Subject: Re: Proof review, Ap.J. Letters #985296
Date: Wed, 16 Sep 1998 11:05:22 -0500
From: Suchitra Gururaj <sgururaj@journals.uchicago.edu>
To: Alan Bridle <abridle@NRAO.EDU>

Dear Dr. Bridle,

Thank you for your corrections. In regard to your questions about the figures:

We will make the changes you've requested and then determine if anything needs to be done to adjust the length of the paper. The editor will email you with details.

We see what you mean by the lines dropping out in Figure 4. We think this might be a problem in Figure 2, as well. We see that the lines are beginning to drop out in the version we would be sending the printer. (We don't really worry much about the version that you are seeing when you print out the proof pages, as this is a downsampled version of the final print version.)

In order to remedy this situation, I am requesting that you please send the PS files for Figure 2 and Figure 4 to our FTP site. Please revise the figures so that all the lines are slightly heavier. That way, we can ensure that they will all appear in the print and web versions of the paper. The instructions for our FTP site follows. Please just let me know when you place the figures there.

Thank you for your help.

Cordially,

Suchi Gururaj
ApJL

Instructions for the UCPress FTP Site

Please follow these instructions carefully if you
are submitting files by FTP.

ftp to jrnls-ftp.uchicago.edu (128.135.7.201)

log in as ucpress with the password ucp-submit
(you will log into the
/u1/ftp/ucpress/incoming directory)

create a subdirectory, i.e., mkdir <name>,
where name should be unique
(last_name+first_initial of your name, for
example)

cd <name> - this is a very important step: all
files MUST be downloaded into the
subdirectory /u1/ftp/ucpress/incoming<name>

use the appropriate FTP commands to transfer
your files. Authors are asked to submit all
*.tex and *.eps files without any special
formatting (no tar, uuencode, gzip, etc.). For
'large'
PostScript files ONLY a Unix compression
(compress, gzip) may be used to speed up
transmissions (don't forget to transmit

compressed files as 'binary').

quit ftp when you are finished

You will NOT be able to list any files or
directories in the /u1/ftp/ucpress/incoming
directory.
However, you can list the files in your
subdirectory as well as delete any files in this
subdirectory that may have transmitted
incorrectly and need to be replaced. You will
NOT be
able to retrieve any files from this FTP site.

The transmitted files will be moved to another
directory by a member of The Astrophysical
Journal production staff.

Please use these standard file names for all
FTP submissions.

README File containing list of files included
in manuscript submission and any other
information that may be helpful to the
production office

f1.eps, f2a.eps Figure files, for example

tab1.tex, tab2.tex Table files

At 03:59 PM 9/10/98 -0400, you wrote:
>
>
>
>
>Suchitra Gururaj wrote:
>
>> The page proofs of your article, (985296) are now available for
>> retrieval on the World Wide Web (after 12:00 noon Central Time, today).
>
>> Please send your corrections by email to sgururaj@journals.uchicago.edu
>> if at all possible.
>
>
>
>
>I have retrieved and reviewed the proofs of this paper.
>
>Herewith one correction of an authors' error, two questions, and
>a minor (optional) correction of a change made by the copy-editor:
>
>
>1. Authors' error (apologies!)
>==========================
>
>In the first and third sentences of Section 3 ("The Jet and Counterjet")
>the capital Theta and capital Phi symbols must be interchanged. The
>second sentence is correct as it stands, however.
>
>i.e., in LaTeX:
>
>The manuscript section that now says:
>
>"

```
>\section{The Jet and Counterjet}
>
>The transverse widths $\Theta$ of both jets grow only slowly,
>if at all, with increasing distance $\Phi$ from the nucleus,
>after they first
>brighten. Their full opening angles, $\arctan (d\Phi/d\Theta$),
>are $0\fdg85\pm0\fdg26$ (jet) and $1\fdg12\pm0\fdg27$
>(counterjet). These estimates are the averages
>of the regression slopes against $\Phi$
>of five different measures of $\Theta$: FWHM,
>$3\sigma$ and $5\sigma$ isophote separation,
>equivalent rectangular width, and the
>separation of the steepest transverse brightness
>gradients (\cite{swa96a}).
>"
>
>
>should be changed to read:
>
>"
>\section{The Jet and Counterjet}
>
>The transverse widths $\Phi$ of both jets grow only slowly,
>if at all, with increasing distance $\Theta$ from the nucleus,
>after they first
>brighten. Their full opening angles, $\arctan (d\Phi/d\Theta$),
>are $0\fdg85\pm0\fdg26$ (jet) and $1\fdg12\pm0\fdg27$
>(counterjet). These estimates are the averages
>of the regression slopes against $\Theta$
>of five different measures of $\Phi$: FWHM,
>$3\sigma$ and $5\sigma$ isophote separation,
>equivalent rectangular width, and the
>separation of the steepest transverse brightness
>gradients (\cite{swa96a}).
>"
>
>
>2. Question re pagination
>=========================
>
>As Fig. 1 is now to be included within the text, the text runs
>over the 4-page limit.  How is this to be handled?  Are we required
>to shorten the text at this point to accommodate this, or are there
>adjustments that ApJ is able to make to correct this?
>
>
>
>3. Question re Figure 4
>=======================
>
>On the laser printers available to me to check these proofs, the
>dotted lines in both panels of Fig.4 are becoming invisible in places.
>Will this become a problem at the resolution that you will be printing
>at, or is it only a display problem with these proofs?
>
>
>4. (Minor correction request
>============================
>
>In our manuscript as submitted we had italicised the word "minima"
>in the second sentence of the abstract, viz.
>
>"
>\begin{abstract}
>
```

>We have imaged the total and polarized intensity structures of the
>jets in the FR\,II radio galaxy 3C\,353 with transverse resolutions up
>to nine beamwidths using the VLA at 8.4 GHz. Both the polarized
>intensity and the apparent degree of linear polarization
>exhibit elongated {\it minima}
>near both edges of both jets. We interpret these
>"
>
>The copy editor has removed the italics, but kept our italicization
>of the word "apparently" in the penultimate sentence of the abstract.
>If it does not conflict with ApJ rules, we do wish to italicize
>"minima" here, to emphasize the unusual appearance of the main phenomenon
>that we are reporting.
>
>
>I will send the page charge and reprint orders as soon as I hear from
>you re the page count question (item #2) above.
>
>Yours sincerely,
>
>Alan Bridle
>NRAO, Charlottesville
>Tel: 804-296-0375
>

Subject: Your ApJ Letter
Date: Sun, 30 Aug 1998 14:57:52 -0400
From: apjlabs@cfa.harvard.edu
To: abridle@NRAO.EDU

Dear Dr. Bridle :

You are being notified separately that your recent paper has been accepted for publication in ApJ Letters. The abstract of this paper now appears in our list of Letters accepted but not yet published, which is accessible from http://cfa-www.harvard.edu/aas/index.shtml (or directly at http://cfa-www.harvard.edu/aas/apj1_abstracts.shtml).

If you send your full text of this accepted Letter to the xxx.lanl.gov e-print archives, and then send us the identifying number that is provided (e.g., astro-ph/9808128 for Sandquist et al., accepted Aug. 11), we will insert this hyperlink following your abstract, so that the reader is given direct access to your full-text preprint.

For information about sending your LaTeX file to these e-print archives, please see http://xxx.lanl.gov/archive/astro-ph and read the help section.

The Letters Office does not provide technical support for submissions to the lanl preprint server. The contact address www-admin@xxx.lanl.gov should be used for questions. Please note that such submissions are optional, and will not affect the electronic or printed publication of your Letter in any way. Alternatively, if your preprint is available on another server, we would be happy to insert that hyperlink instead.

Letters are published electronically about 4 weeks before the date of the printed issue. At the time of electronic publication the link to your preprint will be changed to a link to the final electronic version; at the time of printed publication, your abstract will be deleted from our Web site.

Yours sincerely,
ApJ Letters Office (On-line Abstracts)
apjlabs@cfa.harvard.edu

Subject: Cover letter for revised text: ApJ Letters paper E98-5296, swain.0423
Date: Mon, 24 Aug 1998 15:36:53 -0400
From: Alan Bridle <abridle@NRAO.EDU>
Organization: NRAO
To: Ap J Letters <apjl@helios.harvard.edu>

RE: ApJ Letters paper E98-5296, swain.0423

Dear Dr. Dalgarno,

Unfortunately, your message to me of July 24 arrived just as I was leaving for an extended trip, so the response has been much delayed.

You stated that:

"Following this message I am sending you the referee's report on your paper 'INTERNAL STRUCTURE OF THE JETS IN 3C 353'. I will be happy to accept a revised version for publication in The Astrophysical Journal Letters."

We have reviewed the referee's second report, and following this message we are sending a file with a version revised as follows:

---

1. Re the referee's concern about showing more of the polarization data:

"I rather strongly feel that readers will want to see for themselves at least the polarization vector map of the main jet, before accepting the interpretation described here. Therefore, if it is not possible to include this as a figure, I think it should be included as a Plate."

This seems irreconcilable with your note to us that:

"For your information, the ApJ Letters now prints its issues on a paper stock that will allow all figures -- color and black-and-white -- to appear within the pages of each manuscript. Hence, all text, tables, and figures, including those halftones and color figures previously called "plates," must now fit within the 4 journal pages allotted for each paper."

As the referee also stated that:

"I have to concede that it would be better not to delay presentation of the current results even if the vector map cannot be included; I will keep my fingers crossed that the you eventually get around to presenting the data fully in a longer paper, as you suggest."

we strongly prefer to go ahead "as is" in this respect, and to present our polarization vector data fully elsewhere.

---

2. Re the referee's second point:

"The discussion of the magnetic field model is improved, but still somewhat ambiguous. The reader needs to know the layout of the lines of sight, or at least their projected seperation in units of the jet radius. Are there 1301 cells per line of sight, or all together, or is this the number of radial zones in the jet model? Were the cells a fixed size (what?) or scaled to the path length through the jet? Are the B distributions zero-mean (I suppose so, but please say)? By

picking separate uniform distributions for B_phi and B_z you get a bias for pitch angles at +/-45 degrees, which is presumably unphysical in the context of Laing-type models, but as far as I can see this will not have a noticable impact on the results."

We have made further small changes to our text in Section 5 to clarify each of these points.

---

3. Re the referee's final point:

"My comment (e) on the asymmetry in Fig. 4 was unfortunately ambiguous. I was referring not to the data, but to the model intensity profiles. In the light of the revised model description I'd guess this (small) asymmetry is just caused by the random fluctuations in the model, although owing to the ambiguities noted above I can't be quite sure of this. It might be worth a mention (e.g. in the figure caption)."

We hope that our further clarification of Section 5 is sufficient to deal with this. The only asymmetries in Figure 4 are below the levels both of the random noise in the data and in the computation. We do not believe that they are worth drawing attention to, and do not wish to add additional text to the caption in the light of this comment.

I hope that the revised paper can indeed now be accepted for publication in the Letters.

Yours sincerely,

Alan H. Bridle

Subject: ApJ Letters paper E98-5296, swain.0423
Date: Fri, 24 Jul 1998 09:40:25 -0400
From: apjl@helios.harvard.edu (Ap J Letters)
To: abridle@NRAO.EDU

July 24, 1998

Dear Dr. Bridle

Following this message I am sending you the referee's report on your paper 'INTERNAL STRUCTURE OF THE JETS IN 3C 353'. I will be happy to accept a revised version for publication in The Astrophysical Journal Letters.

For your information, the ApJ Letters now prints its issues on a paper stock that will allow all figures -- color and black-and-white -- to appear within the pages of each manuscript. Hence, all text, tables, and figures, including those halftones and color figures previously called "plates," must now fit within the 4 journal pages allotted for each paper.

To assist us in the rapid processing of your paper, please take great care in preparing your revised version. In particular, avoid minor errors that then have to be corrected by us or by you.

Yours sincerely,

A. Dalgarno
Letters Editor

AD/lse

Subject: referee report
Date: Fri, 24 Jul 1998 09:40:26 -0400
From: apjl@helios.harvard.edu (Ap J Letters)
To: abridle@NRAO.EDU

Comments for the authors:

This paper has been improved, but there are still a few points where clarification is needed. The major issue remains the presentation of the data.

I rather strongly feel that readers will want to see for themselves at least the polarization vector map of the main jet, before accepting the interpretation described here. Therefore, if it is not possible to include this as a figure, I think it should be included as a Plate.

If the ApJ editors feel this would be an abuse of the regulations (I can't see why it should be; many letters have two or more plates), then reluctantly, I have to concede that it would be better not to delay presentation of the current results even if the vector map cannot be included; I will keep my fingers crossed that the you eventually get around to presenting the data fully in a longer paper, as you suggest.

The discussion of the magnetic field model is improved, but still somewhat ambiguous. The reader needs to know the layout of the lines of sight, or at least their projected seperation in units of the jet radius. Are there 1301 cells per line of sight, or all together, or is this the number of radial zones in the jet model? Were the cells a fixed size (what?) or scaled to the path length through the jet? Are the B distributions zero-mean (I suppose so, but please say)? By picking separate uniform distributions for B_phi and B_z you get a bias for pitch angles at +/-45 degrees, which is presumably unphysical in the context of Laing-type models, but as far as I can see this will not have a noticable impact on the results.

My comment (e) on the asymmetry in Fig. 4 was unfortunately ambiguous. I was referring not to the data, but to the model intensity profiles. In the light of the revised model description I'd guess this (small) asymmetry is just caused by the random fluctuations in the model, although owing to the ambiguities noted above I can't be quite sure of this. It might be worth a mention (e.g. in the figure caption).

Subject: **Cover letter for revised text: Ap.J. Letters paper E98-5296, swain.0423**
Date: Wed, 17 Jun 1998 18:54:25 -0400
From: Alan Bridle <abridle@NRAO.EDU>
Organization: NRAO
To: apjlms@cfa.harvard.edu

Dear Dr. Dalgarno,

The email message immediately following this will contain the text of the Ap.J. Letters paper E98-5296, swain.0423, "Internal Structure of the jets in 3C353" by M.R.Swain, A.H.Bridle and S.A.Baum, revised in response to the referee's comments. We do not wish to change any Figures, so we are not re-sending them. (Please advise if we need to resend the six Figure files.)

We were pleased that the comments were generally favorable, and we thank the referee for a careful reading of the paper and for constructive comments. As the paper's estimated length as submitted was 3.65 journal pages, we do not think it is possible to address every issue raised by the referee in full detail. We have therefore focused on those mentioned in the referee's summary, including the questions of fitting grand-design field structures and of more quantitative assessment of the case with relativistic motion.

In response to the referee's specific comments (in quotes), we have done the following:

"a) What is meant by "opening angle" in Section 3: W/L or dW/dL? Is the implied jet width (W in my notation) measured to "zero", half-intensity, or what? I'd guess dW/dL and FWHM, but it wouldn't hurt to make this crystal clear."

We have briefly explained how our estimate was derived from a variety of width measures. We have added an explicit definition of "opening angle".

"b) I'm not happy with the word "implies" in the first sentence of Section 5. Although the explanation given is plausible, Fig. 3 (on its own, anyway) is consistent with non-self-similar structures, e.g. the shell could get thinner and more polarized as the jet gets brighter. The original polarized intensity image gives the authors some information here that they are not passing on to the reader."

Indeed Figure 3 on its own does not require self-similarity. We have reworded the first sentence of Section 5 to make it clear that the combination of Figure 3 with the small opening angle and the constant separation of the "rails" rules out the type of alternative mentioned by the referee.

"c) It is rather strange that in a paper entirely devoted to the analysis of the polarization data, the actual polarization maps (including angle) are not presented; I'd be happier if they were (at least for a close-up of the jet region), not just because of item (b) but also (for instance) to help the reader assess the quality of the "assumption" in Section 5 that the lobe field is everywhere perpendicular to the jets, and to display in detail the anomalous knot J1: here is the strongest knot and the weakest lobe emission; the jet field should dominate here if anywhere, but apparently (Fig 3) is remarkably weak; it would be nice to see what's going on."

While it would be nice to show the raw data fully here, even one polarization-vector display with enough resolution to show all the vectors clearly would put us over the page limit, without addressing any of the referee's other points. Several such displays would be needed to show the Faraday-rotation corrected field structure in the lobe and the suggested J1 details at appropriate resolutions.

The conventional displays are in M.R.Swain's thesis, and we intend to include them in another paper that will describe the whole source structure and polarimetry in detail. For this Letter we hope that it is sufficient to show our data in the averaged form in which we have modeled them (Figures 2 and 4), and in the form of the important correlation between rail depth and jet intensity (Figure 3). As responding to this comment would in effect force us to submit this work elsewhere, we hope that it is acceptable to you if we instead use the remaining space to address the referee's other comments.

"d) In section 5, para 2, the specified field is said to include "random components". It is not clear what is meant here: does the model include a B_random giving Stokes I but no Q and U, or is it simply that the signs of B_phi and B_r are allowed to fluctuate randomly? Is there also a non-random component as seems to be implied? In general the model is under-described; how many cells were there across the jet? what distribution were the B-values drawn from? was a 3-D grid used? (I guess this is why the coordinate system is quoted, but a 2-D axisymmetric grid can also be said to use r,phi,z coordinates!)."

We agree. We have now spelled out the nature of our field randomization and of the gridding used for the computation. The referee correctly inferred that our program allows non-random components even though our final model set these to zero. As we now also mention some "grand-design" computations explicitly, we hope this point is now quite clear.

"e) The profiles in Fig. 4 show a slight asymmetry in total intensity; I can't see how the model described could lead to this, so please explain."

We fully agree with the referee that that total intensity asymmetries, if such were clearly present in the data, would be an important diagnostic. Any axisymmetric field model for a jet in the plane of the sky will give a symmetric total intensity profile; but away from the plane of the sky, a model with a grand-design helix or a flux rope field can produce significantly asymmetric I and P profiles (the lines of sight intersect the field lines at different angles on the two sides of the jet). Perhaps this is what the referee had in mind. But the observed asymmetries, if any, in our data are marginal relative to the noise and therefore are too small to be used as evidence for or against grand-design models.

"f) It seems to me that the data given would also be consistent with a "grand design" helical field model in the sheath, with pitch angle near 45 degrees, although this does not fit in with the turbulent boundary scenario (it might suggest magnetic confinement or a nearly force-free field, for instance). Arguably this model is "fundamentally dissimilar" from the random field (cf. end of Sect. 5), although the ratio of field components remains the same. It wouid be worth mentioning this ambiguity (or explicitly ruling it out)."

We had indeed calculated the emission from some helical and flux-rope field configurations that were previously suggested for radio jets. We have now added a brief section referring to these calculations, and to the discussion of them in M.R.Swain's thesis. The difficulty in simultaneously reproducing flat-topped I profiles and the observed degree of polarization profiles from such models underscores our belief that the constraints imposed by such simultaneous fitting are strong. Possibly publication of this Letter will encourage others to try to fit our data with grand design models of types that we have not explored.

"g) In the last para Section 6, the logic seems to be flawed. The jet asymmetry constrains the emitting region, which is predominantly the "boundary layer" (which actually occupies 1 - (0.43)^2 = 80% of the jet by volume!). Either this boundary is slow, in which case the original model applies (and seems to force i < 30 deg), or else it is relativistic, in

which case beaming forces the jets into the sky plane. In the former case, the 2:1 asymmetry is intrinsic, since residual beaming would require a *large* angle to cause it.

In the latter case in the jet (& cj) frames we should see both jets from behind, thanks to aberration. The model fits imply that in the *jet frame* i > 30 deg. can be ruled out. This could be used to set a useful upper limit on the boundary Lorentz factor -- much better than the present text which simply denies that the boundary could be relativistic, on the basis of the theoretical prejudice that it is the slowest region. This begs the question, how slow? After all, the "boundary" makes up most of the jet, and FRII jets are widely supposed to be relativistic. It has already been suggested that entrainment is not taking place, which eliminates the main deceleration mechanism. None of this affects the possibility of a faster spine, of course."

There is an interacting parameter set of (field component ratio, velocity, inclination). We now spell this out explicitly both when discussing our model, and in the paragraph discussing its relation to the relativistic jet case. We now offer a quantitative example of the sort of relativistic jet structure that motivated our previous remarks, which we agree may have been too brief for clarity.

We also now spell out that there is a regime of high boundary layer velocity and small angle to the line of sight within which the aberration effects could be large while the Doppler favoritism effects are small.

We also point out why we do not believe that the 2:1 intensity ratio can be entirely of relativistic origin, but in this case must have an intrinsic component. We understand the referee's comment about how to set an upper limit to the boundary Lorentz factor; but this limit would not be firm if the field component ratio can differ from unity.

We hope that our rewritten discussion and example clarifies that there is a regime of slow boundary flow in which our description of the fields would still be correct to first order in the jet frame, but also that there is also another regime, more restricted in angle, of fast boundary flow in which it would not.

We hope that the paper as revised is now acceptable for publication in the Letters and we again wish to thank the referee for very helpful comments.

Yours sincerely,

Alan H. Bridle

Subject: revised apj letter text
Date: Mon, 1 Jun 1998 12:35:46 -0400 (EDT)
From: "Mark R. Swain" <swain@astrosun.tn.cornell.edu>
To: Alan Bridle <abridle@NRAO.EDU>

Alan,

I went through the revised draft you sent and thought it nicely crafted. I particularly liked the new discussion about the relativistic jet scenario and the limits to our model. If you have a chance, you might consider changing the wording in the first sentence of section 5, paragraph 2 to

... coordinates in a cylindrical jet which is axisymmetric about the z axis and inclined by angle i to the plane of the sky.

I think it might read a little smoother that way.

Mark

Subject: referee report
Date: Wed, 20 May 1998 10:34:50 -0400
From: apjl@helios.harvard.edu (Ap J Letters)
To: abridle@NRAO.EDU

1 This paper contains substantive new results, viz: by far the best transverse resolution of a "normal" jet in a powerful (i.e. FRII) radio galaxy. The image of the whole source (Fig 1) is also extremely interesting. The authors claim to derive the intrinsic magnetic field structure with some confidence, but I am not entirely convinced, for two reasons. First, they do not consider the possibility of a grand-design field structure, such as expected in a magnetically dominated jet; second, they do not make a convincing case that the emitting regions are not in relativistic motion, which is a crucial assumption of their model. However this model is certainly plausible and I think the paper should be published after these points have been addressed.

done.

2 This paper will interest all workers on radio jets, and therefore merits publication as a letter.

3 The paper seems to be brief enough for a letter and is certainly entirely to the point. If anything, the clarity of the paper has suffered from trying to compress quite a complex piece of science into the 4-page limit.

4 The background is generally sketched adequately; the general reader might appreciate a reminder of the significance of the FR classification, and its conventional interpretation in terms of supersonic vs. turbulent jets. An extra paragraph in the introduction is about all that could be fitted in given the space constraints.

5 The text is mostly very clear but I have a few comments:

a) What is meant by "opening angle" in Section 3: W/L or dW/dL? Is the implied jet width (W in my notation) measured to "zero", half-intensity, or what? I'd guess dW/dL and FWHM, but it wouldn't hurt to make this crystal clear.

b) I'm not happy with the word "implies" in the first sentence of Section 5. Although the explanation given is plausible, Fig. 3 (on its own, anyway) is consistent with non-self-similar structures, e.g. the shell could get thinner and more polarized as the jet gets brighter. The original polarized intensity image gives the authors some information here that they are not passing on to the reader.

constant rail separation slow spreading here

c) It is rather strange that in a paper entirely devoted to the analysis of the polarization data, the actual polarization maps (including angle) are not presented; I'd be happier if they were (at least for a close-up of the jet region), not just because of item (b) but also (for instance) to help the reader assess the quality of the "assumption" in Section 5 that the lobe field is everywhere perpendicular to the jets, and to display in detail the anomalous knot J1: here is the strongest knot and the weakest lobe emission; the jet field should dominate here if anywhere, but apparently (Fig 3) is remarkably weak; it would be nice to see what's going on.

d) In section 5, para 2, the specified field is said to include "random components". It is not clear what is meant here: does the model include a B_random giving Stokes I but no Q and U, or is it simply that the signs of B_phi and B_r are allowed to fluctuate randomly? Is there also a non-random component as seems to be implied? In general the model is under-described; how many cells were there across the jet? what

Subject: Re: email outage
Date: Wed, 29 Apr 1998 11:56:21 +0100 (BST)
From: Robert Laing <rl@ast.cam.ac.uk>
To: Alan Bridle <abridle@NRAO.EDU>

Guess who deleted the copy. What I think I said, was:

I have done the (somewhat messy) analytical part of the calculation needed to deduce the field structure evolution in the transition region. It is irritating that most of the complexity comes from an entirely arbitrary choice of flow pattern, but the only simple solution involved abrupt kinks in the streamlines, so I suppose anything else would be just as bad. I am in the process of coding and testing this stuff. In the process, I belatedly realised that the spine flow pattern is inconsistent, in the sense that there is actually some velocity shear (the flow lines bend, but the velocity depends on distance from the nucleus). I would like to avoid this by setting zeta1 = zeta0 (=> conical spine). They are, in any case, fairly close. Any objections?

I skimmed your 3c353 paper at the weekend. You will be unsurprised to know that I am happy with the conclusions! I did think that you could have said a little more about some of the relativistic aspects. If the boundary layer has a single velocity, then the effect of aberration on the degree of polarization will be equivalent to a rotation. If t is the angle between the jet and the line of sight, then the polarization is that from a slow jet at an angle t_r given by

sin t_r = D(t) sin t

where D is the Doppler factor. So I would have thought that you could get this from your calculations of jets at different angles to the l of s? I think that the penultimate sentence of section 6 is not quite right: even if the jets are in the plane of the sky, the equation above shows that there are substantial effects on the polarization if the speed is high (e.g. t ~ 90 deg and D(t) << 1). If this is really the case (which it probably isn't) I think you might have to play around with the field component ratios.

Regards

Robert

Subject: Re: email outage
Date: Wed, 29 Apr 1998 17:13:18 +0100 (BST)
From: Robert Laing <rl@ast.cam.ac.uk>
To: Alan Bridle <abridle@NRAO.EDU>

On Wed, 29 Apr 1998, Alan Bridle wrote:

> Just to make sure I don't get lost here ... BETA is a function of RHO only, but
> between RHO1 and RHO0 the flow streamlines are non-conical and so appear to
> diverge from somewhere other than RHO=0 ... so there is a velocity gradient on
> surfaces perpendicular to the streamlines because in this region these surfaces
> are not surfaces of constant RHO. I guess the effects are second-order unless we
> model a very rapid expansion somwhere

Exactly so. It's not a big deal, but it is yet another detail to explain.

> but I agree that we have no strong reason
> to suppose that the boundary of the spine scales with the outer boundary. In
> making the spine monolithic (v a function only of RHO and B_z = 0 everywhere)
> we've swept a lot under the rug already.

The value of zeta1 has never been very well constrained in any case. The fact that zeta0/zeta1 is not equal to xi0/xi1 immediately tells us that the spine/sl structure isn't self-similar, so most of the simplicity disappears.

> In making the two boundaries scale we
> were trying to make a simple assumption for the geometry ... but I agree that a
> single cone may be simpler for the physics. It all begs the question of how
> the "news" of the slowdown in the shear layer is communicated to the spine ...
> might as well do whatever turns out to be comuptationally simplest (or is it
> much too late for that now?)
>

No difference really, since once we allow streamlines that aren't straight somewhere the hard work starts, and the same routines work for spine and shear layer. We can, if we want, have a pure shear layer (e.g. with a Gaussian velocity profile) rather than a distinct spine. The data tell us, however, that they want a lower emissivity in the central region as well, and so the concept of a real spine still holds up. The communication of stress, whilst easy in the RGO, appears to be tricky in radio jets. People have argues that magnetic viscosity can produce a flat-topped velocity profile (because the field increases to resist shear stress) but I wouldn't care to put that in the same sentence as remarks about passively convected field!

> In detail, yes, we have three interlocking variables, t, D(t) and the ratio
> between B_z and B_phi components in the jet frame. To this zeroth order it's
> the same problem that we had with the 3C31 counterjet ... pure B_z is much too
> good at making B-parallel and high degrees of polarization, but the moment you
> let B_phi loose in (rough) equipartition, you can fit the data. Maybe we should
> reword it a bit to take account of that, as it may overstress the equality right
> now. As we don't have enough jet signal to do a more detailed analysis in this
> case, especially with the lobe confusing the polarimetry, I was actually trying
> to avoid the detailed relativistic numbers game here! It's of course a
> tightrope walk between doing that and pointing out the possible connection to
> the FR I model and to the "missing" central emissivity!
>
> Thanks for the comments! It's a naive level of detail compared with 3C31 but I
> think it's helpful to know that some of the same stuff exists on the other side
> of the FRI/II boundary. Too bad we can't the same number of beamwidths across a
> few more with good signal to noise!
>
>

I appreciate your problem. If I were you I would just acknowledge the fact that a fast boundary layer changes the rest-frame theta. You could probably say in a few words that the (qualitative) options are:

- everything non-rel; various combinations of theta and field ratio;

- slow boundary layer and fast spine; theta not too small (else would see spine) => B_phi and B_z roughly equal;

- fast boundary layer; theta close to 90 (else large j/cj ratio); various possible combinations of field ratio and speed.

I agree that it isn't worth going further, esepcially in a Letter.

Cheers

Robert

Subject: **p.s.**
Date: Wed, 29 Apr 1998 12:38:28 +0100 (BST)
From: Robert Laing <rl@ast.cam.ac.uk>
To: Alan Bridle <abridle@NRAO.EDU>

In the 3c353 paper, I suspect you mean <B_phi^2>^1/2 / <B_z^2>^1/2 = 1.

Robert

Subject: Submission to ApJ Letters, swain.0423
Date: Thu, 23 Apr 1998 17:46:05 -0400
From: apjl@helios.harvard.edu (Ap J Letters)
To: abridle@NRAO.EDU

Dear Dr. Bridle,

Thank you for sending your manuscript to ApJ Letters.
I have successfully printed your text and 4 figures and
will forward them to the editors for their consideration.

At this time, we request that you refrain from sending
any additional files until your paper has been through
the referee process and an editorial decision has been
communicated to you. Thank you for your understanding.

Yours truly,

Carolann Barrett
ApJ Letters

Subject: Swain et al. manuscript #0: information
Date: Thu, 23 Apr 1998 13:26:42 -0400 (EDT)
From: Alan Bridle <abridle@NRAO.EDU>
To: abridle@polaris.cv.nrao.edu

```
------- start of forwarded message (RFC 934 encapsulation) -------
Content-Length: 834
Received: (from abridle@localhost) by polaris.cv.nrao.edu (8.8.5/8.8.0/CV-2.2) id
MAA15498; Thu, 23 Apr 1998 12:53:17 -0400 (EDT)
Message-Id: <199804231653.MAA15498@polaris.cv.nrao.edu>
Content-Type: text
X-Status:
X-Keywords:
X-UID: 3
From: Alan Bridle <abridle@NRAO.EDU>
To: apjlms@cfa.harvard.edu
Subject: Swain et al. manuscript #0: information
Date: Thu, 23 Apr 1998 12:53:17 -0400 (EDT)


To: Editorial Office, ApJ Letters

From: Alan H. Bridle, NRAO

Re: Electronic submission of manuscript


"Internal Structure of the Jets in 3C353"

by

Mark R. Swain, Alan H. Bridle and Stefi A. Baum


The contact for correspondence about this manuscript is

Dr Alan H. Bridle
National Radio Astronomy Observatory
520 Edgemont Road
Charlottesville
VA 22903

email: abridle@nrao.edu
phone: 804-296-0375
FAX:   804-296-0278

Page charges will be shared 2/3 by the NRAO and 1/3 by STScI.

The manuscript will be sent to you in 7 further messages

1: main text (20 KB)
2: eps file for figure 1 = Plate 1 (926 KB)
3: ps file for upper half of figure 2 (13 KB)
4: ps file for lower half of figure 2 (12 KB)
5: ps file for figure 3 (12 KB)
6: ps file for upper half of figure 4 (12 KB)
7: ps file for lower half of figure 4 (12 KB)
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NATIONAL RADIO ASTRONOMY OBSERVATORY

520 EDGEMONT ROAD CHARLOTTESVILLE, VA 22903-2475

Dr. Alan H. Bridle email: abridle@nrao.edu Tel: 804-296-0375 Fax: 804-296-0278
http://www.cv.nrao.edu/~abridle

February 13, 1998

Dr. Stefi Baum,
Space Telescope Science Institute,
3700 San Martin Drive,
Baltimore,
MD 21218.

Dear Stefi,

Here's the printout and a larger scale rendition of the grey scale image for the paper on 3C353's jet.

The postscript file that generates the grey scale is of much higher quality than this printer rendition of it, and I'm wondering if you have any better output device available at STScI for getting such things onto glossies for publication. It would be nice to get a good display of this image into the literature so I'd be glad of your ideas about that, not having had a great deal of luck with grey scale displays of such complicated images recently!

I think the results from this source open the possibility that much of what we see in FR II jets in the radio is emission from a boundary layer, and this is not a concept that has been talked about much in the literature yet, although Robert (Laing) and I did write it into the analysis of the apparent jet deceleration we deduced for the 3CR quasar jet sample. The idea that B_z and B_ϕ are in rough equipartition in such a layer was not the first thing we thought of, but I do think that it does make some physical sense, and it seems to be strongly required by the data both here and in the outer layers of 3C31 (which are also about half of the jet diameter, as it turns out).

The picture I have for this in general is that the velocity shear tries to turn B_r into B_z, and that B_z would be increasing without limit in a strong shear except that magnetic stresses and probably reconnection won't allow this. B_ϕ is then the obvious candidate for relieving the situation as it is first-order unaffected by a velocity shear. We are not talking large-scale unidirectional fields, by the way, just how the locally random components are distributed; the field is like Robert's "squashed" random loops, only instead of being in a single plane they are here in planes that are locally tangent to the jet cylinder. The key ingredient is that there is no B_r component across the velocity gradient.

This one has been a long time coming but I think it is now a lot more solid than what we said at the Alabama meeting.

Best wishes,

From: "Mark R. Swain" <swain@astrosun.tn.cornell.edu>
To: abridle@nrao.edu
Subject: 3C353 paper
Date: Tue, 5 Nov 1996 15:55:40 –0500 (EST)

Alan,

Some long over-due comments.

1) Section 2, sentence 1; "The observations ..." I propose changing to "The 8.4 GHz observations".

2) Section 2, sentence 2; "... calibrator ..." I propose changing to "... calibrator NRAO 530".

3) Section 3, item 3; "(quantify this)"
Excluding positions J1 and J4, the approximate, unweighted average of the positive points (points 4 through 9 counting from the left) in Figure 5.21 of my thesis is 1.1E-4 Jy with a rms of 2.0 E-4 Jy. These values for these points were estimated with a plastic ruler and so are approximate; considering the errors though, I think the above numbers would suffice.

4) Section 3, paragraph 1, sentence 3; "Figure 1 shows these rails and ...." I propose changing to "Figure 1 shows the rails, which range approximately between $0\farcs96$ and $1\farcs44$ in FWH minimum, and ...".

5) Section 4, paragraph 3, sentence 3; "So, if the..." I propose changing to "If the ...".

6) Section 4, item 2; "(e.g. references)"
I am likely to agree with what ever references you have in mind.

7) Section 5, paragraph 2, sentence 2; "r > 0.43" I propose to change to "r $\stackrel{>}{\sim}$ than 0.43" or what ever the TeX equivalent for "approximately greater than or equal to" would be.

8) Section 5, paragraph 4, sentence 3; "Its field configuration" I propose changing to "Its average field configuration".

9) Section 8; I propose that after item 4 we include:
"Thus the jet deceleration scale and orientation determine the observed morphology. A possible test of this "unified scheme" would be measuring the transverse profiles of several quasar (of similar red shift) jets; the proposed "unified scheme" predicts that the quasar jets will be center-brightened rather than "flat-topped"."

10) Figure 1; I propose making the caption read,
"Averaged transverse profiles along the jet (top) taken from the straight section between the knot at which the jet is first detected near the nucleus and the knot immediately prior to the bend to the south-west where the jet enters the hot spot. The designations NR and SR mark the position so the north and south polarization minima (rails) associated with the edges of the jet. Similar features are found for the counter jet."

11) Can't we include the 8.4 GHz polarization image to try and show the rails directly? We don't even have to show the entire galaxy; maybe just the region around the jet. If we are too short on space, I would replace the observing log with a polarization image. Actually, I think a image of just the jet region at 8.4 GHz in total and polarized intensity,

side-by-side would help clarify _both_ the flat-topped claim and what rails look like in an image. Let me know if you want me to make this "side-by-side" picture.

Other than the above comments, I feel like the paper is "ready to go". I would like to try and get it submitted fairly soon if that will work into your schedule.

Now a word about what has been going on around here with SPIFI. We are months and months behind schedule, very low on money, and highly visible. We also have some problems with our three-way collaboration (actually a four-way collaboration depending on how you count). The current state of SPIFI is rapidly becoming a liability too two of the PIs. As you can well imagine, the pressure is always high and, at times, seems extreme. Among the three groups, the finger-pointing at which group is causing us to be behind is, in my opinion, well into the relm of nonproductive. Naturally, the local graduate student and I are working as much as we can; which is more or less all the time. Money is so tight that the only way I could get the other graduate student here to help us was if I agreed to let him live with Melanie and me; he has stayed at our house for five weeks so far though not all at once. There is also a potentially embarrasing gap between how the instrument has, on occasion, been represent and what its true state is.

Things are far from hopless and I think we will pull through and have a working instrument but when is the question; currently, it looks like all aspects of this project, technical, fiscal, and political will get worse before they get better. While the above comments are not top secret, I would appreciate discretion.

All that venting is my attempt to explain why I haven't been communicating regarding our paper. I am departing for the South Pole for a "look-see" trip on Dec. 5. While the Pole does have email, I don't know how well it works and I don't count on doing much non-SPIFI work while down there. When I "come off the ice" as they say, is not clear but I intend to get to Toronto in time for the AAS meeting. If we can try to finish this paper up in the next couple of weeks, I would like to try and do so.

Mark

From: "Mark R. Swain" <swain@astrosun.tn.cornell.edu>
To: Alan Bridle <abridle@nrao.edu>
Subject: Re: 3C 353 paper
Date: Mon, 24 Jun 1996 15:37:25 -0400 (EDT)

Alan,

Over all I like your rewrite of the paper. Once again, you have demonstrated yourself a master of brevity. Here are my initial comments:

1) p. 3, paragraph 4, sentence 3; "unresolved calibrator ??"
The unresolved calibrator you refer to in NRAO 530 for all observations except the reobserving in D configuration at X and C band.

2) p. 2, item 3; "(quantify this)"
the polarization excess you wish to quantify depends on location along the jet (see figure 5.21 in thesis).

3) I don't think this draft includes the estimate of rail width - even though we concentrate on average properties, it seems reasonable to include the estimate of rail width (I think that was present in my original draft).

4) p. 4, item 2 ; "vector- cancellation"
There is an extra space between the hyphen and "cancellation."

5) p. 5, paragraph 4, sentence 2; "r > 0.43"
should be approximately greater than

6) p. 5, paragraph 6, sentence 4; "Its field"
should be "Its average field" - the average filed configuration near the jet axis is not well constrained but the field at specific locations along the jet may be better constrained.

7) p. 7,
After item 4, should we include
   a) pointing out that deceleration scale and orientation govern source morphology
   b) point out that a test of our "unified scheme" is to see if the transverse I profiles of quasar jets are center-bright.

8) Figure 1.
All references to "J1" and "J4" will need to be changed to something like "jet knot nearest the core" and "jet knot nearest the hot spot." Same sort of thing with CJ2.

9) Should I conclude you don't want the show the polarized intensity picture so people can see the rails themselves? I think that would be a great loss to the paper. Also, it we show the polarized intensity picture, we should show the total intensity picture so that people can compare the two. I know the total intensity picture has been published in the Alabama conference proceedings but it would be nice to have it right beside the polarization image so people could make their own side-by-side comparison. I had envisioned both those images sharing one glossy page at the back of the ApJ letters.

My turn-around time for comments will be unpredictable as things are

getting pretty frantic here.

Mark

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November 3, 1986

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

Dear Alan,

Thank you very much for the tape with data (UV) on 3C353, and the map data for 3C219.

Edwin and I have discussed the status of the 3C219 data, and concluded that he probably will not use your data, but stick with his own which has a slightly lower common resolution. I shall get in touch with you again when Edwin has gotten further with the 3C353RM analysis.

Thanks again.

Yours sincerely,

Phil

PPK:ac
c.c. Edwin Zukowski