"
      line 9  Italicize "compact"

p.91, full para. 2, line 1  Replace "needs further modification" with
      "must be modified"

p.92  line 4  Replace "are initiated" with "originate"
      line 6 from end   move "need" ahead of "to introduce"
      line 5 from end.  Replace "parameters.  These will" by
                                "parameters that"
                        making one sentence of two.
```

From abridle Mon Dec 27 16:08:23 1993
From: abridle (Alan Bridle)
To: dhough@physics.Trinity.EDU, cjl@wells.haystack.edu, jburns@nmsu.edu,
rl@rgosc.ast.cam.ac.uk
Subject: Last "road map" (I hope)
Date: Mon, 27 Dec 1993 16:07:50 -0500

```
Summary of changes in current draft of quasar paper.
~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~

Note (a) that 3 small points (at end) remain unsettled and (b) I also
expect to do more proofreading myself.  This version comes to you
"early" to maximize time for you to mull over the larger-scale changes
made in response to the points from the last set of comments from Dave
and Colin.  Robert -- I'm hanging on to the previous version in case
you're using its page-references for your comments, but it will be
better to use this version for your final round if possible.

(.ps file follows separately, owing to length)


** MAIN POINTS **

p.37

last para   Rewritten to include all VLBI references and the proper motions
            previously reported in Section 4.

p.61

end         Text on why we use prominence rewritten slightly, moved to front of
            section 5.5

(p.62        Minor changes in wording of first para, continuing above)

p.64

para.2      updated throughout to correspond to new 90,000-trial case with lower
            flux density limits of zero in numerators.

p.65

para.1      modified to include other flux-flux correlations as fourth line
            of evidence that correlation is not indiced ny normalization

p.66        rewritten to include survival analysis results and the
            lack of correlation between central feature and counterjet prominenc
es

p.77

Sec 6.5.1   modified to include the point about bent jet segments being
            systematically brighter than outer counterjet candidates

p.81

first full para.   _is_ new, in response to request from dave
last para.         ditto
```

p.82

full para rewritten to take in Colin's point that jet bending does not remove side-to-side asymmetries completely. The important point here is that bending does not completely symmetrize the situation, so some beaming effects must still be present, albeit with different statistics owing to the reorientation of the flows.

p.83

line 12 point about high obliquity made explicitly as Colin suggested

p.84 extra parentheses in equation

p.85

lines 1-4 all statements made relative.
full para.1 is new
full para.2 is modified in light of above

p.86 The division into two model classes remains, because the first class really does seem to be missing an important ingredient.

p.87

full para.1 The "fatal flaw" wording has gone (we only put it there to make sure that Colin would speak up by the deadline!) and its paragraph is replaced by new text that still points out what is missing in the large-scale asymmetry model. Some of us believe this _is_ a fatal flaw, but we now have "kinder, gentler" (G.H.W.Bush 1988) wording about the issue!

p.88

last para. Has been enlarged along lines suggested by Colin.

p.90

full para.2 I've dropped the part about the depolarization asymmetry as Colin has a wriggle, whose plausibility we could probably debate for a while but we can leave that for somewhere else. (Robert, this was your issue originally, are you o.k. with this?) I don't think there's a similar wriggle for the optical problems, so I want to leave them in.

Section 8 I'm trying to merge the tone of Colin's new text with what was previously said. You'll need to read this from scratch to judge, whether this is working as the tone of the two contribution epochs was rather different.

-- minor points --

There are a few (very minor ones) that go unmentioned here because I did them on the fly without marking up my previous text. But this should be most of them:

p.2/3

Abstract, para 6: First two sentences compressed

p.3

second para, last two lines: reworded so "large bend" applies only to jetted hs.

p.4

full para.1 "dominates" replaced by "helps to determine", to account for
last line the point about bent jet segments still being brighter than
counterjet candidates

p.9

line 16 delete "at both frequencies", leftover from 1.4 GHz data inclusion

p.18

last line, south-west and north-east interchanged

p.20

last line, amended to Figure 10 (no a, b now)

p.22

line 6 from end contour interval 75 microJy/beam
line 2 from end Figure 14a

p.25

line 3 from end selected contours of total intensity, not from Figure 20

p.30

line 7 lobe designations now south-east, north west

p.45

line 2 added "apparent" to make this explicit
line 9 deleted "polynomial"

p.47

para.2 added h-dependences

p.54

lines 2 and 3 from end added h-dependences

p.58

line 5 now points out dependency on 3C68.1, 351
para 2 lobe extent ratio examples added

p.63 5.5.2 title includes counterjets

p.65

lines 10-13 modified to use error ratio and 'expected" slope

p.67 slight rearrangement of para.1 and 2 to emphasize conclusion

p.69 5.5.4 intro modified to include counterjetted features

p.71

6.1, line 5 added h-dependence

p.75

line 3 now says "absent"

p.79

line 5 "perhaps all" made explicit

p.80

line 5/6 explicit recapitulation of correlation and slopes for comparison
line 10 Dave's final slope estimate used

p.87

7.2.2 line 3 Robert has suggested a Pelletier/Sol reference be added here but I could not find one that referred to exactly what we are discussing. Will stay dropped unless Robert or someone else can come up with an actually appropriate one.

p.94

line 6 added h-dependence of power

Refs. Hough et al 1993 updated
Isobe et al. added
LaValley et al. added
Vermuelen et al. updated
York moved

Fig. caps. 14 modified for Jack's grey-scale
41 added h-dependence of size limit
44 and 45 changes as Dave requested.

Items still to do, or consider
~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~

Tabulate flux densities of "B" option for cores/jets (presently embedded only in the luminosity table). I'm not sure where best to put these; is it worth a new Table 17 and moving all the others now >= 17 up in number by 1?

Jack was bothered by pressure units in Table 10/13. Anyone else concerned? Reason for these is that X-ray folks often quote densities in m^-3 so these pressures are easy to copare with their results. But if we were being consistent, we would then have B in nT, not gauss!

Add h-dependence in Tables 10/13. I _will_ do this.
~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~

Cheers, A.

P.S. If we don't finalize by January 3rd, it will be the end of January before I can get back to this for any length of time. So deadline for more comments etc. is 1994!

From abridle Wed Dec 22 14:54:17 1993
From: abridle (Alan Bridle)
To: Colin Lonsdale <cjl@dopey.haystack.edu>
Subject: Re: Flux-flux correlations
Date: Wed, 22 Dec 1993 14:54:13 -0500

I see where we disagree. I believe that in order for _all_ the feature, luminosities to be constrained to within a small range, as a picture of a "standard" source would assert, there must be underlying correlations between the feature prominences. As that is what we are trying to test, that must not be where we start.

The point of view I am starting from is that just because we have a group of sources that are similar in extended luminosity, there is no a priori reason to suppose that their central features and jets will be standardized in _luminosity_. If they are, then we can infer that prominence restrictions are at work. So, instead of arguing from the idea that there is a "standard" of central feature or straight jet luminosity that might falsely correlate the fluxes, I am arguing from the idea that in a random world we might find sources in our sample whose central feature, or straight jet flux densities might _independently_ range down to zero. Thus the test case is one in which they do, although the lobes don't.

The fact that we find no "coreless" and no "jetless" sources in this sample is then a _result_ whose significance we can assess. So is the appearance of a correlation between the jet and central feature flux densities in a sample that was "standardized" mainly on lobe luminosity, not directly on either jet luminosity or central feature luminosity. It is the statistical significance of the limited range of, and partial correlation of, the flux densities and prominences of the central features and cores, but not of anything else, that I am trying to get at.

In my view of the problem, one only starts to ask questions about "standard" central features and jets in sources standardized by lobes _after_ buying the result that these quantities really are all correlated, and after inventing specific models to try to account for this fact.

So I think we're coming at this from diametrically opposite directions (nothing new in that, but some need still to explain to each other how we're thinking).

I'm going to propose that we use all of our arguments for significance of our reult on p.64 -- the statistical significance of the prominence correlation relative to the fully random trials (98%), the fact that no other prominence correlation reached this significance level, the fact that the flux densities directly correlate, the fact that no other flux density pairs correlate this well, and the fact that the slope of the relationship as estimated by the York method is well below unity. I guess you may still be dubious about one of the five unless the words above have convinced you that it is relevant after all. Maybe you can live with that even if so?

Cheers, A.

From root Wed Dec 22 14:28:15 1993
From: Colin Lonsdale <cjl@dopey.haystack.edu>
To: abridle@polaris.cv.nrao.edu (Alan Bridle) (Alan Bridle)
Subject: Re: Flux–flux correlations
Date: Wed, 22 Dec 93 14:26:58 EST

```
>
>
> Colin Lonsdale writes:
>
>  > .... we know of a mechanism which will generate a trivial correlation for
>  > these flux parameters.
>
> I'm not sure we do.  The redshift will try to correlate the
> luminosities even if the flux densities are random.  You are looking
> for the inverse, a correlation in the flux densities because there is
> some standard luminosity?  But the c.f. and jet luminosities will only
> be standardized over a limited total power range if there is an
> underlying prominence correlation (which is what we are after!).
>
> What am I missing about your argument?
>
> A.
>
```

Alan, I refer only to the fact that our sources display a range of flux densities. If we believe to any extent that such a thing as a "standard source" exists, with intrinsic ratios of flux densities of various parts in a non-infinite range, then if you have a sample with a big enough spread of total flux density, the different parts of the sources will correlate, independent of ANYTHING else, redshift, luminosity or what have you. The core and the jet in Cygnus A are both stronger (in Janskys) than some anonymous 5C source, regardless of their relative luminosities or redshifts. This must be going on in our sample, so we can't jump up and down too much when we see a correlation. As I said, though, we have a handle on the extent of the effect from Dave's (to my mind very surprising) lack of other flux-flux correlations, which suggests that the core/straight jet flux-flux correlation has some significance after all.

Colin

From abridle Wed Dec 22 13:44:06 1993
From: abridle (Alan Bridle)
To: Colin Lonsdale <cjl@dopey.haystack.edu>
Subject: Re: Flux–flux correlations
Date: Wed, 22 Dec 1993 13:44:01 –0500

Colin Lonsdale writes:

> .... we know of a mechanism which will generate a trivial correlation for
> these flux parameters.

I'm not sure we do. The redshift will try to correlate the luminosities even if the flux densities are random. You are looking for the inverse, a correlation in the flux densities because there is some standard luminosity? But the c.f. and jet luminosities will only be standardized over a limited total power range if there is an underlying prominence correlation (which is what we are after!).

What am I missing about your argument?

A.

From root Wed Dec 22 12:14:18 1993
From: dhough@physics.Trinity.EDU (David Hough)
To: abridle@polaris.cv.nrao.edu, cjl@wells.haystack.edu, jburns@nmsu.edu, rl@rgosc.ast.cam.ac.uk
Subject: The continuing saga of the cf–jst prominence correlation
Date: Wed, 22 Dec 93 11:04:41 CST

SA, TX
12/22/93

Dear folks:

OK, to augment yesterday's flux density correlation results, here's the whole works for central feature, straight jet, bent jet, extended jet lobe, jet hot spot, counterjet, counterjet hot spot, and extended counterjet lobe flux densities, all run against each other. The MOST significant result is STILL the central feature-straight jet one, with only counterjet hot spot-extended counterjet lobe even coming close (no others break the 95% confidence level). So I think this IS evidence of some connection between central feature and straight jet strength, and I don't see any problem in continuing to say so in the paper. Perhaps we should put it in perspective and say it is easily the most significant of ALL the flux density correlations. I'm absolutely for it and am somewhat mystified as to how we began to doubt ourselves so seriously on this.

OUTPUT TABLE OF LINEAR-CORRELATION COEFFICIENTS

	Scf,B	Sjst,B	S,jbt	Sjx	Sjh	Scj	Scjh	Scjx
Scf,B	1.0000	0.6301	-0.2950	-0.0084	0.4948	0.1527	-0.0209	0.4334
Sjst,B	0.6301	1.0000	-0.3203	-0.1100	-0.1405	0.1797	0.0559	0.5237
S,jbt	-0.2950	-0.3203	1.0000	-0.2739	-0.1216	-0.1713	-0.0779	-0.4314
Sjx	-0.0084	-0.1100	-0.2739	1.0000	0.3654	-0.1114	-0.1968	-0.2675
Sjh	0.4948	-0.1405	-0.1216	0.3654	1.0000	-0.1395	-0.2318	-0.2267
Scj	0.1527	0.1797	-0.1713	-0.1114	-0.1395	1.0000	0.3827	0.4033
Scjh	-0.0209	0.0559	-0.0779	-0.1968	-0.2318	0.3827	1.0000	0.5842
Scjx	0.4334	0.5237	-0.4314	-0.2675	-0.2267	0.4033	0.5842	1.0000

OUTPUT TABLE OF PROBABILITIES

	Scf,B	Sjst,B	S,jbt	Sjx	Sjh	Scj	Scjh	Scjx
Scf,B	0.0000	0.0210	0.3279	0.9783	0.0856	0.6186	0.9459	0.1390
Sjst,B	0.0210	0.0000	0.2860	0.7206	0.6471	0.5569	0.8562	0.0662
S,jbt	0.3279	0.2860	0.0000	0.3652	0.6924	0.5759	0.8002	0.1410
Sjx	0.9783	0.7206	0.3652	0.0000	0.2195	0.7172	0.5194	0.3770
Sjh	0.0856	0.6471	0.6924	0.2195	0.0000	0.6494	0.4460	0.4563
Scj	0.6186	0.5569	0.5759	0.7172	0.6494	0.0000	0.1968	0.1718
Scjh	0.9459	0.8562	0.8002	0.5194	0.4460	0.1968	0.0000	0.0360
Scjx	0.1390	0.0662	0.1410	0.3770	0.4563	0.1718	0.0360	0.0000

Further, I don't think extending the central feature and straight jet fluxes down to zero changes things very much. To back this up, I did my "mini-trials" again with this change, and I still get 3% of the correlations coming up with r>0.83 just as before. Yes, the correlation coefficients and slopes bounce around a little, but on balance nothing changes, and I don't particularly see why we should change the present text at all in this regard.

-Dave

From root Tue Dec 21 10:47:50 1993
From: Colin Lonsdale <cjl@dopey.haystack.edu>
To: abridle@NRAO.EDU
Subject: Some text
Date: Tue, 21 Dec 93 10:46:40 EST

```
Alan, I thought you would prefer this hurriedly prepared snippet now to a more
polished text quite a bit later.  I have to do urgent software modifications
for geodesy at least an hour ago.  I haven't thought about exactly where sentime
nts
such as this should be inserted.

Colin

-----------------------------------------------------------------------
As may be anticipated with the advent of more detailed observational constraints
,
the simplest forms of the three models we have discussed have proved to be inade
quate.
A simple, constant-velocity, Doppler boosting model cannot easily be
reconciled with hot spot asymmetries and correlations between hot spot prominenc
e
and jet bending.  Asymmetric dissipation models in their purest form have
difficulties with, for example, hot spot recession trends.  The simplest form
of intrinsic power asymmetry, a pure flip-flop, appears untenable due to the
incidence of compact and evidently active hot spots on the counterjet side of
some sources.

Instead, it seems clear that the correct explanation of the data and trends we h
ave
presented lies in substantially modified or merged variants of these models.
The degree of Doppler boosting is probably a strong function of distance from th
e
nucleus, and velocity structure across the jet may play an important role.  If
asymmetric dissipation is important, it probably originates on small scales near
the nucleus, and modifies the flow parameters to an extent sufficient to
influence the gross morphology of the resulting lobes.  If the jets are intrinsi
cally
asymmetric, at any given time the power ratio between the two sides is
probably not infinite.  Other properties of the flows, such as velocity or
collimation, may also be asymmetric.  Such asymmetries may be induced far from t
he
nucleus by small-scale asymmetric dissipation of an initially symmetric pair
of jets.

Acknowledging the need for refinement of the models, we note that the addition o
f
such free parameters dramatically reduces their testability.  We suspect that fu
ture
work should be aimed at establishing which class of model generates the dominant
effects, rather than which specific, detailed model is correct.
```

From abridle Mon Dec 20 10:32:38 1993
From: abridle (Alan Bridle)
To: dhough@physics.Trinity.EDU, cjl@wells.haystack.edu, jburns@nmsu.edu,
rl@rgosc.ast.cam.ac.uk
Subject: Re: Comments on the draft
Date: Mon, 20 Dec 1993 10:31:14 -0500

Colin Lonsdale writes:

>
> On page 64, line -8, the distribution of extended fluxes is cited as the only
> reason that our analysis is likely to be too conservative, but that's not rea
lly it
> at all. The whole point of the prominence parameters is the idea that we hav
e a
> mixture of strong and weak sources here, and prominences remove this dispersi
on in
> source strength (caused by distance or intrinsic differences in power), leavi
ng
> only power-independent quantities. A false correlation will arise in these p
rominence
> parameters only from departures from source strength scaling. In other words
, if
> our 13 sources span, say, a factor of 10 in intrinsic strength, that is track
ed perfectly
> by the extended emission (because, e.g., it's unbeamed), the observed distrib
ution
> of extended emission strength has nothing whatever to do with false correlati
ons,
> and we can just use the r-values uncorrected. So, I have been arguing that t
hese
> false correlation simulations have been "worst-case" because I strongly suspe
ct that
> a large part of the observed extended emission dispersion is due to strong so
urces
> being strong, and weak ones being weak. No way to prove it, though.
>

I agree that we should probably say more about what the _good_ aspects
of using prominence data are, i.e. why we are concerned about
power-normalization. We may be emphasizing the negative aspects of
the common normalizer too exclusively. (I'm a little amused that it
was Colin who first brought up the purely statistical problem, maybe
he was a bit too good at convincing us!).

> That brings me to the bottom of page 64. I thought we had dispensed with thi
s flux
> density/flux density correlation as trivial, precisely because we have a rang
e of
> source strengths here. Bright ones are bright, faint ones are faint, and bin
go you
> have a correlation. How can we ever prove otherwise if we don't try and norm
alize
> by using prominences? Unless, of course (and I forget, with all the flying e
mail) the
> flux-flux correlation mentioned is the only one among all the parameters, in
which
> case this fact is what should be mentioned, not the mere fact of an apparentl
y

 > significant correlation. Having said all this, I'm still quite happy with th
e strength
 > of our correlation.
 >

I'm not quite sure what Colin is suggesting here. Do you want to drop all mention of the flux-flux correlation, Colin? Our present text points out that it is _part_ of the evidence that not all of the apparent correlation between the prominence parameters comes from the common normalization.

Comments from others needed here

Another way of looking at this problem occurred to me over the weekend. Would it be correct to say that if the apparent prominence-prominence correlation came _entirely_ from the normalization, then the statistics of our observed sample should resemble those in a sample whose flux densities are randomized between ZERO and our largest observed values, not between our lowest flux densities and our largest? Because the flux densities used in our simulations are confined to the range we actually observed for central features and straight jets, the limited range already contains some effects of the real-world correlation between central feature and jet properties. E.g., if Colin will excuse me for pointing this out again, there are no sources here with detected jets but no detected central features. In a fully random world, there could be.

This is a sense in which the our comparison with the simulations is indeed "too conservative". Would a random trial assigning flux densities from zero to the upper limit be a more legitimate comparison?

 > On page 73, the first paragraph has me confused. I have the feeling that it'
s been
 > explained to me before, but are there really folks who entertain for an insta
nt the
 > idea that these FRII sources are really just FRI sources viewed in a funny wa
y? Why
 > is this paragraph needed? I don't get it.

Steer

Nope, it's like it says -- evidence that the jets in FR II's are intrinsically more luminous (not just higher Mach-number) than those in FR I's. I don't think anyone, us included, was suggesting that this difference might be due to orientation.

 >
 > Page 78, lines 7/8, we refer to "some portion" of the jet, conveniently forge
tting that
 > the entire jet is pretty darned asymmetric. You must play the same game betw
een the
 > straight and bent jet segments as is done between the pc and kpc jet scales.
 Whatever
 > produces one asymmetry must be related to whatever produces the other, since
they are
 > always on the same side. But now we are starting to argue that the bent jet
segment
 > is dominated by interaction. Its prominence certainly appears to be, true.
Has anybody
 > stopped to think what degree of bent jet prominence correlation with core pro
minence you
 > expect if beaming in the bent segment is still enough to generate the degree
of asymmetry

> between bent jet and bent counterjet? My strong hunch is that you expect a w
hole lot
> more than you see. In many cases the limits on straight jet/straight counter
jet are
> markedly smaller than those on bent jet/bent counterjet, I think.

mention this problem.

"Some portion" obviously includes "all" as a possibility. I think
it's o.k. where it stands as we are talking about what the _model_
expects. The point about the bent-jet versus central feature (and
counterjet candidate) prominence should probably be taken up somewhere
lower down in this section, however. I take it, Colin, that you'd
like to see a gamma_j estimate for the bent parts as well?

>
> Page 79, lines 9/10, a bit hard to read without stating what the correlation
coefficients
> change **from**.

O.K., I'll restate these from p.64 and 65.
>
> Page 81, line -10, I would insert ", given high obliquity," between possibly
and
> relativistic.

Fine.

> The oblique shock model of hotspots must tread a thin line between oblique en
ough to
> maintain post-shock beaming, but not so oblique that the resulting feature is
nothing
> more than a jet knot. We even make this distinction explicitly when defining
> hotspots in section 4.1. I think this line may be so thin as to be invisible
... many
> of these hotspots are enormously bright, and have no sign of a well-collimate
d flow
> beyond them. What makes the terminal shock so different from those that prod
uce
> jet knots if it is so oblique that you have high gamma beyond them? See also
my
> reservations about quantification of the inhomogeneous jet model below.
>

Any comment, Robert?

> One of the several correlations noted in section 5 that is left
> basically unused in later sections is that linking the hotspot
> compactness ratio to the core power, sec. 5.3, r=0.83 (i.e. very
> good). This trend, for strong core sources to have small asymmetries
> in hotspot compactness, runs exactly opposite to what should be a
> strong prediction of the inhomogeneous jet model. Isn't this so?
> Just for the record, an intrinsic asymmetry booster like myself would
> think in terms of disk (i.e. Mach disk) shaped hotspots on the active
> side, and more spherical blobs on the less active side because of a
> weaker terminal shock, lower densities, longer synch. lifetimes, more
> diffusion etc. Then a source close to the line of sight (strong core)
> would show the active disk hotspot face-on and big, while the weak
> core sources would show them more nearly edge on, and small. Hence
> the correlation.

I'll add this to Section 7.2 unless anyone can shoot it down. Seems a
good point to me.

>
> Page 83 line 1, I find the word "decollimating" to be strangely placed.

Just reminding people that this flow is likely to have a broader range of angles than the incoming one. Anyone else have a problem with this?

> The statements on lines 4 to 6 are excessively sweeping in their claims. What does
> "suppressed" mean? By how much? Counterjet hotspot emission would not be "entirely"
> from sheath material, depends on the post-shock Doppler factors.

I'm not sure these statements need to be a sweeping as they are. It's just another case of Doppler "favoritism", so less black-and-white language could be used.

>
> Splitting up section 7.2 is OK, but 7.2.2 highlights what I see as a general problem
> with sections 7.2 ands 7.3. The distinction between the models is, as stated right
> at the start of the paper, somewhat artificial. Trying to rigidly maintain the
> separation between the models is bound to lead to lots of charging through wide open
> doors. Obviously, since high gammas abound in the cores, nobody but a fool would go
> around claiming that beaming has no significant influence on parsec-scale appearance.
> The end of 7.2.2 is an example. In general, any asymmetry that originated on pc scales
> and is big enough to alter the emissivity of the jet by factors of 100 and more is
> also big enough to dramatically change the sound speed in that jet, and therefore its
> mach number. So your asymmetric dissipation model on small scales becomes something
> much more on large scales, perhaps taking the two jets into completely different flow
> regimes. Then you have no problem at all dealing with recessed hotspots. If you have
> an unrealistic model such as in 7.2.2, it's easy to dismiss it. However, we cannot
> in all fairness dismiss a model in which the only basic asymmetry is in the dissipation
> on pc scales, for the above reasons. This sort of thing always brings a picture of
> Hercules A to mind .. were those two jets once symmetric? Sure ain't now!
>

What in particular are you suggesting we do here, Colin?

> The main problem I have with section 7.3 is in a similar vein. There are all sorts of
> repeated references to the "strict" flipflop model, which frankly I thought was always
> a complete non-starter (and have said so in print, many years ago).
> As I pointed out some time ago, a time-dependent power ratio
> is the only thing to consider, unless we broaden the definition of "intrinsic

asymmetry"
> to include composition or other differences between the jets. If we do that, it merges
> with the type of model put forward in 7.2.2. I see no point even discussing an obviously
> inadequate model, of which the strict flipflop is one. This inadequacy is used repeatedly
> to paint a picture of disfavour for the whole class of intrinsic asymmetry models.
> An analogous treatment of section 7.1 would be to constantly point out what does not
> fit with a constant-gamma jet from core to hotspot. The last paragraph on page 87 is
> the most objectionable in this regard.
>

I don't agree, I encounter quite a few folks who need reminding why strict flip-flop is a problem. But you make the case for pointing out the survival capabilities of models with time-dependent power ratio. Care to draft a paragraph that says exactly what you want, Colin?

> I'd like to point out that the sound speed in the lobe is likely to be close to c/root3,
> which means that as a newly invigorated jet enters it, the influence of that jet (shocks,
> stirring etc) will be felt at the sides of the lobe at about the same time that the
> jet reaches the hotspot. The explanation ofthe depolarization asymmetry in this type
> of model is that the lobe is surrounded by a sheath of potentially depolarizing material,
> which is ineffective when the lobe is fed by the stronger jet (shearing, and compression
> due to lobe expansion de-emphasises line-of sight field? Electron content drops because
> of shock-induced condensation and recombination in sheath? Something else?). The
> material is spatially correlated with the lobe, and may well be a thin sheath. As such,
> it could respond to the presence of the active jet on the same timescale as the hotspot.
> I.e. if you admit an intrinsic asymmetry explanation for the depol asymmetry at all, I
> think you have to acknowledge that it can respond quickly.
>

I thought the Cygnus data made the thin-sheath model extremely unlikely because of the high densities required. But this is an extreme case. Robert, you were keen to see the depolarization-asymmetry argument go in here, any comment on Colin's suggested wriggle?

>
> By the way, since we drag in the depolarization asymmetry, why not also drag in the
> spectral index asymmetry? As we now know (right, Alan?), this is not fully explained
> by the hotspot asymmetry, but extends into the lobes. This is a bigger problem for
> the beaming model than depolarization is for the intrinsic asymmetry model.
>

Um, Peter asked us not to introduce the spectral asymmetries into this paper as they are to be the topic of a later one with different authorship. Basically the answer is that the low-brightness levels have the same spectral asymmetry as the radio galaxies (short side has steeper spectrum), but the high brightness levels have a jet-correlated spectral asymmetry. The matter of whether or not the jet-correlated asymmetry extends far enough beyond the hot spots to be a problem is still being studied. Beaming could produce such an symmetry in the presence of a curved spectrum (blue-shifted side looks flatter), but the extent of the asymmetric regions still needs to be pinned down. Problem is that the literature confuses the two effects (Liu and Pooley sample has, by chance, an excess of sources with the jet on the long side, so over-emphasizes the asymmetry). Because that asymmetry isn't as well-documented as the depolarization asymmetry, and the documentation will come from another work we have in preparation, it seemed reasonable to leave it aside for the moment.

I'll go ahead and make small changes along lines suggested explicitly above, but I suggest that Colin draft an extra para for 7.3 and Robert comment on Colin's points re shock obliquity and depolarization flip-flop before we finalize.

From root Fri Dec 17 16:24:06 1993
From: dhough@physics.Trinity.EDU (David Hough)
To: abridle@polaris.cv.nrao.edu
Subject: More comments on your comments...
Date: Fri, 17 Dec 93 15:15:08 CST

SA, TX
12/17/93

A.,

OK, on your latest message, I think I only need say the following:

(1) Go ahead and leave the beaming bits in where we first present the weak jet-counterjet prominence correlation. The correlation naturally fits here, and the slight dissonance will probably not be noticed by readers, even those who aren't tone deaf.

(2) All right, I'll quit being a soldier for Tony and let slip SR79 as a gamma~2 ref., but I still think we should provide some history here. I know we have the first results based on proper dissection of jets into straight & bent bits, etc., but we ain't the first to say and try to defend low gammas on large scales. Maybe at least mentioning your ARAA article results (or quote any references therein that you might find suitable) would serve this purpose.

(3) There was a reason I didn't want to provide a central feature-counterjet prominence number, since it means adding a column in Table 18 for Fcj,cjx (I note, however, that such a column is easily accommodated and provides a nice symmetry: 3 rows of 6 columns each!). So for Fcf,B,cjx vs. Fcj,cjx, we have r = +0.08, with the following to be added to Table 18 for Fcj,cjx:

need to ASURV these.

3C 9	-2.433
3C 47	-1.812
3C 68.1	-1.365
3C175	-2.927
3C204	-2.753
3C208	-1.717
3C215	-1.681
3C249.1	-2.473
3C263	-2.485
3C334	-1.995
3C336	-2.025
3C351	-2.619
3C432	-3.379

Cjet prominence may decrease with increasing core ratio hs?

Other requests?

-D.

From root Fri Dec 17 15:40:23 1993
From: dhough@physics.Trinity.EDU (David Hough)
To: abridle@polaris.cv.nrao.edu
Subject: Your comments on my comments
Date: Fri, 17 Dec 93 14:31:27 CST

OK, I'm basically happy with your message today. I have a slight discomfort with just exchanging "found" for "expected" in the false correlation slope sentence, but I can live with this based on what my feeling for the uncertainty in the expectation value probably is (a few %). I was quite serious on my last message being the FINAL WORD; what seems to be left is still the final compromise on what Robert and Colin want some of the interpretation to be on beaming vs. intrinisic asymmetry. Today I tend to think that Colin has a very valid point that we should be careful about making strong statements ruling models out; as he has demonstrated in some of his recent creative messages, there's probably some scheme that can be dreamed up to salvage almost any random model! With that said, I'm not going to be too picky about details of exactly what variations of the three broad classes of model get discussed, as long as they are presented with "for" and "against" evidence appropriately and we let the "voters" decide if a model is clearly the absolute truth or total rubbish. I'll be around for any last-minute items if next week is still a real possibility for getting the paper off in the mail...

-Dave

From root Thu Dec 16 09:08:38 1993
From: Colin Lonsdale <cjl@dopey.haystack.edu>
To: abridle@polaris.cv.nrao.edu, dhough@physics.Trinity.EDU, jburns@nmsu.edu, rl@rgosc.ast.cam.ac.uk
Subject: Dave's message
Date: Thu, 16 Dec 93 9:07:56 EST

Question - can we explain?

I wanted to quickly point out, while it's fresh in my mind, that Dave's point (1
0)
about hotspot recession actually fits quite naturally into an intrinsic
asymmetry model. I can think of two mechanisms, with examples. First, any
reinvigorated jet will typically have to re-excavate a channel out to the lobe,
and at any given time of observation simply may not have made it all the way to
the edge of the source (this is analogous to the beam wandering so often invoked
for splatter spot models, except that in the intrinsic asymmetry model it is
an inevitable consequence of the model, rather than the result of an added
free parameter). The prototypical example is, of course, 3C249.1.

Add ✓

The other mechanism that springs to mind is the double hotspot phenomenon. As
pointed out in the literature, the primary hotspots of double hotspots tend
to be very compact and bright. The implication is that you don't get sufficient
post-hotspot flow collimation to form a secondary hotspot unless the primary
is really active. Thus, ongoing flows (which will often generate more "lobe"
beyond the hotspot) will tend to occur only on the more active side of the sourc
e.
Various sources come to mind, perhaps most notably 3C204 and 3C351. It's hard
to envisage an ongoing flow beyond the southern hotspot candidate in 3C351
generating anything analogous to the northern "fan" emission responsible for
substantial "recession" of the northern hotspot. It's simply not active enough
because it's not currently being fed with a similarly energetic beam.

Add ✓

Dave also mentioned the core power/hotspot brightness asymmetry correlation, and
suggested that the hotspot collimation/brightness ratio one is due to a relation
ship
between gamma and jet collimation (sounds reasonable physically). I suppose
you could say that poorly collimated jets spray their core flux into a bigger
solid angle, and the cores therefore appear brighter on average, because the
well-collimated cores are Doppler diminished. More free parameters, though.
Since we know (from VLBI) that compact hotspots are ridgelike, I still prefer
the intrinsic asymmetry explanation of disk-shaped active hotspots as in my
initial message.

But we see recollimation on large scales!

We could concoct explanations for all kinds of things, for each model, ad nausea
m,
and the paper would never get submitted. I'd like to reiterate my sentiment
that we back off the model development aspect, point out only broad issues
for the models without favour (in particular remove statements that certain mode
ls
have fatal flaws or others clearly fit everything), and leave the model
building to other papers.

I'm glad Dave seems to be happy with my suggested revisions. How does everybody
else feel?

Colin

From abridle Fri Dec 17 15:40:16 1993
From: abridle (Alan Bridle)
To: dhough@physics.Trinity.EDU, cjl@wells.haystack.edu, jburns@nmsu.edu, rl@rgosc.ast.cam.ac.uk
Subject: Re: Hough's FINAL COMMENTS on paper as of 12/15/93
Date: Fri, 17 Dec 1993 15:39:41 -0500

```
Dave said:

 >
 > (1) Abstract, pp.2-3:  We correctly mention that counterjet candidates
 > are found in sources with a wide range of central feature prominence and
 > projected linear size.  I only note that this seems never to be stated
 > explicitly in the body of the paper (although I may easily have missed
 > it).  I hardly think this is critical, just thought I'd point it out.
 >

Yup, only the brief mention of the central-feature prominence relation
on p.80 had survived.  As mentioned in my previous message, I suggest
we put the material back in 5.5.2.


 > (2) p. 25 last line, p. 26 first line:  "..on selected contours of
 > total intensity."  This is one where the contours on the pol. plot
 > are NOT same as any on the intensity plot (Fig. 21 for 3C249.1).

 done

 >
 > (3) p. 38, first paragraph:  I think the VLBI sidedness stuff is
 > OK here, but it might be slightly preferable to tack it on to the
 > end of each source's bit in Section 4 (or opening of Section 5 for 3C47),
 > as we've done for the superluminal results.  It would be very simple for
 > the four sources for which we've already done this - 3C47, 3C204, 3C263,
 > and 3C334 - since the superluminal papers also discuss the sidedness.
 > For 3C208, it would be a "Hough (unpublished)" ref.; for 3C249.1, it
 > would be a "Hough (1986)" ref.

I think it's useful to collate the sidedness result for all 6
somewhere, and this has to be done after we introduce 3C47.  So it
may be that the neatest strategy is to serve up all of the
sidedness and proper motion results, with their references,
under "The central features" in Section 5.1.  I'll draft it that
way and see what you think.

 >
 > (4) p. 56 & p. 58:  We discuss correlations of Colljh with Qhs and
 > Qlob (the latter of which I think is dominated by the hot spot
 > contributions anyway).  For Qhs on p. 56, we make light of it because it
 > supposedly depends so strongly on 3C68.1 & 3C351.  However, the
 > significance drops only to 97% without them, just as it does for
 > Qlob on p. 58 (where we don't mention any 68.1/351 caveats).  So
 > either we mention the 68.1/351 catch for both of them, or we do it
 > for neither.

Done

 >   Since Colin has been striving to assign some physical meaning to
 > at least one correlation we've left unexplained (the core power-
```

> hot spot compactness one), I thought I'd resurrect an old qualitative
> argument I'd been making for the hot spot collimation-hot spot flux
> ratio one. Perhaps better collimated jets maintain higher gammas
> right out to the hot spot, thus producing a spot that's both smaller
> and Doppler boosted?
>

Any takers?

> (5) p. 65, last par. (onto p. 66): We survived the entire discussion
> of the central feature-straight jet promience correlation to this
> point without ever mentioning relativistic beaming, so it strikes a
> somewhat dissonant chord in the opening and closing of the jet-counterjet
> prominence paragraph.

I agree this is a bit dissonant. But I don't see how else to introduce the idea that we should test for an anti-correlation here. Maybe this should be moved to 6.2 or 7.1?

(agreed later to leave this section as it is)

>
> (6) p. 78: Like Colin, I can do without a derivation of the slope
> formula. The only thing I would still like to see added here is a
> statement of our assumption of constant intrinsic ratios of central
> feature to extended emission and straight jet to extended emission
> (the formula really does depend on that; otherwise all bets are off).
>

I read this as covered by the "intrinsically similar" assumption. Easy to expand if anyone else wants it spelled out more ....

No comments from others yet....

> (7) p. 78, Sec. 7.1: I feel pretty strongly that some mention of earlier
> work suggesting gamma_jet~2 should be made. I have passed along
> Scheuer & Readhead (1979) and Owen & Puschell (1984) as examples;
> perhaps you would cite arguments in your 1984 ARAA article, or some
> more recent review?

agreed to leave this out, others now commented on.

Unfortunately, I think Scheuer and Readhead were estimating gamma_c, not gamma_j as we now use it. Owen and Puschell estimated gamma_j <=2 from the rate of jet detection in the Jodrell sample, and Bridle and Perley estimated the same from the rate of detection in the then-available 3CR sample, both on the precept that the parent population would be isotropically oriented (but still QSRs). We now have a 100% detection rate but in a sample that we think is orientationally biased. I'm really not sure that any of these old "gamma=2" statements can properly be compared with what we are now doing. Twist my arm, please ....

>
> (8) p. 80, 2nd par.: Just change first sentence to begin "The lack of
> correlation between the prominence of the counterjet candidates and
> that of the straight jets suggests that....". This is because we
> never actually present a result for lack of correlation between
> central feature and counterjet prominence. We could, but I don't
> think it adds much; however, I'll get numbers for you if you want them.
>

now done

I think we should put the number in rather than leave the statement out.

All other comments and suggestions in Dave's message included as is .....

From abridle Fri Dec 17 15:20:50 1993
From: abridle (Alan Bridle)
To: dhough@physics.Trinity.EDU, cjl@wells.haystack.edu, jburns@nmsu.edu, rl@rgosc.ast.cam.ac.uk
Subject: Re: Comments
Date: Fri, 17 Dec 1993 15:19:24 -0500

```
colin_lonsdale writes:
 >
 > Alan, this is just a quick primer.  The detailed comments and suggested
 > text will arrive early next week, and I will send that message to the
 > whole group (except Robert, who I have never been able to reach ...).

Robert, can you help Colin with this?  I seem to be able to find you
at all three of your addresses.

 >   I do have a number of comments, which should be no surprise to you.
 > I feel the bias towards relativistic jets is fairly blatant, and many
 > changes in wording will be recommennded, with the goal of a more
 > dispassionate assessment of the models.

Send 'em along, Colin!

 > The inhomogeneous jet picture is well presented, but several key points
 > are omitted or glossed over.  First, there are physical problems with
 > maintaining a high-gamma (high Mach number?) spine through the sometimes
 > extreme thrashing of the jet beefore hotspot entry.

I agree this is an interesting point if we have to deal with "extreme
thrashing", as in 3C68.1, maybe 3C336.  Anyone got a quantitative
argument for what is "extreme" though?

 > Second, the argument
 > is purely qualitative, but the original objection is quantitative.  The
 > new model changes the quantities somewhat, but the objection remains.
 > I plan to work out some quantities, based on required ratios of **compact**
 > hotspot emission (can use extreme cases like 3C351 with validity),
 > postshock gammas with various obliquities.

Maybe this is what will answer the previous question.  Any idea when
you might get to this Colin?  (Dave's Christmas present hangs in the
balance!).


 > In the only viable picture of intrinsic asymmetry-induced depol asymm.,
 > I have to disagree taht the depol. medium response time will be much
 > longer than the jet/hotspot response time.  Internal lobe sound speed
 > will be something like c/sqrt3, so sides of lobe will "feel" jet about
 > same time that hotspot does/.  The spatially correlated "sheath" of
 > depol material that is needed can repond swiftly after that, because it's
 > thin.

I need education here. I presume this is a model with a thin skin
of thermal matter round one lobe and not the other.  What
determines the response time in this skin far from the hot spot?

A.
```

From abridle Fri Dec 17 15:05:13 1993
From: abridle (Alan Bridle)
To: dhough@physics.Trinity.EDU, cjl@wells.haystack.edu, jburns@nmsu.edu,
rl@rgosc.ast.cam.ac.uk
Subject: Re: Hough comments on 11/19 draft
Date: Fri, 17 Dec 1993 15:03:51 -0500

```
Dave Hough writes:
 >
 >      Here is a short list of comments on the November 19th draft.
 > I think it's in GREAT shape, especially after your and Robert's
 > reorganization of the end pieces, which I always thought was
 > needed but couldn't quite put my finger on how to do it - well
 > done.  Here goes:

----- all on Dave's list to be done as he suggested except maybe these:

 >
 > (3) p. 19, last paragraph:  OK, I can let old Fig. 9a go, but then
 > I would argue that the new Fig. 9a (south-west lobe) should be
 > enlarged to show more of the jet.  But maybe it would be easier
 > to keep the old Fig. 9a in?  I was the one who first wanted this,
 > and I still think it's needed.
 >

I think we can simply let 9a go and report the knot polarimetry in
Table 11, and will leave it that way unless someone else wants to
include the jet in the new Fig.9a (note that this would
be at the expense of clarity in the lobe).

 >      Also, I agree that unit slope results from common normalization
 > if the range of central feature and straight jet fluxes is tiny
 > compared to the range of extended lobe fluxes, i.e., a plot of
 > log(X/Z) vs. log (Y/Z) just becomes logZ-logZ for essentially
 > constant X & Y compared to Z.  But for X, Y, & Z having comparable
 > ranges, the slope will TEND towards unity, but there must be some
 > uncertainty associated with this given the ranges of all three
 > parameters.  From my limited simulations, I would guess the
 > uncertainty is in the neighborhood of 5%, which certainly means
 > our statement that our slope is significantly below the unit slope
 > is true.  I guess all I'm after is some acknowledgment of the uncertainty
 > of the unit slope prediction, EVEN if the correlation is entirely due
 > to common normalization, because the numerators have comparable range
 > to the denominator.
 >

Can we just replace "found" in line 6 with "expected"?  1.0 is the
expectation value for an ensemble of such correlations if the
relationship is indeed produced entirely by the normalization.  Dave's
absolutely right that an instance in a small sample could deviate         ✓
from the expectation value, and we can in fact derive the distribution
of apparent slopes from the simulations, but I'm a bit reluctant to
plunge the reader into quite this level of detail here.  Anyone else
with a strong opinion?


 > (11) p. 68, opening of Sec. 5.5.4:  OK, if we're not going to
 > mention counterjets and their hot spots here, should we DELETE
 > their entries in Table 19?
```

>

But we refer to the lack of counterjet-LLS correlation in the abstract and Table 19 was where we justified this. I think the problem is the other way round: the draft is missing pieces of text in 5.5.2 and 5.5.4 that should refer to the counterjet prominence results (or rather, the lack of them). We need "Jets, counterjets and central features" as the title for 5.5.2, and to add the counterjets to the discussion below.

Same for counterjet hot spots, only just in 5.5.4 for them.

Cheers, A.

From root Wed Dec 15 17:01:19 1993
From: dhough@physics.Trinity.EDU (David Hough)
To: abridle@polaris.cv.nrao.edu, cjl@wells.haystack.edu, jburns@nmsu.edu, rl@rgosc.ast.cam.ac.uk
Subject: Hough's FINAL COMMENTS on paper as of 12/15/93
Date: Wed, 15 Dec 93 15:51:39 CST

San Antonio, TX
December 15, 1993

Alan:

My last major message, on November 28, contained a fairly short list of 25 items that I thought should be addressed for the paper. That list, which I know you have at least partly handled already, together with today's short list of 15 items below, will be my FINAL WORD on the paper at this point.

Today's list
----

(1) Abstract, pp. 2-3: We correctly mention that counterjet candidates are found in sources with a wide range of central feature prominence and projected linear size. I only note that this seems never to be stated explicitly in the body of the paper (although I may easily have missed it). I hardly think this is critical, just thought I'd point it out.

CJ-cf is on p.80. CJ-LLS is in Table 19 and should be added to explicit discussion on p.80.

✓ (2) p. 25 last line, p. 26 first line: "..on selected contours of total intensity." This is one where the contours on the pol. plot are NOT same as any on the intensity plot (Fig. 21 for 3C249.1).

✓ (3) p. 38, first paragraph: I think the VLBI sidedness stuff is OK here, but it might be slightly preferable to tack it on to the end of each source's bit in Section 4 (or opening of Section 5 for 3C47), as we've done for the superluminal results. It would be very simple for the four sources for which we've already done this - 3C47, 3C204, 3C263, and 3C334 - since the superluminal papers also discuss the sidedness. For 3C208, it would be a "Hough (unpublished)" ref.; for 3C249.1, it would be a "Hough (1986)" ref.
AND TO ANSWER COLIN'S QUESTION ON THIS POINT: For the seven sources NOT mentioned, 4 have been poorly observed and 3 unobserved with VLBI. No determination of symmetry or asymmetry of the pc-scale structure is possible (we can just tell that 175, 215, 336 & 351 are slightly resolved).

$\boxed{\text{Expand to give refs. again.}}$

Keep together to emphasize the point.

✓ (4) p. 56 & p. 58: We discuss correlations of Colljh with Qhs and Qlob (the latter of which I think is dominated by the hot spot contributions anyway). For Qhs on p. 56, we make light of it because it supposedly depends so strongly on 3C68.1 & 3C351. However, the significance drops only to 97% without them, just as it does for Qlob on p. 58 (where we don't mention any 68.1/351 caveats). So either we mention the 68.1/351 catch for both of them, or we do it for neither. (Interesting that dropping these two sources drops significance for the collimation correlations to a similar level as for the central feature-straight jet prominence relationship!).
Since Colin has been striving to assign some physical meaning to at least one correlation we've left unexplained (the core power-hot spot compactness one), I thought I'd resurrect an old qualitative argument I'd been making for the hot spot collimation-hot spot flux ratio one. Perhaps better collimated jets maintain higher gammas

right out to the hot spot, thus producing a spot that's both smaller and Doppler boosted?

(5) p. 65, last par. (onto p. 66): We survived the entire discussion of the central feature-straight jet promience correlation to this point without ever mentioning relativistic beaming, so it strikes a somewhat dissonant chord in the opening and closing of the jet-counterjet prominence paragraph.
ALSO, I'm happy with your ASURV results.

But how to get around this, how else to introduce the anti-correlation? Move to 6.2 or 7.1?

(6) p. 78: Like Colin, I can do without a derivation of the slope formula. The only thing I would still like to see added here is a statement of our assumption of constant intrinsic ratios of central feature to extended emission and straight jet to extended emission (the formula really does depend on that; otherwise all bets are off).

— Isn't this covered by the "[illegible]" [illegible]

(7) p. 78, Sec. 7.1: I feel pretty strongly that some mention of earlier work suggesting gamma_jet~2 should be made. I have passed along Scheuer & Readhead (1979) and Owen & Puschell (1984) as examples; perhaps you would cite arguments in your 1984 ARAA article, or some more recent review?

check what they said. S&R were estimating γ_c, not γ_j. O&P & B&P set $\gamma_j \approx 2$ based on occurrence rates ~ JB [illegible].

(8) p. 80, 2nd par.: Just change first sentence to begin "The lack of correlation between the prominence of the counterjet candidates and that of the straight jets suggests that....". This is because we never actually present a result for lack of correlation between central feature and counterjet prominence. We could, but I don't think it adds much; however, I'll get numbers for you if you want them.

why not?

(9) p. 85, third line from bottom: "among" rather than "between". ✓

(10) p. 86, Sec. 7.3, 2nd par.: Can we think of any reason a flip-flop model might, or might not, predict the recession of the jet hot spots (since the other important point about compactness is addressed)?

(11) p. 86, Sec. 7.3, 3rd par.: If we were to instead say "The connections among counterjet detection, jet bending, and HOT SPOT PROMINENCE, and between the SIDEDNESS and prominence of the straight jets and central features, might ...", is that claiming too much for the variant of flip-flop model discussed here? ?

(12) p. 95: Hough, Zensus, ... ref.: it's out now, and the editors should be changed to read "R. J. Davis and R. S. Booth", and the page number is "p. 195". ✓

(13) Table 9: Fine, with your explanation, don't bother with 3C263 J. ✓

(14) Tables: On your visit here, you mentioned the need for a table with the "B" values of central feature and straight jet flux densities; do we still think this is necessary? Yes.

(15) Finally, I have gone through as carefully as I know how, checking to see if results from Sections 5 & 6 have been appropriately brought to bear on source models in Section 7, and to see if the Abstract and Summary really contain all essential elements and are sensibly organized. I could quibble over things at a low level, but I recommend NO changes based on my review. Of course, Colin has some strong opinions about Section 7 now, but that's probably healthy, since I suspect Robert really had a chance to tip things his way for a while in Socorro! As I said, I can take Secs. 7 & 8 "as is", but would not be disturbed by some modifications along the lines Colin is suggesting.

From abridle Wed Dec 15 12:32:49 1993
From: abridle (Alan Bridle)
To: jec@dopey.haystack.edu, cjl@wells.haystack.edu
Subject: Re: statistics software
Date: Wed, 15 Dec 1993 12:32:43 -0500

```
Colin, Jennifer,

My core-jet prominence simulator is now in our anon-ftp area on
polaris as promsim.exe.  I have fudged it to be a bit more general
than the version I put together for our quasar sample, and have tested
that this version still gives the same results as before for our 3CR
case.  To copy it to your machine, you will need to do the following:

ftp  polaris.cv.nrao.edu

log in as:     ftp
password:      your email address

cd /pub            {to get into the public directory}
binary             {to set binary transfer protocol}
get promsim.exe    {to initiate the transfer, 27072 bytes}
quit               {when transfer complete}


Then copy file to your PC (if this was not the machine from
which you ran the ftp).

Once it's in the PC, type promsim and the program should come up in 80
x 25 text mode and prompt you for the following inputs:

Min and Max Flux densities for Core
  {enter flux density range in whatever units are convenient,
   does not matter so long as same units for core, jet and
   lobes.  Use space to separate min and max flux densities, not
   comma}
Min and Max Flux densities for Jet
Min and Max Flux densities for Lobe
Number of Sources per Correlation
   {this is the number of simulated sources per regression.
    Maximum is set to 100 at present}
Number of Correlations to try
   {this is the number of regressions that will be processed,
    Maximum is set to 32767 at present}
Correlation level to examine
   {The program will produce a histogram of the achieved
    correlation coefficients binned in 0.05 intervals, but
    you can also count the number of times that any given
    correlation coefficient is exceeded.  This is where
    you set that threshold correlation value}
Write Individual correlations to disk?
   {answer y or n. 'y' will dump the result of every
    regression analysis to the disk: one line giving
    the correlation coefficient, slope of the standard
    Y-on-X regression (Jet on Core), slope of the
    standard X-on-Y regression (Core on Jet), and
    slope and error of the "major axis line" -- the
    regression done minimizing the perpendicular
    distances of the data from the line rather than
```

just the vertical distances (Y on X) or horizontal distances (X on Y).

N.B. the output file will be about a Megabyte if you select 'y' here and run 30,000 trials. Have plenty of disk space available if you want to log all the correlations from a big run!}

Once you've entered all of these, the program will go off and compute the correlations, showing its progress on a bar graph across the bottom of your screen. (Once the number of sources times the number of trials exceeds a few hundred thousand, the program will take some minutes to run ...).

Once all the correlations are complete, it writes a file called promsim.out to the disk in whatever directory promsim.exe was run from. This file records your input data, the individual correlation results if you asked for them, and the statistics of the correlation coefficients always.

Hope this makes sense and does what you want.

Best wishes,

Alan B.

From root Wed Dec 8 09:30:11 1993
From: Colin Lonsdale <cjl@dopey.haystack.edu>
To: abridle@NRAO.EDU
Subject: Concern about straight jet correlation ...
Date: Wed, 8 Dec 93 9:26:00 EST

Alan, offline from the main debate, I want to point out to you something that has come to my attention during the analysis of the correlation for this sample plus my high-z sample. It turns out that the plot, while a beautiful correlation, segregates by sample membership in prominence. The prominences from the high-z sample are generally higher than those in the counterjet sample. This effect causes a correlation between bent jet/core prominences, though with a non-unity slope (0.72, r=.61, 26 sources). It could be due to two effects. First, we could simply be missing lots of lobe flux in the high-z sample. This is possible, but not to the extent we see, I think (typical high-z prominences 2 orders of magnitude higher !). The second possibility I thought of, which should concern us more in the paper, is that the high-z sources are more luminous, and therefore more FR2-like. In other words, the lobes become much more hotspot-dominated, and when you normalize by extended flux, prominences go way up. This kind of luminosity/morphology effect has not, I believe, been considered by us as a possible origin of the correlation. Maybe you want to think about it some more, and let me know what you think.

Cheers,
Colin

From root Wed Dec 8 09:30:11 1993
From: Colin Lonsdale <cjl@dopey.haystack.edu>
To: abridle@NRAO.EDU
Subject: Concern about straight jet correlation ...
Date: Wed, 8 Dec 93 9:26:00 EST

Alan, offline from the main debate, I want to point out to you something that has come to my attention during the analysis of the correlation for this sample plus my high-z sample. It turns out that the plot, while a beautiful correlatio n, segregates by sample membership in prominence. The prominences from the high-z sample are generally higher than those in the counterjet sample. This effect causes a correlation between bent jet/core prominences, though with a non-unity slope (0.72, r=.61, 26 sources). It could be due to two effects. First, we could simply be missing lots of lobe flux in the high-z sample. This is possibl e, but not to the extent we see, I think (typical high-z prominences 2 orders of magnitude higher !). The second possibility I thought of, which should concern us more in the paper, is that the high-z sources are more luminous, and therefor e more FR2-like. In other words, the lobes become much more hotspot-dominated, and when you normalize by extended flux, prominences go way up. This kind of luminosity/morphology effect has not, I believe, been considered by us as a possible origin of the correlation. Maybe you want to think about it some more, and let me know what you think.

Cheers,
Colin

From root Wed Dec 8 09:10:15 1993
From: Colin Lonsdale <cjl@dopey.haystack.edu>
To: abridle@polaris.cv.nrao.edu, dhough@physics.Trinity.EDU, jburns@nmsu.edu,
rl@rgosc.ast.cam.ac.uk
Subject: Comments on the draft
Date: Wed, 8 Dec 93 9:06:02 EST

Comments on the latest draft of the counterjet paper, CJL December 7 1993

Let me start by taking each of Alan's "roadmap" points in turn.

1. New regression method, fine with me.
2. Agreed, 1.3 it is.
3. Fine.
4. One caveat about survival analysis - it buys you very little if anything
if your upper limits are not well mixed with the detected points. Are they
well mixed?
5. No objections from me. I don't think the paper is in danger of wasting away.
6. Fine (agreeable chap, aren't I?)
7. Question: in the other 7 sources for which "enough closure phase" information
is not available, why is that? Are there any which are basically unresolved?
If the primary asymmetry were intrinsic, you would expect to see a one-sided
jet if it is approaching, and no parsec-scale jet if receding.
8. Fine.
9. Electron streaming motions? Interesting, look forward to seeing it.
10. I have some problems with the rewrite, dealt with in detail below.
11. By and large, I like the new section 6.5
12. More problems, detailed below. I don't think the derivation is needed, it's
not that hard (except when, like me, you forget to take the logs!)
13. This section, 7.3, is now too dismissive, details of my opinions below.
14. Again, I recommend some rewording, see below
15. Good decision. We are probably seriously over-interpreting some of this
stuff as it is.
16. I regret that I must plead excessive workload to go over all 112 pages of te
xt,
21 tables and 46 figures with a fine toothcomb. The bulk of the paper looke
d
pretty clean to me last time I was able to really pore over it, and I trust
you guys to have done nothing destructive! Plus, we have Dave, the human
microscope to do it for us.

On to specific comments:

On page 64, line -8, the distribution of extended fluxes is cited as the only
reason that our analysis is likely to be too conservative, but that's not really
it
at all. The whole point of the prominence parameters is the idea that we have a
mixture of strong and weak sources here, and prominences remove this dispersion
in
source strength (caused by distance or intrinsic differences in power), leaving
only power-independent quantities. A false correlation will arise in these prom
inence
parameters only from departures from source strength scaling. In other words, i
f
our 13 sources span, say, a factor of 10 in intrinsic strength, that is tracked
perfectly
by the extended emission (because, e.g., it's unbeamed), the observed distributi
on
of extended emission strength has nothing whatever to do with false correlations

$0 \to$ Mex for Core
$0 \to$ Mex Jet
Min $\to$ Mex for Lobes
$\to r = 0.83$
exceeded 2% of time.

,
and we can just use the r-values uncorrected. So, I have been arguing that thes
e
false correlation simulations have been "worst-case" because I strongly suspect
that
a large part of the observed extended emission dispersion is due to strong sourc
es
being strong, and weak ones being weak. No way to prove it, though.

That brings me to the bottom of page 64. I thought we had dispensed with this f
lux
density/flux density correlation as trivial, precisely because we have a range o
f
source strengths here. Bright ones are bright, faint ones are faint, and bingo
you
have a correlation. How can we ever prove otherwise if we don't try and normali
ze
by using prominences? Unless, of course (and I forget, with all the flying emai
l) the
flux-flux correlation mentioned is the only one among all the parameters, in whi
ch
case this fact is what should be mentioned, not the mere fact of an apparently
significant correlation. Having said all this, I'm still quite happy with the s
trength
of our correlation.

On page 73, the first paragraph has me confused. I have the feeling that it's b
een
explained to me before, but are there really folks who entertain for an instant
the
idea that these FRII sources are really just FRI sources viewed in a funny way?
 Why
is this paragraph needed? I don't get it.

As you must all have expected, I have various problems with the new incarnation
of the
various parts of section 7. Here goes.

Page 78, lines 7/8, we refer to "some portion" of the jet, conveniently forgetti
ng that
the entire jet is pretty darned asymmetric. You must play the same game between
 the
straight and bent jet segments as is done between the pc and kpc jet scales. Wh
atever
produces one asymmetry must be related to whatever produces the other, since the
y are
always on the same side. But now we are starting to argue that the bent jet seg
ment
is dominated by interaction. Its prominence certainly appears to be, true. Has
 anybody
stopped to think what degree of bent jet prominence correlation with core promin
ence you
expect if beaming in the bent segment is still enough to generate the degree of
asymmetry
between bent jet and bent counterjet? My strong hunch is that you expect a whol
e lot
more than you see. In many cases the limits on straight jet/straight counterjet
 are
markedly smaller than those on bent jet/bent counterjet, I think.

Page 79, lines 9/10, a bit hard to read without stating what the correlation coe

fficients
change **from**.

Page 81, line -10, I would insert ", given high obliquity," between possibly and relativistic. Turns out that the conditions are quite restrictive, and unless t he incoming
flow is very high gamma, you need a very oblique shock to keep deceleration to acceptable levels. I latched onto this as a convenient point to mention somethi ng.
The oblique shock model of hotspots must tread a thin line between oblique enoug h to
maintain post-shock beaming, but not so oblique that the resulting feature is no thing
more than a jet knot. We even make this distinction explicitly when defining hotspots in section 4.1. I think this line may be so thin as to be invisible .. . many
of these hotspots are enormously bright, and have no sign of a well-collimated f low
beyond them. What makes the terminal shock so different from those that produce jet knots if it is so oblique that you have high gamma beyond them? See also my reservations about quantification of the inhomogeneous jet model below.

Section 7.1.1 I applaud the development of such a creative modification to the model.
However, I want to point out that it is strictly qualitative at the moment. Bef ore I
explain that, let me point out that one of the several correlations noted in sec tion 5
that is left basically unused in later sections is that linking the hotspot comp actness
ratio to the core power, sec. 5.3, r=0.83 (i.e. very good).
This trend, for strong core sources
to have small asymmetries in hotspot compactness, runs exactly opposite to what should
be a strong prediction of the inhomogeneous jet model. Isn't this so? Just for the
record, an intrinsic asymmetry booster like myself would think in terms of disk (i.e. Mach disk) shaped hotspots on the active side, and more spherical blobs on the
less active side because of a weaker terminal shock, lower densities, longer syn ch.
lifetimes, more diffusion etc. Then a source close to the line of sight (strong core)
would show the active disk hotspot face-on and big, while the weak core sources would show them more nearly edge on, and small. Hence the correlation.

Page 83 line 1, I find the word "decollimating" to be strangely placed.
The statements on lines 4 to 6 are excessively sweeping in their claims. What d oes
"suppressed" mean? By how much? Counterjet hotspot emission would not be "enti rely"
from sheath material, depends on the post-shock Doppler factors. My point is th at
the relevant ratio is between the surface brightnesses of the two hotspots. You are
saying that the counterpart of the jetted hotspot (the beamed "core" of the post -shock
flow") is effectively hidden. On the counterjet side, it must have a similar si ze, but
just be beamed away from us. The flux ratios range up to 100, and often exceed 10, but

the surface brightness ratios are much higher. Given the likely orientation ran
ge of
our sources, you should be able to state directly what kind of post-shock Lorent
z
factors are needed to reconcile observation and inhomogeneous jet model. They m
ust be
high, my guess is embarrassingly high. Remember, this fast spine, whatever gamm
a it
started with (5, at the core?), has to negotiate sometimes violent thrashing (wh
ich
simply must set up decelerating internal shocks) AND the terminal hotspot, and S
TILL
have enough oomph to generate the above mentioned surface brightness ratios. It
 is
not enough to blithely state that the counterjet hotspot spines will disappear "
entirely".
I suggest that this section either be seriously watered down in the claims depar
tment,
or be beefed up with the addition of some numbers which show that it really can
do the
job.

One more observation ... the true flux density of the counterjet hotspot "spine"
which is supposed to be invisible is not directly measurable. All we can do is
set
an upper limit to its brightness, because it is embedded in the more diffuse
"sheath hotspot", so you'll see the sheath hotspot flux density times the compac
tness
ratio anyway. The thing you are after may really be much weaker. So the surfac
e
brightness ratio sets a lower limit only on the required postshock gammas.

I have no real problem with the additional complexity required. Any model which
explains our data needs additional complexity, this is as plausible as any in th
at
department. I just don't think it'll do the job, but I could be wrong.

Splitting up section 7.2 is OK, but 7.2.2 highlights what I see as a general pro
blem
with sections 7.2 ands 7.3. The distinction between the models is, as stated ri
ght
at the start of the paper, somewhat artificial. Trying to rigidly maintain the
separation between the models is bound to lead to lots of charging through wide
open
doors. Obviously, since high gammas abound in the cores, nobody but a fool woul
d go
around claiming that beaming has no significant influence on parsec-scale appear
ance.
The end of 7.2.2 is an example. In general, any asymmetry that originated on pc
 scales
and is big enough to alter the emissivity of the jet by factors of 100 and more
is
also big enough to dramatically change the sound speed in that jet, and therefor
e its
mach number. So your asymmetric dissipation model on small scales becomes somet
hing
much more on large scales, perhaps taking the two jets into completely different
 flow
regimes. Then you have no problem at all dealing with recessed hotspots. If yo
u have
an unrealistic model such as in 7.2.2, it's easy to dismiss it. However, we can

not
in all fairness dismiss a model in which the only basic asymmetry is in the diss
ipation
on pc scales, for the above reasons. This sort of thing always brings a picture
of
Hercules A to mind .. were those two jets once symmetric? Sure ain't now!

The main problem I have with section 7.3 is in a similar vein. There are all so
rts of
repeated references to the "strict" flipflop model, which frankly I thought was
always
a complete non-starter (and have said so in print, many years ago).
As I pointed out some time ago, a time-dependent power ratio
is the only thing to consider, unless we broaden the definition of "intrinsic as
ymmetry"
to include composition or other differences between the jets. If we do that, it
merges
with the type of model put forward in 7.2.2. I see no point even discussing an
obviously
inadequate model, of which the strict flipflop is one. This inadequacy is used
repeatedly
to paint a picture of disfavour for the whole class of intrinsic asymmetry model
s.
An analogous treatment of section 7.1 would be to constantly point out what does
not
fit with a constant-gamma jet from core to hotspot. The last paragraph on page
87 is
the most objectionable in this regard.

I'd like to point out that the sound speed in the lobe is likely to be close to
c/root3,
which means that as a newly invigorated jet enters it, the influence of that jet
(shocks,
stirring etc) will be felt at the sides of the lobe at about the same time that
the
jet reaches the hotspot. The explanation ofthe depolarization asymmetry in this
type
of model is that the lobe is surrounded by a sheath of potentially depolarizing
material,
which is ineffective when the lobe is fed by the stronger jet (shearing, and com
pression
due to lobe expansion de-emphasises line-of sight field? Electron content drops
because
of shock-induced condensation and recombination in sheath? Something else?). Th
e
material is spatially correlated with the lobe, and may well be a thin sheath.
As such,
it could respond to the presence of the active jet on the same timescale as the
hotspot.
I.e. if you admit an intrinsic asymmetry explanation for the depol asymmetry at
all, I
think you have to acknowledge that it can respond quickly.

Before somebody else attacks me for being biased in what I require of each model
, I of
course acknowledge that one cannot blithely state that the intrinsic asymmetry m
odel
can do depolarization without quantifying the various timescales a bit. Just as
the beamed hotspot idea is treading a thin line between obliquity and hotspotnes
s,
the intrinsic model is treading a thin line between timescales for asymmetry

variations and lobe evolution.

This begs the question ... to lean strongly towards one model or the other, we m
ust
do some quantitative calculations. IS IT WORTH IT? Or should we be content to
point out the issues, and not prejudge which constraints are fatal (as is done t
o
an extent now)? I favour the latter, which means backing off some of the more
condemnatory language in sections 7.2 and 7.3, while pointing out explicitly tha
t
inhomogeneous jets have to satisfy possibly very stringent requirements that it
is
not at all clear can be met. I would leave model-building to other papers.

By the way, since we drag in the depolarization asymmetry, why not also drag in
the
spectral index asymmetry? As we now know (right, Alan?), this is not fully expl
ained
by the hotspot asymmetry, but extends into the lobes. This is a bigger problem
for
the beaming model than depolarization is for the intrinsic asymmetry model.

I am running out of steam here, and will shortly be dragged away onto other thin
gs
for a while, so will defer comments on section 8. I think it's basically good,
though
I feel it should be modified to reflect the other changes I am recommending. Ra
ther
than make everybody wait for my comments on 8, I'll let you see the above and wa
it
for reactions.

Regards to all,

Colin

From root Thu Dec 2 14:50:55 1993
From: colin_lonsdale <cjl@dopey.haystack.edu>
To: abridle@NRAO.EDU
Subject: Comments
Date: Thu, 2 Dec 93 14:47:17 -0500

Alan, this is just a quick primer. The detailed comments and suggested text will arrive early next week, and I will send that message to the whole group (except Robert, who I have never been able to reach ...).

I do have a number of comments, which should be no surprise to you. I feel the bias towards relativistic jets is fairly blatant, and many changes in wording will be recommennded, with the goal of a more dispassionate assessment of the models.

The inhomogeneous jet picture is well presented, but several key points are omitted or glossed over. First, there are physical problems with maintaining a high-gamma (high Mach number?) spine through the sometimes extreme thrashing of the jet beefore hotspot entry. Second, the argument is purely qualitative, but the original objection is quantitative. The new model changes the quantities somewhat, but the objection remains.

I plan to work out some quantities, based on required ratios of **compact** hotspot emission (can use extreme cases like 3C351 with validity), postshock gammas with various obliquities. The inhomogeneous jet model expands the wriggling room somewhat, that's all. But it is presnted as the a sufficient solution to hte problem, and sanctified by the later use of gamma-baars and biased language in section 8. I feel the deiscussion of the models is too compartmentalized. The most natural conclusion from this work, I believe, is that a combination of models is appropriate.

As the designated intrinsic asymmetry champion, I'm not about to attack our gamma_j=1.8 conclusion. I think it's right. It's just that there are big intrinsic asymmetries too. Shhoting models down because they don't explain everything (as happens to a degree in 7.2 and 7.3) is not the right approach, I feel ... we don't have to make an either-or choice so starkly.

In the only viable picture of intrinsic asymmetry-induced depol asymm., I have to disagree taht the depol. medium response time will be much longer than the jet/hotspot response time. Internal lobe sound speed will be something like c/sqrt3, so sides of lobe will "feel" jet about same time that hotspot does/. The spatially correlated "sheath" of depol material that is needed can repond swiftly after that, because it's thin.

So that's a preview of what is coming. I don't mean to be too critical, much of the reorganization is a huge improvement. But you must have anticipated this reaction from me! I have my plate full through the weekend, will do my best to be quick.

Colin

From root Sat Nov 27 11:34:48 1993
From: dhough@physics.Trinity.EDU (David Hough)
To: abridle@polaris.cv.nrao.edu
Subject: Nov. 1 Set of Tables & Figures
Date: Fri, 26 Nov 93 17:23:20 CST

San Antonio, TX
November 26, 1993

Alan:

Here are my brief comments on the set of Tables & Figures that you sent to us all, postmarked 11/1/93:

TABLES

#1 - Comma after "Wardle" in ref. 26. ✓

#9 - Should there not be an entry for 3C263 J, since it seems to appear on Fig. 39?

Didn't consider it part of the periodic-appearing train.
Also all → lower case knot ID.

#10- Need "Source" and "ID" headings on first two columns. ✓

#13- Need "Source" and "ID" headings on first two columns. ✓

#19- Delete stray comma after "cf" in notes to the table. ✓

#21- Remove hyphen in "Linear Correlation" (or else ADD it in Table 19).
All "eta"s (9 of 'em) should be set in Greek font. ✓
All "Spreadj"s (9 of 'em) should become "Spr"s.
Strike note to the table, since a similar note is not used in Table 19, and for both tables adequate explanation of significance is given in the text.

FIGURES

#16- There appear to be small discrepancies in the lowest level contours in comparison to the previous version of this figure. I'd chalk this up to low-level funny business in AIPS, but I looked carefully at ALL the figures and this is one of only a handful where I could see definite discrepancies. So if a DIFFERENT SET OF CONTOURS was actually plotted, that should be checked and taken care of in the Figure Captions.

#17a-Here it is unmistakable that the FIFTH CONTOUR is lower than it was on the earlier plot you sent around. I don't see in the latest Figure Captions that you indicate any change here, so again this should be looked into.

Same figure as each other identical to my plot of 12 Jan 1988 which had a small Δz put in. Old fig. 16 was 9 Jan 1988 did not have this Δz.

#21- The low level contour used on the polarization map here does NOT correspond to any actually shown on Fig. 20, but maybe we don't care.

That's right and that's why we don't say the lowest contours are from Fig. 20 any more.

#31a&b-Again, the low level contour is not shown on Fig. 30, but I suppose we don't care?
More importantly, I just wanted to check that the polarization scale is now really "1 arcsec = 0.33". It WAS 0.25 before, and

Yes, the scale was changed to 1" = 0.33 for clarity. Contours are from B only: that's where we had the polarimetry. Caption to Fig. 3 need to be changed

I note the peak flux on the map is 0.33 Jy/beam, so I just wanted to confirm there wasn't a mix-up there, since this is the only plot I noticed a change of scale on.

#34- Again, there are small discrepancies, at least at the lowest contour level. If different contours were really used, can we amend the Figure Captions accordingly?

Contours are correct, the map has small Δz done to correspond to actual image.

#44- Horizontal axis label should go back to reading "Central Feature Prominence", as it once did. ✓

#45- This is ACTUALLY Fig. 46, the hot spot-bend angle figure, and is referred to by this number in the text and Figure Captions. More importantly, I have no idea where the data now plotted come from. This looks totally unlike the previous version, which I had checked carefully and thought was correct. The problem does NOT lie merely in the changing of the plot limits; my eye took this into account. Did you find yet another error in the data base that has changed the hot spot prominence numbers? ✓

#46- Is actually Fig. 45. ✓

OK, that's it, more or less all pretty trivial. I'll see how far I get through the latest, greatest draft (guess it's dated Nov. 19?) tonight, and try to get something off to you tomorrow.

-Dave

From abridle Fri Nov 19 17:53:25 1993
From: abridle (Alan Bridle)
To: dhough@physics.Trinity.EDU, cjl@wells.haystack.edu, jburns@nmsu.edu, rl@lpve.ing.iac.es
Subject: Road map of changes in QSR paper
Date: Fri, 19 Nov 1993 17:52:33 -0500

Hello all,

This is to accompany the new .ps version of the paper coming your way separately. It is an attempt to list the main changes that have come out of the meetings I had with Dave in San Antonio, Jack in Las Cruces and Robert in Socorro.

1. Dave and I found an algorithm for handling the prominence-prominence slope statistics that takes account of the existence of errors in both variables, and have used it to re-estimate the slopeand error of straight-jet versus central feature prominence relation. Because we have a 3:1 ratio of median variance between the jet prominence data and the central feature prominence data the answer is not far from the original "Y on X" regression: 0.63 +/- 0.12. It seems that Dave, Jack, Robert and I are all content to go with this result, so we collectively hope that Colin agrees!

2. Similar agreement that we should compare with the expected slope for alpha_c = 0, not alpha_c = 0.2, i.e. with a slope of 1.30.

3 The false-correlation simulation that I ran for the prominence-prominence data also shows that the significance of the correlation, not just its slope, rests on 3C68.1 and 3C351.

The discussion of all of this in Section 7.1 has therefore been updated. Because the simulation suggests we might be dealing with only a two-sigma _correlation_ significance, we are enlarging the discussion in Section 5.5.2 that supports the reality of the correlation. Colin has argued that the simulation may be a "worst case" comparison, and several lines of evidence agree with this. Section 5.5.2 is intended to bolster the case, so please read it particularly carefully to see if you are satisfied with the outcome.

4. We should run the counterjet prominence correlations that contain upper limits through Feigelson's ASURV package (which may also contain some tools for the "errors in both parameters" regression). I will look into this here (Socorro) and report results. If this doesn't pan out, I'll run the data through ASURV when I'm back in C'ville (2nd week of December). Present version of paper just has notes about where ASURV will be run. done

5. Jack suggested we drop Figs 9a, 10b, 10c. Robert agreed. Any objections from Dave or Colin? done

6. Jack also suggested that we should make a grey scale to draw attention to the counterjet in 3C215, as this does not stand out well on the contour plots. As A.J. did a terrible job on reproducing the photographs that Jack prepared for the Fernini et al. radio-galaxy paper, we think we should try to make a photograph and an AIPS GREYS bitmap. I have sent Jack a bitmap that may be suitable, and he will try to make a good photograph done.

also. I have also added to the text on p.22 in the hope of guiding novices to find a weak ridge-like feature on the contour map!

7. Robert noticed that we had forgotten to look into the correlation of VLBI extended-feature sidedness with our large-scale jet sidedness in this sample. Quick check by Dave of which central features have enough closure information shows that we know the answer for 6 of the 13, and all 6 correlate "as expected" from the general pc-scale to kpc-scale relation. Paper will now point this out and use the sidedness correlation to bolster the relativistic-jet discussion. At present this VLBI evidence appears only at the end of Section 5.1, and its consequences surface throughout Sections 6, 7 and 8. It might better be introduced source-by-source in Section 4?

8. Dave suggested we should make more of the fact that the hot spot prominence correlates best with the "abrupt deflection" measure eta_3c. Jack, Robert and I all agree, so it's emphasized in several more places, including the abstract.

9. Robert points out that the exponent of the beaming relation can be outside the range 2+a ---> 3+a if the fields are well-ordered (because of the relativistic aberration correction, esp for B-parallel to the jet). This is now mentioned in Section 6.2 (p.72).

10. Robert and I felt that the existing Section 7.2 confused several issues and needed a major rewrite. First problem was that it contained some discussion of dissipation that had nothing to do with _asymmetries_ in dissipation. Second problem is that it did not distinguish clearly enough between the models that invoke large-scale environmental asymmetries as the source of the asymmetric dissipation and those that build them in from the start of the jets. We believe that the former class of models is untenable, and now say so explicitly. We also felt that the discussion of the second class was over-simplified and have re-written it to be more general. So Section 7.2 now has two subsections, one dealing with each class, and the material in the old section 7.2 dealing with dissipation t hat is not necessarily asymmetric has been moved elsewhere.

11. "Elsewhere" is mainly a new section 6.5 which tries to collate collate the generic evidence for links between the central features and larger-scale properties in 6.5.1, and for interactions that modify jet properties in 6.5.2. There's no overall increase in text, I've collected this stuff from Section 7 by stripping out the material that was not actually model-specific. I think we may be repeating ourselves a few times still, however, and I think we may want to check the overlap between 6.5 and other parts of the paper rather carefully.

12. Section 7.1 has been streamlined and strengthened. Robert and I both felt that the inhomogeneous-jet hypothesis should be spelled out more clearly. I've rearranged the order of some of the material to make the logic of this section clearer and there's now a section 7.1.1 containing everything that we believe needs to be said about the inhomogeneous jet case. Colin especially should scrutinize this section, as it's the portion he's least likely to feel comfortable with.

Jack has suggested that we should include the derivation of the slope relation on p.78. I suggest this should be an Appendix if we do include it. Any comments?

13. Section 7.3 has been streamlined a bit and the additional argument against "pure" flip-flop from the depolarization asymmetry (a large scale medium can't change sidedness as rapidly as a supersonic jet can) has been added to the optical evidence.

14. Robert and I felt a major effort was needed to make the paper end on a more positive note. Section 7.4 has been replaced with a Section 8 that contains the more positive conclusions plus the stuff on comparisons with other samples and on how to test the relativistic-jet models in future in the light of what we've found here. The more low-key statements have been deleted or shuffled elsewhere, so that the finale is more rousing, though Colin in particular may now feel that it's too oriented towards the relativistic-jet models. I've also added some words about counterjet detection rates to the material on the comparison with the Fernini et al. RG's.

15. Robert and I got re-focussed on the issue of what happens when invisible counterjets start to bend. We went through asking what sorts of features lie just off the straight-jet axis in the counterjetted lobes in which we did _not_ identify counterjet candidates. This grew out o discussing the "hook" features in 3C334 and 336, which were classified as counterjet candidates. We also have 3C432 as an example of one of these seen at low resolution, and we were wondering if there might be any further such candidates. Looking at the images from this point of view, we noticed that the straight jet axes in 3C47 and 3C175 do not point at the counterjetted hot spots or candidates but into ridges that _hook_ into these hot spots. In 3C47 the ridge is in the middle of the counterjet lobe and is poorly defined (this is lower-resolution data than for the others). In 3C175, the ridge is the upper arc of the U-structure around feature O, and is clearly the narrowest thing in the lobe outside the hot spot. We debated whether to relabel the diagrams to draw attention to these features, but decided not to. For 3C47, the resolution is marginal, and the situation in 3C175 closely resembles a hot spot feature that I've mapped in 3C353, in which a narrow curved feature connects to the hot spot from one side, but an unimpeachable jet hooks into the spot along a different, but parallel track. We therefore decided simply to point out the existence of these features in the text, but not to raise the issue of whether they could be counterjet candidates loudly enough to second-guess our earlier decisions.

16. Numerous small changes. Far too many to itemize, but mostly just clarifying details. I'll assume that everyone will read the whole text once more before it goes off, and will then check whether the changes they suggested have been made. If you don't notice, or disapprove of, the small changes that others have suggested while you're doing this, I'll assume all is well. One disadvantage of my "road trip" was that some feedback came to me by old-fashioned routes like conversations over a beer or pieces of paper, and I can't circulate an E-record of every single change to you all. From here on, I ask that anyone proposing changes do so by E-mail

to the whole group simultaneously so everyone has a list of, and a chance to comment on, the proposed changes right away.

Colin -- we missed your input while you were on the road and NEROC was off the Internet. You may, probably will, find this version weighted further towards the relativistic jet picture than you like. I've also not included your point about hot spot multiplicity asymmetries because I was not sure it's consistent with what we are saying about there being no significant inhomogeneity asymmetries outside the hot spots as we now define them. When you get the new .ps file please wade in with non-relativistic jet perspectives and further stuff on the hot spot asymmetries wherever you think they are needed, but please also do this soon. There's a strong sentiment for submitting this before Christmas!!

Cheers,

Alan

From root Sun Nov 28 11:32:00 1993
From: dhough@physics.Trinity.EDU (David Hough)
To: abridle@polaris.cv.nrao.edu
Subject: Hough comments on 11/19 draft
Date: Sun, 28 Nov 93 10:24:36 CST

San Antonio, TX
November 28, 1993

Alan:

Here is a short list of comments on the November 19th draft. I think it's in GREAT shape, especially after your and Robert's reorganization of the end pieces, which I always thought was needed but couldn't quite put my finger on how to do it - well done. Here goes:

(1) p. 3, lines 17-18 in Abstract: careful about associating "particularly if a large bend occurs abruptly" with jetted hot spot prominence AND ill-definition of counterjetted hot spot; it should be attached to only the former, not the latter. ✓

(2) p. 18, last line: interchange order of "north-east" and "south-west". ✓

(3) p. 19, last paragraph: OK, I can let old Fig. 9a go, but then I would argue that the new Fig. 9a (south-west lobe) should be enlarged to show more of the jet. But maybe it would be easier to keep the old Fig. 9a in? I was the one who first wanted this, and I still think it's needed. NO.

(4) p. 20 last line-p. 21 first line: "from Figure 10. The jet..." ✓

(5) p. 22, lines 20-21: The contours are not 2, 6, and 16 mJy/beam, but 2, 6, and 16 times 75 microJy/beam. ✓

(6) p. 23, first line: "from Figure 14a. At this..." ✓

(7) p. 30, lines 11-12: it's "south-east" and "north-west". ✓

(8) p. 36, Section 5, 1st par.: Maybe this is the place to add the 3C47 superluminal motion result? Sorry this obvious point didn't occur to me earlier! It's "Vermeulen et al. (1993) reported proper motions in the central feature that imply a pattern speed of 3.7h^-1c." ✓

(9) p. 38, line 1: might be nice to have a reference list here; in fact, the list given in the prominence section (p. 62, third paragraph) should be exactly what's needed here. ✓

(10) p. 65, lines 4-7: It's actually the "mean probable error" or just "mean error" ratio that's "1.7:1". While I did use the standard deviation (the square of which is of course the variance) for the central features, a more complicated error propagation method was used to get the mean percentage error for the straight jets. This is what gives slope 0.63+/-0.12. If the 1.7 is too precise for your taste, rounding to 2 gives York's c=4, and takes us back to 0.62+/-0.12. ✓

Also, I agree that unit slope results from common normalization if the range of central feature and straight jet fluxes is tiny compared to the range of extended lobe fluxes, i.e., a plot of log(X/Z) vs. log (Y/Z) just becomes logZ-logZ for essentially constant X & Y compared to Z. But for X, Y, & Z having comparable ranges, the slope will TEND towards unity, but there must be some uncertainty associated with this given the ranges of all three parameters. From my limited simulations, I would guess the uncertainty is in the neighborhood of 5%, which certainly means our statement that our slope is significantly below the unit slope is true. I guess all I'm after is some acknowledgment of the uncertainty of the unit slope prediction, EVEN if the correlation is entirely due to common normalization, because the numerators have comparable range to the denominator. → expected.

(11) p. 68, opening of Sec. 5.5.4: OK, if we're not going to mention counterjets and their hot spots here, should we DELETE their entries in Table 19? no'add here.

(12) p. 74, line 12: remove one "of" in "of of".

(13) p. 74, line 15: I definitely prefer "absent" to "weaker".

(14) p. 79, line 15: the "A" slope is really 0.52+/-0.12, because the York c factor is different (2.8^2=8) for the "A" data! (My mistake).

(15) p. 81, line 2: delete stray comma after "supply".

(16) p. 82, Sec 7.1.1, line 4: the fraction is not f(Beta), but f(Beta)dBeta. AND in the equation, the entire Doppler factor of gamma(1-BetaCosTheta) should be raised to the -(2+alpha_j) on both sides of the equation.

(17) p. 86, line 1: "travel time to them (see Table 13)."

(18) p. 88, Sec. 8.1, line 5: it's "average gamma_c ~ 5".

(19) p. 97, Refs.: (a) Vermeulen et al. has a page number now: 541.
(b) York should follow Yee, and it should have no period at the end.

(20) p. 101, Fig. 9: the "vector scale" sentence should precede "a" and "b" for consistency. And, as said above, maybe "a" should be enlarged to be "South-west lobe and jet".

(21) p. 102, Fig. 14: should write "2 mJy", not 2 milliJy, for consistency.

(22) p. 107, Fig. 31: is the vector scale 1" corresponds to p=0.33 or 0.25? Maybe I'm the only one confused here.

(23) p. 110, Fig. 44: refer to left panel as "a" and right panel as "b".

(24) p. 110, Fig. 45: add that triangles are upper limits.

(25) p. 112, Tables 17 & 18: can both titles be kept short by using simply "of Radio Features" in each?

THAT'S IT!! Hope this reaches you before you hit the road. Have a good trip, and I'll check in with you next week.

-Dave

From root Tue Nov 16 00:47:51 1993
X-VM-v5-Data: ([nil nil nil nil nil nil nil nil nil]
["7714" "Mon" "15" "November" "93" "23:41:26" "CST" "David Hough" "dhough@physics.Trinity.EDU " nil "171" "Comments on Latest Draft of CJ Paper" "^From: " nil nil "11"])
Received: from vm1.tucc.trinity.edu by polaris.cv.nrao.edu (AIX 3.2/UCB 5.64/4.03)
id AA32546; Tue, 16 Nov 1993 00:47:48 -0500
Received: from physics.Trinity.EDU by VM1.TUCC.TRINITY.EDU (IBM VM SMTP V2R2)
with TCP; Mon, 15 Nov 93 23:49:08 CDT
Received: by physics.Trinity.EDU (4.1/SMI-4.1)
id AA17158; Mon, 15 Nov 93 23:41:26 CST
Message-Id: <9311160541.AA17158@physics.Trinity.EDU>
From: dhough@physics.Trinity.EDU (David Hough)
To: abridle@polaris.cv.nrao.edu
Subject: Comments on Latest Draft of CJ Paper
Date: Mon, 15 Nov 93 23:41:26 CST

San Antonio, TX
November 15, 1993

Alan:

I've gone over it all again, except careful rechecking of the latest tables and figures you sent in the U.S. mail (that will have to wait until the weekend now, I'm afraid). Here goes:

(1) p. 2, line 8: "...knot closest to the central feature is is usually the brightest UNTIL THE JET IS CLOSE TO ITS TERMINATING HOT SPOT(?)."

(2) p. 3, line 16: worth adding at the end that the most ABRUPT change in angle gives the strongest jet h.s. prom.-eta anti-correlation?

(3) p. 4, line 5: hyphenate "milliarcsecond-scale".

(4) p. 40, line 23: comma between "long" and "relatively".

(5) p. 46, lines 13-14: amend plot symbols here to match new figure captions and figures: 1-d profiles = filled circles, 2-d models = open circles, 2-d model limits = open triangles.

(6) p. 55, line 16: r= -0.76, and it's now Table 21.

(7) p. 63, line 10: it's not really true that the normalizing flux densities have a smaller range than the straight jet flux densities, right? So we should be careful here.

(8) p. 63, line 22: "..supported by three other lines of argument." (Just confirming what I think we agreed on during your visit here).

(9) p. 64, line 2: ...and we agreed to add a sentence here about the slope of 0.63 +/-0.12 being less than the value of unity expected for false correlations due to common normalization.

(10) p. 64, line 16: remove stray comma after "straight jet segments".

(11) p. 65, line 11: as I believe you caught, should read "..significant at the 1.0% level or better (|r| > 0.68 for..."

(12) p. 65, line 19: as I again believe you already have, note that the correlation is strongest for ABRUPT deflection of jet.

(13) p. 66, line 1: "The strengths of these..."

(14) p. 66, line 13: need right parenthesis following "eta_1c".

(15) p. 67, line 11: why drop mention of correlation tests involving counterjet and counterjet hot spot prominence? ?

(16) p. 68, line 2: it's "r = -0.05" for central feature prominence.

(17) p. 68, line 14: need comma after "jet deflection angle".

(18) p. 68, lines 22-23: "Larger extended flux density RATIOS between the lobes CORRELATE with larger..."

(19) p. 69, line 3: remove one of the two "is"s.

(20) p. 69, line 5: "asymmetric" typo.

(21) p. 69, last two paragraphs: wholesale deletion of these, please; all points addressed elsewhere now.

(22) p. 70, line 13: "S-symmetry" should have BOLDFACE "S".

(23) p. 73, lines 17-21: we agreed to drop the two regression line business and just give the new 0.63+/-0.12 value, and perhaps mention that the constant-velocity slope is 1.30 (see Section 7.1).

(24) p. 73, line 23: Can we say something besides "The other side of this coin"? Foreign readers might choke on that one, for example.

(25) p. 74, lines 20-22: we agreed to rearrange this, splitting the first sentence into two, mentioning the ABRUPT bend correlation being particularly strong, and going on with "As these trends imply..."

(26) p. 76, line 23: slope now 0.63.

(27) p. 77: we should probably mention just before presenting the slope equation that we've also assumed a constant intrinsic prominence each source would have in the absence of beaming, both for the c.f. and straight jet.

Just after the equation, we're now using alpha_c=0.0, and the slope of 0.63+/0.12 now corresponds to gamma_j = 1.8+/-0.2 (that's really what it is, so why don't we stick with that since it's formally what comes out of the calculation?). Also, we should say that undecelerated slope is 1.30.

And finally, I must point out an apparent contradiction between what we seemed to agree on here and the last sentence of the full paragraph after the equation, about the slope but not the strength of the correlation depending on 3C68.1 & 3C351. In fact, I thought we had agreed that we wouldn't even be concerned about the different slope you get in the absence of these two sources BECAUSE the correlation doesn't reach a high enough level of significance to "enter the game". So if we say that, aren't we saying the STRENGTH of the correlation DOES depend on these two sources? Some numbers to ponder:

Without 3C68.1 & 3C351: r=0.67, which might be only ~90% significance level doing a rough extrapolation from your simulations? And the biassed slope is 0.72+/-0.26, unbiassed with York's c=3.0 is

0.87+/-0.29, w/c=1.0 it's 1.10+/-0.37.
(By the way, just for encouragement, I did check the "A" slopes for all 13 sources, correcting an error I made while you were here, and I now get a pleasant 0.54+/-0.13 for c=3.0, 0.60+/-0.14 for c=1.0; biassed was 0.51).

(28) p. 79, lines 19 & 21: we agreed to excise "tired" stuff here and stuff it at the end in 7.4.

(29) p. 83, lines 5 & 15: same as comment (28) just above.

(30) p. 83, lines 5-9: mention synchrotron lifetime argument presently here back in 7.1 for the first time instead, as we agreed, and make it briefer here since the details will now appear in 7.1.

(31) p. 86, bottom: THE "tired jet" model sentence now appears here, as we agreed.

(32) p. 93, refs.: ADD York, D. 1966, Can. J. Phys., 44, 1079.

(33) p. 104(106?), Fig. 45 caption: say "(a- left panel)" and "(b- right panel)", since we refer to 45a and 45b in the text now.

Some afterthoughts:

(A) We might do well to acknowledge not only that the bends we measure are apparent bends, but that we expect no particularly strong tendency for the intrinsic bends to be preferentially amplified or diminished, on average, by projection.
For example, in Readhead et al. 1983 (ApJ 265, 107), Fig. 12 shows apparent bend as a function of azimuthal angle for a family of curves giving the initial direction between some bit of jet and the line of sight, assuming an intrinsic bend of 10 degrees. Fig. 11 then shows the probability of observing a certain apparent bend. For randomly oriented sources, about 60% have < 10 degrees apparent bend, 35% have 10-20 degrees. For a 0-60 degree biassed orientation sample, the proportions become about 45% each in the 0-10 and 10-20 degree apparent bend ranges (e.g., my thesis has these calculations, and for other intrinsic bend angles as well). So the bottom line is it's probably just fine to use our results for the apparent bends, but maybe we should have one sentence stating that we expect no strong correction to larger or smaller typical intrinsic bends. Does that make any sense? I don't know any more, it's getting awfully late.

(B) It seems that Owen and Puschell (1984) and Scheuer and Readhead (1979) are at least two examples of papers that argued for gamma_j~2. Should we not acknowledge (at least) these (Tony Readhead has kindly suggested we might do so, at least for the latter reference)?

(C) You know, I finally thought to look at the r distribution for the 30 prominence-prominence checks I did, not just against your simulated distribution, but against the normal distribution. Turns out that it's a much better fit for the latter (reduced chi-square only 2.5, as opposed to 9.4 for the former, using 4 bins in r). Just makes me feel a bit better about the common normalization problem.

Well, that's it from me until after I get back from DC. I might answer a quick e-mail, but otherwise I'm out of commission. I hope you and Robert are doing well on that bit of Sec. 7 reorganization - sounds appropriate to me. Give him my regards, by the way.

-Dave

Hello all,

This is to accompany the new .ps version of the paper coming your way separately. It is an attempt to list the main changes that have come out of the meetings I had with Dave in San Antonio, Jack in Las Cruces and Robert in Socorro.

1. Dave and I found an algorithm for handling the prominence-prominence slope statistics that takes account of the existence of errors in both variables, and have used it to re-estimate the slopeand error of straight-jet versus central feature prominence relation. Because we have a 3:1 ratio of median variance between the jet prominence data and the central feature prominence data the answer is not far from the original "Y on X" regression: 0.63 +/- 0.12. It seems that Dave, Jack, Robert and I are all content to go with this result, so we collectively hope that Colin agrees!

2. Similar agreement that we should compare with the expected slope for alpha_c = 0, not alpha_c = 0.2, i.e. with a slope of 1.30.

3 The false-correlation simulation that I ran for the prominence-prominence data also shows that the significance of the correlation, not just its slope, rests on 3C68.1 and 3C351.

The discussion of all of this in Section 7.1 has therefore been updated. Because the simulation suggests we might be dealing with only a two-sigma _correlation_ significance, we are enlarging the discussion in Section 5.5.2 that supports the reality of the correlation. Colin has argued that the simulation may be a "worst case" comparison, and several lines of evidence agree with this. Section 5.5.2 is intended to bolster the case, so please read it particularly carefully to see if you are satisfied with the outcome.

4. We should run the counterjet prominence correlations that contain upper limits through Feigelson's ASURV package (which may also contain some tools for the "errors in both parameters" regression). I will look into this here (Socorro) and report results. If this doesn't pan out, I'll run the data through ASURV when I'm back in C'ville (2nd week of December). Present verion of paper just has notes about where ASURV will be run.

5. Jack suggested we drop Figs 9a, 10b, 10c. Robert agreed. Any objections from Dave or Colin?

6. Jack also suggested that we should make a grey scale to draw attention to the counterjet in 3C215, as this does not stand out well on the contour plots. As A.J. did a terrible job on reproducing the photographs that Jack prepared for the Fernini et al. radio-galaxy paper, we think we should try to make a photograph and an AIPS GREYS bitmap. I have sent Jack a bitmap that may be suitable, and he will try to make a good photograph also. I have also added to the text on p.22 in the hope of guiding novices to find a weak ridge-like feature on the contour map!

7. Robert noticed that we had forgotten to look into the correlation of VLBI extended-feature sidedness with our large-scale jet sidedness in this sample. Quick check by Dave of which central features have enough closure information shows

that we know the answer for 6 of the 13, and all 6 correlate "as expected" from the general pc-scale to kpc-scale relation. Paper will now point this out and use the sidedness correlation to bolster the relativistic-jet discussion. At present this VLBI evidence appears only at the end of Section 5.1, and its consequences surface throughout Sections 6, 7 and 8. It might better be introduced source-by-source in Section 4?

8. Dave suggested we should make more of the fact that the hot spot prominence correlates best with the "abrupt deflection" measure eta_3c. Jack, Robert and I all agree, so it's emphasized in several more places, including the abstract.

9. Robert points out that the exponent of the beaming relation can be outside the range 2+a ---> 3+a if the fields are well-ordered (because of the relativistic aberration correction, esp for B-parallel to the jet). This is now mentioned in Section 6.2 (p.72).

10. Robert and I felt that the existing Section 7.2 confused several issues and needed a major rewrite. First problem was that it contained some discussion of dissipation that had nothing to do with _asymmetries_ in dissipation. Second problem is that it did not distinguish clearly enough between the models that invoke large-scale environmental asymmetries as the source of the asymmetric dissipation and those that build them in from the start of the jets. We believe that the former class of models is untenable, and now say so explicitly. We also felt that the discussion of the second class was over-simplified and have re-written it to be more general. So Section 7.2 now has two subsections, one dealing with each class, and the material in the old section 7.2 dealing with dissipation t hat is not necessarily asymmetric has been moved elsewhere.

11. "Elsewhere" is mainly a new section 6.5 which tries to collate collate the generic evidence for links between the central features and larger-scale properties in 6.5.1, and for interactions that modify jet properties in 6.5.2. There's no overall increase in text, I've collected this stuff from Section 7 by stripping out the material that was not actually model-specific. I think we may be repeating ourselves a few times still, however, and I think we may want to check the overlap between 6.5 and other parts of the paper rather carefully.

12. Section 7.1 has been streamlined and strengthened. Robert and I both felt that the inhomogeneous-jet hyopthesis should be spelled out more clearly. I've rearranged the order of some of the material to make the logic of this section clearer and there's now a section 7.1.1 containing everything that we believe needs to be said about the inhomogeneous jet case. Colin especially should scrutinize this section, as it's the portion he's least likely to feel comfortable with. Jack has suggested that we should include the derivation of the slope relation on p.78. I suggest this should be an Appendix if we do include it. Any comments?

13. Section 7.3 has been streamlined a bit and the additional argument against "pure" flip-flop from the depolarization asymmetry (a large scale medium can't change sidedness as rapidly as a supersonic jet can) has been added to the

optical evidence.

14. Robert and I felt a major effort was needed to make the paper end on a more positive note. Section 7.4 has been replaced with a Section 8 that contains the more positive conclusions plus the stuff on comparisons with other samples and on how to test the relativistic-jet models in future in the light of what we;ve found here. The more low-key statements have been deleted or shuffled elsewhere, so that the finale is more rousing, though Colin in particular may now feel that it's too oriented towards the relativistic-jet models. I've also added some words about counterjet detection rates to the material on the comparison with the Fernini et al. RG's.

15. Robert and I got re-focussed on the issue of what happens when invisible counterjets start to bend. We went through asking what sorts of features lie just off the straight-jet axis in the counterjetted lobes in which we did _not_ identify counterjet candidates. This grew out o discussing the "hook" features in 3C334 and 336, which were classified as counterjet candidates. We also have 3C432 as an example of one of these seen at low resolution, and we were wondering if there might be any further such candidates. Looking at the images from this point of view, we noticed that the straight jet axes in 3C47 and 3C175 do not point at the counterjetted hot spots or candidates but into ridges that _hook_ into these hot spots. In 3C47 the ridge is in the middle of the counterjet lobe and is poorly defined (this is lower-resolution data than for the others). In 3C175, the ridge is the upper arc of the U-structure around feature O, and is clearly the narrowest thing in the lobe outside the hot spot. We debated whether to relabel the diagrams to draw attention to these features, but decided not to. For 3C47, the resolution is marginal, and the situation in 3C175 closely resembles a hot spot feature that I've mapped in 3C353, in which a narrow curved feature connects to the hot spot from one side, but an unimpeachable jet hooks into the spot along a different, but parallel track. We therefore decided simply to point out the existence of these features in the text, but not to raise the issue of whether they could be counterjet candidates loudly enough to second-guess our earlier decisions.

16. Numerous small changes. Far too many to itemize, but mostly just clarifying details. I'll assume that everyone will read the whole text once more before it goes off, and will then check whether the changes they suggested have been made. If you don't notice, or disapprove of, the small changes that others have suggested while you're doing this, I'll assume all is well. One disadvantage of my "road trip" was that some feedback came to me by old-fashioned routes like conversations over a beer or pieces of paper, and I can't circulate an E-record of every single change to you all. From here on, I ask that anyone proposing changes do so by E-mail **to the whole group simultaneously** so everyone has a list of, and a chance to comment on, the proposed changes right away.

Colin -- we missed your input while you were on the road and NEROC was off the Internet. You may, probably will, find this version weighted further towards the relativistic jet picture than you like. I've also not included your point about hot spot multiplicity asymmetries because I was not sure it's consistent with what we are saying about there being no significant inhomogeneity asymmetries outside the hot spots as we now define them. When you get the new .ps file please wade in with non-relativistic jet perspectives and further stuff on the hot spot asymmetries wherever you think they are needed, but please also do this soon. There's a strong sentiment for submitting this before Christmas!!