Response to reviewer's Comments:

This is an excellent paper presenting a wealth of data, a careful theoretical analysis tastefully applied, and even a new kind of relativistic phenomenology in the form of the asymmetric "arc" structures. I recommend publication after minor revision to improve clarity in a few places.

Thank you. We always attempt to stay just within the bounds of good taste. We have addressed all of the comments, which were very helpful (especially in finally provoking us to produce an adequate version of Fig 25, which we had been arguing about, to little effect).

Detailed comments:

Sec 3.1 Description of the Sobel filter seems a an unnecessarily convoluted way of describing Mod(Grad I).

We now say: "The Sobel operator \citep{Pratt} computes an approximation to $\mid \nabla(I)\mid$ and therefore highlights large brightness gradients."

Sec. 3.2 Discussing Fig. 4(a) you say "the appearance of a narrow, steep-spectrum rim" is misleading, but I couldn't see (i) what the incorrect impression is that you think this gives and (ii) what is the correct explanation. Also, the discussion of the same (?) rim as seen at higher resolution in Fig 4(b) (last-but-one para) is not obviously consistent with the previous discussion.

We have tried to clarify this point. Our concern was that the narrowness of the rim might cause it to be misinterpreted as a physically thin sheath of material, whereas we think it is actually a superposition effect caused by a flat-spectrum jet superposed on much broader steeper-spectrum lobe emission with a surface brightness just below our blanking level at 8.5 GHz. The low- and high-resolution images are consistent where they overlap (note that the spectral-index ranges are different, as we needed to display smaller variations in spectral index along the jets at the higher resolution)>

Fig 7/Sec 3.3: Please label which side of these graphs corresponds to which lobe/jet (the reader can work it out but it is not obvious).

We have added labels to the bottom panels in each column and also noted the sign convention in the caption.

Sec 3.4, para 2. I didn't find the figures particularly suggestive of a "jet that turns through 180deg", at least not if "jet" implies any sort of collimation. Maybe you just mean "flow".

Indeed, that is a better term and we now use it.

Fig 16(d) caption: there doesn't seem to be *any* blanking!

The profile was in fact produced from an image blanked as described. But inspection of the profile indeed shows that none of the points on the jet axis were actually blanked! We have amended the caption slightly to make this clear.

Sec. 6.3: You say the X-ray emission out to 5.9 kpc in the jet frame is *before* rapid deceleration; but Fig. 23 shows the deceleration occuring between 4.5 and 5.9 kpc so apparently the X-ray emission includes it.

Somewhat uncertain, owing to the small number of photons in the X-ray image (the uv emission is not detected as far out). We now say: "up to and possibly including the rapid deceleration."

Sec. 6.6 I was less than convinced by the discussion of shear. Obviously if you set the radial field to zero, shear is not an issue, but physically this seems highly implausible. Unlike the other jets modelled, this one is inferred to have a very strong shear flow and it is hard to imagine that this would not result in the generation of some longitudinal field component. Worth discussing in light of the slower-than-expected initial growth of the toriodal component.

We think it is important first to state self-consistent conclusions for our assumed (laminar!) velocity field. In fact, the field-component evolution does not work for such a simple field/velocity combination in any of the other sources we have modelled either.

We are fully aware that the real situation must be more complicated. We will investigate whether adding a small radial component would allow a self-consistent evolution and now promise this. This did not work for 3C31, as we demonstrated in Laing & Bridle (2004) with a self-consistent 2D model (significant effort would be needed to include our latest velocity and field parameterizations in this code, and the present paper is long enough as it is). It seems more likely to us that there is effective conversion from toroidal to longitudinal field, perhaps by ~two-dimensional turbulence. We now also mention this speculative idea (very briefly).

Sec 6.7: Is there any milage in comparing these arcs to the so-called "ring" features in Hercules A?

This is an interesting idea, but one which we would prefer to leave until our attempts to model arc structures are better developed. A quick check of Gizani & Leahy (2003) suggests that the structures in Her A are different from those in the less luminous sources, but we would need to look at the original images to get much further, I think.

Fig 25: This would work better in colour, especially if the blank regions could be set to a neutral colour like grey, clearly distinct from the palette for data values (the same applies to a lesser extent for Figs 4 and 6).

That is a very helpful suggestion. We had been unable to make an adequate monochrome figure (either positive or negative) for the field components precisely because the values at the edges of the jets merge into the background on one panel or the other. We have now produced a colour version of the figure which we think is much better. We have also modified the relevant panels of Figs 4 and 6. All now differentiate clearly between blanked areas (grey) and values which are outside the range of the colour wedge (black or red).

In turn, the revision of Fig. 4 prompted us to remark on the flat spectra seen at the ends of the source (unreliable because of large primary beam corrections).

Sec 7.1 End: the conclusion that adiabatic flow works well after the initial zone can be traced back to Bicknell's non-relativistic models in the 1980s (at least). This early work should be cited.

Here we do not entirely agree. Bicknell's models ASSUME the adiabatic approximation in order to relate the observed surface brightness of the jets to the velocity and spreading rate (as our referee himself noted on pp 165-166 of his chapter in "Beams and Jets in Astrophysics"). It is applied in the flaring regions (where we have shown that it does not work) as well as in the outer regions. Now it is indeed the case that Bicknell comments on the possibility of dissipation in the flaring region of NGC 315 (Bicknell 1986, ApJ 305, 109), but his inferred deceleration has to be much more abrupt than we find in the same source (Canvin et al. 2005) in order to fit the relatively slow decrease of surface brightness.

We have modified items (xv) and (xvi) to make these points and to cite earlier work, starting with Burch (1979).

After submitting the paper, we noticed that the interpretation of the arcs had observed and rest frames the wrong way round in some, but not all, of the description (the numerical results were correct and our conclusions are unaffected). We have fixed this.

We have also corrected a small number of typographical errors and infelicities.

We feel that colour is essential for Figs 4, 6 and (now) 25 and we take it that the referee agrees given the remarks above. We would ask that they be included without charge.

Subject: Next attempt
From: Robert Laing <rlaing@eso.org>
Date: Tue, 9 May 2006 18:31:31 +0200 (CEST)
To: Martin Hardcastle <mjh@star.herts.ac.uk>
CC: Alan Bridle <abridle@nrao.edu>, James Canvin <jcanvin@physics.usyd.edu.au>

Dear Martin

Thanks for the comments.

The following is a response with some remarks from Alan at the end. I've attached the latest version so that everyone can see the changes.

James: have you had a chance to read through yet?

p. 2 Sentence beginning `The lobe containing the counterjet' and ending `it is receding from us'. `it' here clearly means the counterjet, not the lobe: suggest `it is further away from us' or something similar that's applicable to the lobe not the jet.

Agreed. Also slightly rewritten:

"Of the two lobes, that containing the counter-jet depolarizes more rapidly with decreasing frequency between 1.7 and 0.6\,GHz \citep*{GHS}, consistent with the idea that it is further away from us."

p. 9 Sentence beginning `The few other FR I jets...' -- perhaps it would be worth explicitly saying at the end of this that none of these other jets are superposed on lobes, so strengthening the point that it's unlikely that lobe/jet contamination is the origin of the flattening.

Agreed. Now says:

"The few other FR\,I jets observed with adequate resolution and sensitivity (none of which are superposed on lobes) show a similar spectral flattening with distance: PKS\,1333$-$33 \citep{KBE}, 3C\,449 \citep{K-SR}, NGC\,315 \citep{ngc315ls}, and NGC\,326 (Murgia, private communication)."

p. 15 For consistency add commas around phrase `to allow direct comparison with the observations'

Yes

p. 24. i) Could we label the two types of arc in the two relevant figures? I can see what you mean but only after some flipping between pictures and text.

Yes, that's a good idea. I have modified the figures. The classifications into arc types are mostly unambiguous. I have marked two with ?'s (because they are faint). I have not classified the 2 arcs close to the bend in the main jet of 3C31, as they appear to me to be something to do with the bending process (i.e. not axisymmetric). Captions are modified appropriately.

ii) `Terrell 1959' -- presumably you know about the Penrose 1958 reference for this? I usually refer to both since there seems to be a perception that this was purely Terrell's discovery. Up to you, of course.

Penrose reference added, but (as in previous message) still not sure whether 1958 or 1959. It would be good if someone could track down a paper copy.

iii) Is it worth speculating on the physical origins of the arcs? How do sudden changes in emissivity fit into the model of a smooth flow? Are they as periodic in distance as they look?

I don't want to do that here, although the question is indeed an interesting one. We have a reasonable qualitative picture for the arcs, assuming that they are emissivity but not velocity features, but I haven't done detailed calculations. Questions of periodicity are quite complicated, given light-travel time issues and I think we would rapidly get bogged down. I believe that Alan agrees. Defer, I suggest.

No changes made.

p. 25. `(well fitted by a cubic function)' -- think you need to say what is well fitted by a cubic function of what, or delete this (given that it's in the main text and the summary, maybe the latter)

Deleted. I think Alan made the same suggestion earlier.

p. 25 `this region may be present, but unresolved in ...' -- delete comma to avoid ambiguity.

Yes

p. 27. Point (i) at the start of section 7.2 doesn't mention the more interesting possibility that the velocity structure is different in 296 because the jets are embedded in the lobe. This is later hinted at in the final paragraph of the section (though rather weakly, since you say that the lobes are `more prominent', which might lead a reader from the idea of two different *kinds* of FRI that's present elsewhere in the paper) but I wonder whether it would be worth changing the order to draw more attention to it.

That's an interesting one. My views on this have mutated during the writing of the paper, and there are probably some residual inconsistencies as a result. I'm groping for the right words here. How about splitting the first bullet of 7.2 into:

"\item The transverse velocity profile falls to a low fractional value $\la 0.1$ at the edge of the jets in 3C\,296, whereas all the other sources are consistent with an edge/on-axis velocity ratio $\approx$0.7 everywhere, and also with a top-hat velocity profile at the brightening point. This could be related to the differences in lobe structure: as noted earlier, 3C\,296 is the only source we have observed with a bridged twin-jet structure. Its jets may then be embedded (almost) completely within the lobes rather than propagating in direct contact with the interstellar medium of the host galaxy. Modelling of other bridged FR\,I sources would then be expected to show similarly low edge velocities. Whether the lower-velocity material is best regarded as part of the jets or the lobes (and, indeed, whether this distinction is meaningful) remains unclear.
\item Some support for the idea of an interaction between jets and lobes comes from our detection at the edges of the jets in 3C\,296 of spectral and polarization anomalies which are not seen in the other sources we have studied; in particular, we have argued that the anomalous polarization in the outer parts of the main jet (which is inconsistent with our model predictions) cannot be a simple superposition of unrelated jet and lobe emission."

Also delete old bullet (iv); now covered in (ii).

and shortening the final para to:

"Our results are generally consistent with the conclusions of a statistical study of a larger sample of FR\,I sources with jets \citep{LPdRF}, but the velocity range at the brightening point derived from the distribution of jet/counter-jet ratios assuming an isotropic sample has a maximum $\beta_{\rm max} \approx 0.9$ and a minimum in the range $0.3 \ga \beta_{\rm min} \ga 0$. Such a range would be expected if low edge velocities (as in 3C\,296) are indeed typical of bridged twin-jet sources, since these form the majority of the sample \citep{PDF96}."

?

Note that I haven't said why I think the velocity profiles should be different, as the observed difference seems counter-intuitive to me, at least on some days of the week.

p. 28. Could you add `MJH thanks the Royal Society for a research fellowship.' to the acknowledgements somewhere?

Done

That's all -- sorry not to be more constructive!

I'm not unhappy with a small number of changes ☺

The following from Alan:

Just generally, I felt on my last reading that the combination of summary, comparison with other sources and abstract might be a bit too repetitive. But I don't know whether it's worth trying to slim it down a little before getting referee's comments.

I think that is probably right. A few of the last round of changes go in that direction, but I can't see an easy way to do much more. Maybe leave for now?

In the references, should change the order of the two Miller at al. refs to make them alphabetical.

Done

Fig. 26(b) seems to be a few tics short of a full set on the lower RA axis, at the East end.

I modified the label to avoid overlapping the LH panel but seem to have thrown out the tick-marks too. I've uncommented them.

Cheers

Robert

Subject: Re: Negative Fig. 25
From: rlaing@eso.org
Date: Wed, 3 May 2006 16:41:32 +0200 (CEST)
To: Alan Bridle <abridle@nrao.edu>

On Wed, 3 May 2006, Alan Bridle wrote:

> Dear Robert,
>
> Attached is a fully annotated version of the
> alternate Figure 25. It comes out well on
> two moderate-quality printers here, so I think
> it is fairly safe to go with this version.
>
> A couple of teeny typos:
>
> On p.25, col.2, line 3, "difference" not "differences"
>
> On p.23, col.2, line 3 above end of Sec.6.5,
> "The" should be "the".

All included, thanks (I modified the wedge label very slightly to include 0 and 1, as is my habit). I don't like the sentence that includes the first typo (too many "differences") but my brain is too spongy today to think of anything better.

James and Martin say that they will provide comments early next week.

Then on to the next project. If I can get an injection of energy from somewhere. I guess what's at the top of the FRI queue is:

NGC315 high-resolution + X-ray

followed, in some order, by

0326+39, 3C296 and NGC315 conservation law analysis
Compare and contrast paper trying to summarise the physics
3C31 multifrequency
M84

It might also be worth while trawling the archive for useful FRI data. I would be very tempted to have another look at PKS1333-33 and 3C449 in search of arcs, spectral gradients and some way of applying models without actually fitting them (and have indeed downloaded the available long observations for the former).

As I'm in Holland on 16th, I'm taking the opportunity to go to some of a meeting on long-baseline work with LOFAR. It would have been nice to show something of the 74-MHz work on 3C31 and talk about the possibility of detecting a low-energy cut-off, but I'm still stuck on the B-array data. As you may recall, I tried working on this in Bologna, with very little success. There were 2 reasons for this:

- The machine I was using seemed very slow, although nominally reasonable (2.something GHz, dual processor, several Gb RAM, unloaded). It occurred to me subsequently that it might have been suffering from the Fedora core + internet sockets problem, so I will try to benchmark things on my laptop. Maybe I'll try to get my hands on a faster machine.

- There's something not right about the B-array data. I ought to try Bill's software and see whether this does any better than standard AIPS. A array is fine.

Cheers

Robert

Subject: Re: Spectral index and magnetic field
From: Robert Laing <rlaing@eso.org>
Date: Sun, 23 Apr 2006 12:31:21 +0200 (CEST)
To: Alan Bridle <abridle@nrao.edu>

On Wed, 19 Apr 2006, Alan Bridle wrote:

> could always be replaced by a Sobel + vector image, as the total
> intensity doesn't show up well.

Yes that might be the best way to go. Could you put the Fig.9 vectors
at this resolution in your ftp area? Then I'll kick that around too.
I was only exploring the with the low-resolution version because we
have mentioned some spectral patterns and some magnetic patterns
so I thought it would be worth looking at whether they are related at
all, even if some of it is just another way of seeing the lobe superposition
issues.

Dear Alan

Will do - details in a later message. I'm a little wary of overusing Sobel-filtered images as they sometimes elide the distinction between steps in a brightness distribution and narrow features, but the current I + vectors at 1.5 arcsec doesn't show I very well at all.

> I am not suggesting that the spectral-B superposition should replace
>
> anything in the paper, this was just meant as a check for us to make
>
> sure we aren't missing anything.

Fair enough

> I think this makes two important points: polarization changes
> abruptly between jet and lobe and fields run along sharp edges at
> the lobe edges. I would like to keep the inset from the old Fig 8
> as a separate 1-column figure, as the discussion about possible
> lobe contamination of the jet field estimate depends on it.

OK. There are a lot of figures already so there's no
need to be parsimonious.

It's a confusing point, as we are talking about spectral gradients in termns of lobe contamination and polarization as intrinsic in the same area (6-12 arcsec from the jet axis). The point we have to explain carefully is that the lobes contribute very little at 8.5 GHz, but

much more at lower frequency.

> That was part of my problem, it reads like we were
> endorsing a "standard" model of the lobe spectral curvature
> and that's not quite the way I feel about it. I think what
> we should be saying is that the spectral graduient s in the
> extended emission of 3C296 resemble those seen in many
> FRII source lobes where the steepest spectra are seen
> closest to the nucleus, but the 0.75 spectrum ``extensions''
> that we see on the jets in this case leave open the possibility
> that this is a source with broad steep spectrum ``plumes"
> seen in a more head-on projection. It's hard to rule that out
> after seeing how complicated 3C31 looked, but of course
> these readers have not been officially exposed to that yet 

I think that extreme projection is unlikely, but the situation is much more complicated than a simple axisymmetric model, obviously. Words in the next draft (coming shortly) to cover this.

> > Re 3C31 reference: I think some mention has to be made in the arc
> > explanation - all other references are unnecessary. The 3C296
> > picture is not wholly convincing on its own (the fourth
> > counter-jet arc from the nucleus as marked in Fig 2b looks bright
> > on-axis to me). I even wonder whether we should show the 3C31
> > equivalent of Fig 2. The two sources side-by-side are VERY
> > convincing. What do you think?
>
> Let's try that, as it would be better than a forward reference
> to the whole 3C31 paper.
>
> > If we do, would be nicest next to each other, but putting pictures
> > of another source in Sec 3.1 seems a bit odd. I did wonder, BTW,
> > whether the jets in 3C296 might have slowed signficantly by 80-90
> > arcsec (I_j/I_cj must be lower). Perhaps the last marked arcs on
> > each side are more similar because less aberrated?
>
> That's a possibility. I still wonder about showing the
> curves for the two aberration angles at 58deg

Sorry, you've lost me. What curves?

> to help make
> points like that. The pictures are much easier to grasp than
> the formulae, though I agree we need the latter.
>
> > I need to do the sums for 3C31. We might have to argue that the
> > central velocity levels off after we stop modelling and that there
> > is a bigger velocity gradient from centre to edge than we see
> > close in, otherwise we might have trouble wit the outer arcs. The
> > inner ones on both sides are well inside the modelled region, so
> > should work reasonably.

> Let's save that for an arc-modeling section or paper somewhere else.
>
> I think if we stay convinced that the arc asymmetry is another relativity effect (and we have to, else we are in philosophical trouble) then it may be possible to extract some extra constraints from this, though we will always be less certain that arc have the same intrinsic shapes on both sides than we are that emissivities
> follow the same decline, for example, becaise arcs are more localised phenomena by definition.

I meant just to convince ourselves, not for the paper!

> I think it may be worth showing the 3C31 arcs in this paper to bolster the case for the asymmetry being another aberration effect in principle, but the details of fitting the arcs into the flow models for both sources shouldn't go here. I think it's at the right level of detail for this paper now and elaborating it too much could be a distraction here.

Agreed. As well as taxinf my brain too much.

Will send a revised draft today. Probably too busy with ALMA and VO meetings next week to do much, but would like to converge and get this of to the other authors very shortly.

Cheers

Robert

Subject: More stuff
From: Robert Laing <rlaing@eso.org>
Date: Tue, 18 Apr 2006 13:28:45 +0200 (CEST)
To: Alan Bridle <abridle@nrao.edu>

Dear Alan

Just a quick note as I'm tied up with an ALMA telecon this afternoon and travelling to Italy tomorrow. I've got your recent messages, but have not had time to read through the new text yet.

I also wrote some stuff about the relation between field and Sobel, so will try to amalgamate with yours.

Sobel/spectral index figure

I think the panels make more sense in the order spectral index low/high-res; Sobel ditto, as we refer to the spectral index first. I have changed the text to reflect this. I couldn't figure out exactly what combination of functyp, rgbgamma etc you had used, so could you replot the figures, using the areas:

5.5 arcsec: BLC 360 360; TRC 635 625
1.5 arcsec: BLC 760 761; TRC 1308 1309

The 1.5-arcsec spectral index plot I made was blanked for rms > 0.1 as in the caption. The image you used certainly looks as if it was blanked for rms >0.2 and I have changed the caption to reflect this.

Could you send the individual panels, so that I can play with the positioning, labels etc.? Please use ltype 3 so that the maximum information about plot file version, date etc. is retained: I prefer to comment this out in the postscript in case of later lapses of memory.

Re spectral gradients, backflow etc.: I'm not sure about this. Certainly the FRII model sentence can go, but we probably need to address the issue a bit more thoroughly. I'm struck by the narrow steep-spectrum sheath around the jets, even quite close in. Is this moving in, out or nowhere? Is it a lobe pinched off by buoyancy (cf. Scheuer 1974), part of the jet, or what? Incidentally, I've forgotten why we didn't use a Sobel-filtered L-band image at 1.5 arcsec. Maybe it's too noisy? But it ought to show the jet edge more clearly.

More tomorrow. I should have time to work on this at home before I leave for the airport.

Cheers

Robert

Subject: Stodge
From: rlaing@eso.org
Date: Tue, 28 Mar 2006 17:56:46 +0200 (CEST)
To: Alan Bridle <abridle@nrao.edu>

Dear Alan

Here is a complete draft of the 3C296 paper. I though I had better put James as second author, since this work started life in his thesis, but it has been completely redone from scratch using the new data. I found myself getting rather bored (having written similar stuff too many times already) and I may have missed some important points. The draft certainly needs a fresh eye and a rigorous read-through, which I know you will provide.

Various questions you might consider:

- Arcs: very brief description. Different shapes in main and counter-jets require explanation base on aberration for consistency. I have some vague thoughts but no calculations. Do we need more?

- Are you happy with the level of detail in the spectrum/RM/apparent field section?

— yes.

- The model stuff is quite terse by comparison with other papers: is it at about the right level? The total-intensity comparison (5.1) could do with some more points: can you think of anything?

- Have I over-stressed the problems of fitting 3C296 (I_j/I_cj < 1 at edges; problems with the field at the edge of the main jet)? no.

- Terminology - see previous correspondence. I've used "brightening point" and "flaring region" for now; global edit can be made later. As you say, this needs a careful redefinition early on. I think we agree on the concepts; what remains is to find memorable names.

- Abstract, summary and source comparison section: written last, well after I had lost the will to live. Suggestions for more elegant and accurate prose gratefully received.

Source comparison — other followups planned? If so, could all be [illegible] results here.

- Diagrams: reasonably legible now, but maybe not entirely consistent in style. Fig 17 an anomaly, but it's unreadable in 1-col format. Maybe less blatant labelling would help. — yes, smaller font labels.

- Table A1: notation changed for clarity. Has been a breeding ground for typos in the past. Sanity check needed.

- Chi-squared: don't really understand why the reduced chi-squared < 1.

- Spectrum: looks a bit like what one would expect in the standard FRII model if material flows through jets and forms a sort of backflow. I've put some comments in about this but I'm not sure whether I believe them.

Can this explain inner jet having B⊥ at edge?

Happy reading

Robert

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