Subject: New (final?) version
From: rlaing@eso.org
Date: Mon, 16 Jan 2006 11:22:59 +0100 (CET)
To: Alan Bridle <abridle@nrao.edu>
CC: Bill Cotton <bcotton@nrao.edu>, James Canvin <jcanvin@physics.usyd.edu.au>

Dear All

Colour figs:

Following the referee's report and Alan and Bill's comments, I have modified Figs 5 (also 8 and 9, which you won't have seen previously) to use a different RGBGAMMA. This makes the red somewhat less glaring. There is a substantive change to Fig 5(a), where I have blanked points with rms error on alpha > 0.05 (as in panel b). On reflection, I think Alan was right, and it is unreasonable to expect a gradient of 0.1 in spectral index to show up cleanly if the sigma is as large. This caused me to add a few words to the second para of 4.2 to motivate the use of averages. I think the new version is clear as well as honest.

RM fluctuations

I have tried to incorporate suggestions: see reply to referee for details. In particular, I have backed off on assertions based on 13c and emphasised those from 13b. As Alan suggested, I have moved the reference to Carilli & Taylor to the summary and added a few words to emphasise the reason why the jet polarizations are different to the penultimate para of the summary.

Revised versions of the paper and draft reply to referee's comments are attached. Any final thoughts before I send them both off?

Regards

Robert

--
Robert Laing
European ALMA Instrument Scientist

European Southern Observatory
Karl-Schwarzschild-Strasse 2
D-85748 Garching-bei-Muenchen
Germany

Telephone (direct) (+49) 89 3200 6625
(secretary) 6631/6234/6678
Fax 6611

rlaing@eso.org

Editor's comments:

"This is a very positive review and we should be able to accept your paper after minor revision. I encourage you to explore the use of colour to address the referee's point (1). If colour is necessary to show important features, then I can waive our normal charge for colour printing."

Use of colour will definitely improve the paper. Thank you for supporting the referee's suggestion.

Referee's report:

This is a very well written and important paper on the detailed spectral structure of the jets in the radio galaxy NGC 315. Information at this level of detail is not available on other radio galaxies, and opens the door to serious modeling of relativistic particle acceleration. The observations and reduction of the data are themselves a tour de force, and the authors introduce some new analysis techniques that provide a much more meaningful way to separate physical from observed properties. The Faraday rotation work is also useful, although not at the same level as the spectral results.

We thank the referee for his or her kind remarks and for helpful comments, which we address below.

I have a number of minor comments that the authors may wish to consider prior to publication.

1. It is in general quite difficult to see what the authors intend in the grayscale representations of spectral index and Faraday rotation. This is a problem throughout the literature, but the detailed, high quality information presented here makes it even more important to explore either color or other representations.

We agree, and originally used grey-scales purely to avoid charges. We have replaced Figs 5, 8, 9 and 11 by colour versions, which we think are significantly better. The colour range is slightly different from that of the grey-scales and we have plotted a smaller area in Fig 5(a) to avoid wasting space. We have made the blanking levels consistent between panels (a) and (b) of Fig. 5 [the colour look-up tables are also identical.] The use of a more stringent blank in Fig 5(a) also caused to add a few words on the need for averaging of the spectral index images to the second paragraph of Section 4.2.

8? why?

2. I do not see the banding referred to by the authors in Figure 2. There are lots of different kinds of intensity variations going on. I strongly recommend either a 1-d plot, or indicator arrows on Figure 2 to highlight the features of interest.

We have labelled the most prominent bands on Fig. 2. Also, to make it clear that we do not mean that the bands are periodic, we say "repeated, but irregular alternation" where we first mention them.

✓

3. The authors cite a drop in fractional polarization at the position of the "background source" (Fig. 4a) "consistent with dilution of the jet emission by an unpolarized point source". It would be more direct to simply show or certify that the polarized flux / angle is continuous across the background source.

Indeed. this is a much more direct argument. We have verified that the polarized flux and angle vary smoothly across the source, and say so in the text.

✓

4. In the last paragraph of section 3, the region being discussed as "inner jets" needs to be defined, since it now is being used to talk about the intrinsically symmetric reason. This region needs to explicitly exclude on "on axis enhancement" of Figure 4a).

✓

We agree, and now say "inner ±70 arcsec" for clarity.

5. In Section 5.1, the authors state that "the corrected rms RM is lower in the main jet than the counter jet". I do not believe this to be justified. The calculation of the corrected rms values is admittedly a first order one, subtracting two (relatively) large numbers in quadrature to get a small one. There is no justification offered as to why the residual differences between main and counter jet are significant after this first order subtraction.

This is a fair point. We are not certain enough of the errors on our errors to assess the significance of jet/counter-jet differences quantitatively for the box rms's. We have weakened our claim and in Section 5.1 now say "The corrected profile is very uncertain, but suggests that $\sigma_{\rm RM}$ has a maximum of 2\,rad\,m^{-2} close to the nucleus on the counter-jet side and may be slightly asymmetric in the sense that the rms RM is lower in the main jet than the counter-jet at the same distance from the nucleus."

6. In Section 5.3, the above claim is repeated, along with the note that the fluctuations box to box are larger on the counterjet side. Yet the residuals from the fit do not appear to be random fluctuations, but a very ordered pattern, (Figure 13b). A claim for increased disorder would need to be made more carefully.

The pattern is indeed ordered, and we did not mean to imply otherwise. In Section 5.1 we now say: "The fluctuations are significant, and form an ordered pattern with a typical scale $\sim$100\,arcsec." Here, we do show error bars, and contend that the amplitude of the fluctuations is demonstrably larger on the counter-jet side. RM fluctuations on scales significantly larger than the box size are, of course, expected if the power spectrum of field fluctuations has an approximately power-law form, as the recent references we cite suggest.

We have also clarified the same point in Section 5.3 and now say "The larger-scale ($\sim$100\,arcsec) fluctuations are systematically lower on the main (approaching) jet side. The distribution of fluctuations on smaller scales is also consistent with such an asymmetry, but is not well determined. As with the transverse gradients discussed earlier, the observed position-angle rotations are too small to be sure that the RM fluctuations are due to foreground plasma, but the asymmetry between approaching and receding jets suggests an origin in a distributed magnetoionic medium surrounding the host galaxy, a possibility we now explore."

We have moved the point about power-law spectra of field fluctuations later in the section, where it is more directly relevant.

7. The end of Section 5 state that the data suggest that Faraday rotation "is also present...". This statement is not justified because of the reasons above and other differences between jet/counterjet (e.g., <p>). Elsewhere in the paper, the authors use the word "consistency" with a hot diffuse group Faraday medium to describe this same result; the consistency language is more appropriate.

We have removed the last paragraph of Section 5.3 and now briefly make the analogy with cluster RM models in the summary, again using the language of consistency, as suggested: "Our analysis is therefore consistent with models of Faraday rotation proposed for rich clusters (e.g.\ \citealt{CT}), but requires much lower densities and field strengths."

We do not entirely accept the referee's reasoning here, however: the observed differences in <p> between the two jets are, we assert, primarily an effect of aberration on radiation from intrinsically identical magnetic structures. We have slightly modified the penultimate paragraph of the summary to emphasise this point: "The difference in polarization structure between the main and counter-jets observed in the flaring region by \cite{CLBC} persists at larger distances. This can be explained fully as an effect of differential aberration on radiation from intrinsically identical jets, as long as their velocities remain significantly relativistic on the relevant scales. The asymmetry in RM fluctuation amplitude is consistent with the jet orientation required by this analysis and the presence of a tenuous, magnetized group halo."

We have made a few other very small changes in style. In addition, Dolag et al. (2005) becomes (2006). We are aware that the complete author list for this reference is missing, and will track it down as soon as we can.

Subject: Re: New version
From: rlaing@eso.org
Date: Thu, 17 Nov 2005 16:19:21 +0100 (CET)
To: Alan Bridle <abridle@nrao.edu>
CC: Bill Cotton <bcotton@nrao.edu>, James Canvin <jcanvin@physics.usyd.edu.au>

On Tue, 15 Nov 2005, Alan Bridle wrote:

> Dear Robert,
>
> I've only got a couple of minor comments on this at the moment, but there's one thing I'd like to check on the final 5" resolution image that was used for Figure 2, which I'm not sure I have a copy of ... but in any case cannot find right at the moment. Could you put it somewhere that I might grab it?

Dear James, Alan and Bill

I have attached a new version of the paper. This has two changes:

- A very small correction to a figure reference pointed out by James which I failed to correct last time.

- A rewrite of 5.3 (no changes in the conclusions). I did this for two reasons. First, I managed to decode the Galactic RM models given by the recent Dineen & Coles paper (reference in text). This gave a better guess at the Galactic RM contribution in the vicinity of NGC315. Our text as it stood was a bit misleading, as it implied that NGC315 is closer to the centre of Region A than is actually the case. Second, I wanted to clarify our argument for a Galactic origin for the mean RM and gradient by splitting it clearly into three points: mean value is consistent with Galactic model; gradient is consistent with Galactic structure function; gradient magnitude and direction don't know about the jets. Lots of ifs and buts, but I think it is more convincing now. I struggled a bit with the wording here: maybe someone could come up with more elegant phrasing.

Looks ok

I think we have to address the issue of transverse RM gradients and toroidal fields explicitly, so I have added the para on this which you saw last

time. It seems to me that internal Faraday rotation is extremely improbably if
the densities we are getting from the conservation law analysis are even
vaguely correct. I suppose one could argue for a confining field outside the
emitting material, in which case the density might be reasonable although one
then has no other contraints on the field configuration or strength. All very
unsatisfactory. Does anyone think this should be mentioned in the summary?

In response to the request from Alan, I have put the 5.5 arcsec image from which
Fig 2 was generated at
ftp://ftp.eso.org/pub/general/rlaing/I5.5.FITS.gz.

Cheers

Robert

Subject: New version
From: rlaing@eso.org
Date: Mon, November 14, 2005 5:25 am
To: "James Canvin" <jcanvin@physics.usyd.edu.au> (more)
Priority: Normal
Options: View Full Header | View Printable Version | Bounce

Dear James, Alan and Bill

Here's a revised version, incorporating comments.

Cheers

Robert

Details (my comments indented)

Alan

I agree with Robert that the final discussion of comparisons with 3C31 should go into the paper where we present the conservation-law analysis. The scales on which we see flaring, recollimation and deflection in the two sources will be governed not just by the absolute density but also by how the pressure gradients change in the external media relative to those in the jets, which have different equations of state than the media. So they cannot stay in pressure balance at all distances without ongoing readjustments. I think that's all best discussed in the paper where all of the physical parameters get laid out.

   I've put in a short advert.

I also agree with Bill that this paper is in great shape and almost ready'to go. My comments and suggestions are all small ones: I am also happy with Robert's suggested revision to the tomography discussion. It's good to clarify the circumstances in which Larry's approach works as he has claimed (want to bet he's our referee because of this section?)

  I think I managed to get him to agree verbally ....

I hope Robert survives the all-day Einstein conference followed by the all-night video meeting to Japan ...

  No video, thank goodness.

In the abstract,

line 2: suggest word "five" for 5

 done

line 9 suggest "We derive the distribution of the Faraday rotation over the inner 400 arcsec of the radio source"

  yes, but $\pm$400 arcsec ✓

last line suggest "over the first 400 arcsec" rather than "at large distances"

 done, but also $\pm$200 rather than 400, since it's distance from the nucleus rather than total extent and for consistency with above ✓

Section 1

para 2, line 4, don't need comma between "constant" and H_0 ?

 indeed

para 3. first sentence, suggest "Within ≈ 90 arcsec of the nucleus, the jets in NGC315 are initially narrow, then expand rapidly ("flare") and re-collimate (Bridle 1982; Canvin et al. 2005). Canvin et al .

  Done, but changed following sentence to avoid clumsy repetition of reference. ✓

bullet (v) on field transition to predominantly toroidal, should we give an indication of the scale for this? (approx 80 arcsec if we use the dotted-line divider from Fig 16d of CLBC)

  Outer boundary for B-field is at 26 kpc, although I appear to have sketched the boundary at 30 kpc. I think you have forgotten the sin theta projection factor. Should quote 26 kpc/48 arcsec, I think.

Section 2

last sentence "Measurements of ... Faraday rotation are restricted to the inner 200 arcsec of the field" is not correct, as we show data out to 400 arcsec in Fig 13a.

Oops. Yes, that should apply to spectra only. I've also made the point that bandwidth smearing should not systematically bias the Faraday rotation.

Section 3

first para, line 7 "sharp bend in the main jet", perhaps say "approx 20 arcmin from the nucleus to distinguish further from the "deflection"?

Good idea. This bit has been slightly re-ordered too

end of the same para "Finally, the emission from the inner 4 arcmin of the jets at 5 GHz ..."

OK

p.5, right column, para on "banding".

It reads a little too much like we think long-term power fluctuations at the nucleus are the most likely interpretation of the banding, and I am not sure we want to convey that impression. As well as showing a time-of-flight distance asymmetry, power fluctuations should appear concave inwards in a jet that has slower velocities at the edge. Sobel filtering my copy of the 5-arcsec resolution L band image (which I do not think was the final version) shows some concave-inwards edge features inside the flaring region at the more rapid brightness DEcreases in the jet and counterjet but there's nothing with a strong enough gradient to show up above the Sobel filter noise much further out. I made a 10-arcsec resolution image with CONV and Sobel filtered that, which shows that the strongest brightess graidents further out are those at the edges (sides) of the jet, not in the "bands", but the outer bands also show no sign of concave-inwards structure. They are more like slow brightness fluctuations that run right across the middle of the jet, then get lost in the steeper brightness gradients at the edges. The more obvious "banding" also occurs in regions of recollimation or deflection of the jet.

So I suggest some slight rewording of this para:

"The brightness distributions in both jets show large-scale "banding" - alternation of brighter and fainter regions - along their lengths on arc-minute scales. The brightness bands extend across both jets but their variations are slower than those in the flaring region or at the edges of the jets. These variations could, in principle, result either from periods of enhanced activity in the nucleus or from interactions between the jets and their surroundings. If they were due to fluctuations in activity in the nucleus that propagated outwards at constant velocity βc, then ... . although any transverse velocity gradients will complicate this expression and should distort the bands into arcs that are concave towards the nucleus. We see no obvious relation between the distances of the bands, in the two jets for any plausible value of $limits\beta$ and no evidence for systematic concave curvature of the bands beyond the flaring region. Furthermore, the most prominent banding appears to be associated with regions where he jets deflect or change their collimation properties. It therefore seems more likely that the banding is associated with ongoing interactions between the jets and their surroundings, although we cannot rule out a contribution to large-scale brightness fluctuations from slow variations in the jet output."

Done

next para

"The remarkable 180\deg bend in the main jet at the West end of the source id well known from earlier observations. Our L-band data ...

OK

In the final para of this right column,

"The jets bend slightly as they recollimate" (as this is not the bigger bend that we gave labeled "Deflection" fiurther out).

OK. Advert for conservation-law analysis placed just before this.

and I think we should delete mention of the sidedness ratio map if we will not actually show it, so start the final sentence at

"The main jet is brighter than the counterjet {\it on-axis} at all distances from the nucleus ..."

OK. Also {\em edge} slihtly later?

p.6, left column, halfway down last para of section:

"This raises the question: how large is the region over which symmetrical relativistic-jet models can be applied?

Better

Delete "working" or "current" before "hypothesis" in the last sentence, but add "and may remain responsible for the generally brighter appearance of the NW jet far from the nucleus".

Yes

Section 4

bullet (i) "flux-density scale" for "flux scale" (for consistency with everywhere else)

Yes

Section 4.2,

first para, last sentence, "Data are plotted only where ..."

This bit has gone, since it is repeats something from the figure caption.

buller (i) delete "quite" before "subtle"

Oh, all right then

Figure 11 caption In grey scale ranges, use "-90 to -70" and "-10 to +10" rather than "-90 - -70" and "-10 - +10" to make it easier to read. Also "Data are plotted only where ..."

Yes

Section 5,3

The second para. is a repeat of the tail end of the first and should be deleted.

I think that this was a mis-edit which I have since deleted - please confirm.

Didn't delete. Question is one of the "imprint" of the galaxy – linear variation does not distinguish the nucleus, and suggests a larger scale (e.g. foreground)

"would expect some signature of the nuclear region in the variation with distance from the nucleus, rather the simple linear gradient"

Also, "symmetric" instead of "systematic"? A linear variation is "systemmatic", but we are saying that variation is Galactic.

Just delete "systematic" and reword slightly

last sentence of section

"the tenuous halo of a poor group".

(i.e., of this one, else we get into the old story of the astronomer, physicist and mathematician drawing different conclusions from seeing a black sheep in a field ...)

Better justified than some of the talks at this conference ....

Section 7, para 3

"must originate mostly" rather than "must mostly originate"

or

"most of the rotation must originate"

OK

James

Looks about done to me, just a few very minor points.

References to sub-figures have a mix of using and not using brackets around the letter, e.g Fig. 1a and Fig, 1(a), any chance of sticking to just one?

This affected figure references inside brackets. I have removed the square brackets to be consistent with what I now believe to be MN style.

Very pedantic point but: At the Start of Section 3 we say Fig. 2 shows the same area as Fig. 1b. That's not quite true, Fig 2 is offset by about 2 arcmin north from 1b.

That wasn't intentional - a change had been left half-done. I have made the areas the same now.

Section 3, bottom of first column on Page 5: "At a similar distance from the nucleus, the counter-jet has a surface-brightness minimum (Fig. 1b)," I'm not sure I agree with that, it looks to me (admittedly just reading off the figures in the paper) that at the equivalent distance from the nucleus to the "deflection" in the counter-jet, the surface-brightness is falling rapidly, not at a minimum.

You are quite right. Fixed.

Three paragraphs on, starting "The source at RA...": "(Fig. 15, below)" should be (Fig. 15a).

I seem to have fixed this already. Must have rewritten it.

Later in the same paragraph: ("On-axis enhancement" in Fig.4a) [not 4c]

Fixed

Figure 5, line 5: one last instance of "flux-scale" to "flux-density scale" to change.

Yes

Figure 6, last line: averaged as in panel (a) [not (b)]

Yes

Figure 8: As per Alan's comment for Fig. 11, change "-1 - +1" to "-1 to +1".

Done

More changes:

- Fig 16

  Changed x axis range to match equivalent profiles for spectra.

  Fixed glitches in counter-jet profile in panel (b) - these were caused by AIPS changing units from milli to micro-ratio when I wasn't looking.

- Checked and where necessary corrected jet PA's. Some of the values were the rotation to bring the jet to PA 90 rather than from PA 0.

- Addition of more material on the transverse RM variation, including profiles (Figs 11 and 14; Section 5.3) to address issue of internal rotation due to toroidal field. Please read through this and tell me what you think. OK

--
Robert Laing
European ALMA Instrument Scientist

European Southern Observatory
Karl-Schwarzschild-Strasse 2
D-85748 Garching-bei-Muenchen
Germany

Telephone (direct) (+49) 89 3200 6625
(secretary) 6631/6234/6678
Fax 6611

rlaing@eso.org

Download this as a file

Attachments:

ngc315ls.ps.gz	**2.3 M**	[application/octet-stream]	**Revised paper**	Download

Take Address

Delete & Prev | Delete & Next

Move to: INBOX Move

Subject: Mass entrainment (fwd)
From: rlaing@eso.org
Date: Mon, November 14, 2005 7:42 am
To: "Alan Bridle" <abridle@nrao.edu>
Priority: Normal
Options: View Full Header | View Printable Version | Bounce

Dear Alan

Am I alone in finding this less than helpful as an answer to a request for quantitative estimates of entrainment rates as functions of something we might even be able to measure?

Regards

Robert
---------- Forwarded message ----------
Date: Mon, 7 Nov 2005 12:09:27 -0700 (MST)
From: Dave De Young <deyoung@noao.edu>
To: rlaing@eso.org
Subject: Mass entrainment

Dear Robert;

Thanks for your note enquiring about entrainment; I am sorry to have been delayed in replying. As I am sure you know, a proper treatment of this problem involves solution to the full non-linear set of equations, for which no analytic solutions exist. Hence analytic approaches to this difficult problem all rest upon varying degrees of approximation and in some cases wishful thinking.

The treatment in my 1996 paper for the Energy Transport conference was fun because in involved a rather different approach since it looked at individual vortices. The treatment there is, I think not bad if the flow can be approximated as incompressible (in which case the treatment is actually rigorous), and if one is concerned about the early stages in the development of the mixing layer (strictly speaking it is not a boundary layer at all) where the vortices do not touch or at least where vortex interaction and merging is not a dominant process. The numbers at the end were gotten from the numerical integration of the dM/dt and dR/dt equations. One could find a solution in scale free form with appropriate constants out in front and then just insert the values for the particular case being considered.

Another approach which may be more valid in the fully developed non-linear turbulence regime is one that is semi-empirical. This is described in my book (pp 158 - 162), and also in ApJ 405, L13. I think that approach is fairly straight forward, and it can be applied to mildly compressible (i.e., mildly supersonic) flows. But the analytic forms resulting from the semiempirical approach do not directly give an entrainment rate. They provide an estimate of the "saturation length" along the jet after which the jet is fully turbulent and entrained material can penetrate to the centerline of the jet. An assumption here is that the growth of the boundary layer thickness is linear with distance, a result that seems verified empirically well into the non-linear turbulent regime. An entrainment rate can come from this result if one makes the "not unreasonable"(!) assumption that once the jet becomes fully turbulent throughout its volume, half of that volume is occupied by entrained ambient material. An entrainment rate then follows directly from the jet parameters. This assumption also presumes saturation of entrainment after the fully turbulent regime is achieved, which may be approximately true at best.

I hope this at least partially addresses the question you raised. Due to the various assumptions and regimes of applicability involved in all the analytic approximations, I cannot provide you with an equation of the form you requested that will be valid for all jets in all environments. I would be happy to continue along this topic if you have any additional comments or questions.

Best wishes,

Dave

Download this as a file

I agree with Robert that the final discussion of comparisons with 3C31 should go into the paper where we present the conservation-law analysis. Basically the scales on which we see phenomena such as flaring, recollimation and deflection in the two sources will be governed by how the pressure gradients change in the external media versus the pressure drops in the relativistic jets, which have different equations of state than those in the external media, so they cannot stay in the pressure balance at all distances without some ongoing readjustments. I think that's best discussed in the paper where he have all the physical parameters laid out.

I also agree with Bill that this paper is now in good shape and almost ready'to
go, my comments and suggestions are all small ones: I am happy with Robert's suggested revision to the tomography discussion. It's good to clarify the circumstances in which Larry's approach works as he has claimed (want to bet he's our referee because of this section?)

In the abstract,

line 2: suggest word "five" for 5

line 9 suggest "We derive the distribution of the Faraday rotation over the inner 400 arcsec of the radio source"

last line suggest "over the first 400 arcsec" rather than "at large distances"

Section 1

para 2, line 4, don't need comma between "constant" and H_0 ?

para 3. first sentence, suggest "Within ≈ 90 arcsec of the nucleus, the
jets in NGC315 are initially narrow, then expand rapidly ("flare") and
re-collimate (Bridle 1982; Canvin et al. 2005). Canvin et al .

bullet (v) on field transition to predominantly toroidal, should we give an indication of the scale for this? (approx 80 arcsec if we use the dotted-line divider from Fig 16d of CLBC)

Section 2

last sentence "Measurements of ... Faraday rotation are restricted to the inner
200 arcsec of the field" is not correct, as we show data out to 400 arcsec in Fig 13a.

Section 3

first para, line 7 "sharp bend in the main jet", perhaps say "approx 20 arcmin
from the nucleus to distinguish further from the "deflection"?

end of the same para "Finally, the emission from the inner 4 arcmin of the jets at 5 GHz ..."

p.5, right column, para on "banding".

It reads a little too much like we think long-term power fluctuations at the nucleus are the most likely interpretation of the banding, and I am not sure we want to convey that impression. As well as showing a time-of-flight distance asymmetry, power fluctuations should appear concave inwards in a jet that has slower velocities at the edge. Sobel filtering my copy of the 5-arcsec resolution L band image (which I do not think was the final version) shows some concave-inwards edge features inside the flaring region at

the more rapid brightness DEcreases in the jet and counterjet but there's nothing with a strong enough gradient to show up above the Sobel filter noise much further out. I made a 10-arcsec resolution image with CONV and Sobel filtered that, which shows that the strongest brightess graidents further out are those at the edges (sides) of the jet, not in the "bands", but the outer bands also show no sign of concave-inwards structure. They are more like slow brightness fluctuations that run right across the middle of the jet, then
get lost in the steeper brightness gradients at the edges. The more obvious "banding" also occurs in regions of recollimation or deflection of the jet.

So I suggest some slight rewording of this para:

"The brightness distributions in both jets show large-scale "banding" - alternation of brighter and fainter regions - along their lengths on arc-minute scales. The brightness bands extend across both jets but their variations are slower than those in the flaring region or at the edges of the jets. These variations could, in principle, result either from periods of enhanced activity in the nucleus or from interactions between the jets and their surroundings. If they were due to fluctuations in activity in the nucleus that propagated outwards at constant velocity βc, then ...
.... although any transverse velocity gradients will complicate this expression and should distort the bands into arcs that are concave towards the
nucleus. We see no obvious relation between the distances of the bands, in the two jets for any plausible value of $limits\beta$ and no evidence for systematic concave curvature of the bands beyond the flaring region. Furthermore, the most prominent banding appears to be associated with regions where he jets deflect or change their collimation properties. It therefore seems more likely that the banding is associated with ongoing interactions between the jets and their surroundings, although we cannot rule out a contribution to large-scale brightness fluctuations from slow variations in the jet output."

next para

"The remarkable 180\deg bend in the main jet at the West end of the source id well known from earlier observations. Our L-band data ...

In the final para of this right column,

"The jets bend slightly as they recollimate" (as this is not the bigger bend that we gave labeled "Deflection" fiurther out).

and I think we should delete mention of the sidedness ratio map if we will not
actually show it, so start the final sentence at

"The main jet is brighter than the counterjet {\it on-axis} at all distances from the nucleus ..."

p.6, left column, halfway down last para of section:

"This raises the question: how large is the region over which symmetrical relativistic-jet models can be applied?

Delete "working" or "current" before "hypothesis" in the last sentence, but add "and may remain responsible for the generally brighter appearance of the NW jet far from the nucleus".

Section 4

bullet (i) "flux-density scale" for "flux scale" (for consistency with everywhere
else)

Section 4.2,

first para, last sentence, "Data are plotted only where ..."

buller (i) delete "quite" before "subtle"

Figure 11 caption In grey scale ranges, use "-90 to -70" and "-10 to +10" rather than "-90 - -70" and "-10 - +10" to make it easier to read. Also "Data are plotted only where ..."

Section 5,3

The second para. is a repeat of the tail end of the first and should be deleted.

Also, "symmetric" instead of "systematic"? A linear variation is "systemmatic",
but we are saying that variation is Galactic.

last sentence of section

"the tenuous halo of a poor group".

(i.e., of this one, else we get into the old story of the astronomer, physicist
and mathematician drawing different conclusions from seeing a black sheep in a
field ...)

Section 7, para 3

"must originate mostly" rather than "must mostly originate"

or

"most of the rotation must originate"

.

.

,

Subject: Re: New version
From: Bill Cotton <bcotton@nrao.edu>
Date: Wed, 9 Nov 2005 06:58:58 -0500
To: Robert Laing <rlaing@eso.org>
CC: Bill Cotton <bcotton@nrao.edu>, Alan Bridle <abridle@nrao.edu>,
jcanvin@physics.usyd.edu.au

Robert Laing writes:
 > Quoting Bill Cotton <bcotton@cv.nrao.edu>:
 > - Sect 4.3. The opening of this section is partially redundant
with
 > Section 4.2, 3rd paragraph. A backwards reference to the
tomographic
 > technique may be sufficient.
 >
 >   I don't think that this is redundant, but it may not be clear.
The point is
 >   that I agree with the use of the tomographic technique to
demonstrate the
 >   spectral index of the edge component (which is not superposed on
anything) but
 >   NOT with the claim that you can establish the spectral index of
the on-axis
 >   component in the same way. The fundamental reason is that we
probably aren't
 >   dealing with the superposition of two unrelated components along
the line of
 >   sight, but rather with an on-axis component sitting inside the
off-axis one.
 >   I probably didn't make this clear enough.
 >
 >   Suggested text to make this clearer (beginning of 4.3):
 >
 >   "\citet{KSetal} and \citet{K-SR} suggested that the spectral
index of an {\em
 >   on-axis} component in a jet is the value of $\alpha_{\rm t}$ at
which the
 >   component appears to vanish against the background of the
surrounding emission
 >   (exactly as for an {\em edge} component such as that in the
NGC\,315 jets;
 >   Section~\ref{tomography}) and can therefore be derived simply
from a
 >   tomographic analysis. This requires an additional assumption ....
"
 >
 >   Does this help? Any suggestions for clarification?
 >

   This is fine. I guess my comment wasn't clear enough. What
appeared redundant to be was that the original wording appeared to be

introducing the topic of tomography rather than continuing the discussion of it. Your change takes care of this.

-Bill

Subject: Re: New version
From: Bill Cotton <bcotton@nrao.edu>
Date: Tue, 8 Nov 2005 09:17:19 -0500
To: Robert Laing <rlaing@eso.org>
CC: Alan Bridle <abridle@nrao.edu>, James@eso.org, Canvin@eso.org, jcanvin@physics.usyd.edu.au, Bill Cotton <bcotton@nrao.edu>

Robert,

It's looking pretty close. The only substantive comment I have is that there is an analagy with 3C31 that could be commented on. The size scale in NGC315 for all the various jet features is substantially larger than in 3C31 for which there is similar linear resolution. However, the RMS RM fluctuations are 10x lower in NGC315. If the RMS is proportional to the mean plasma density (or even close) then the IGM around NGC315 is much more tenuous than 3C31. If the various flaring, recollimation etc, are largely determined by the external medium, then the apparent difference in external density between 3C31 and NGC315 could explain the difference in size scale of the jet features.

Minor comments:

- Introduction. Most of the discussion of features is in terms of angular distance from the core which helps identify them on figures. However, the the list in the introduction (ii) the distances are given in kpc. It might be worth giving angular distances parenthetically.

- Sect 4.3. The opening of this section is partially redundant with Section 4.2, 3rd paragraph. A backwards reference to the tomographic technique may be sufficient.

-Bill

Subject: Re: Comments on NGC315 large-scale paper
From: Robert Laing <rlaing@eso.org>
Date: Mon, 31 Oct 2005 19:39:58 +0100 (CET)
To: Alan Bridle <abridle@nrao.edu>
CC: Bill Cotton <bcotton@nrao.edu>, James Canvin <jcanvin@physics.usyd.edu.au>

Dear Alan

Thanks for the comments. I'll come back to the "banding" and confinement issues in a bit, as they need a little more thought. You mentioned large-scale sidedness. I've attached a FITS version of a sidedness image at 2.35 arcsec resolution. Do you think we should show this or comment on it in more detail?

I've put in your small changes essentially without modification (see below).

Thanks again.

Robert

Some small text suggestions:

In the abstract, line 10, say we derive the "variation" (or "distribution" of the Faraday rotation over the radio source.

distribution - DONE

In Section 2.2 on data reduction, I don't like "the core flux" much, could we say "flux density of the unresolved component"?

DONE

In Section 4.1 could we say "flux-density scale" rather than "flux scale"

DONE. I'm usually pedantic about that point. Must be slipping.

Throughout Section 4.2, 4.4 mnd 4.5 we use the term "flat-spectrum edge". To me "flat-spectrum" should mean a spectral index near zero. Could we just say

"flatter-spectrum" as we mean an index of 0.44?

Agreed. DONE

Section 4.5 should be titled "Acceleration mechanisms".

DONE Why on Earth did I say "Emission"?

Also to make this connect better with the people who actually study acceleration

mechanisms it might be a good idea to identify the electron energy regimes we're

talking about for the radio and X-ray bands. I suggest that we use field strengths that are consistent with your recently-fitted conservation-law models in order to do this (and recapitulate these energy regimes in Section 7)

Agreed in principle. Needs a little thought.

Figure 12 caption. "representative" not "representive"

DONE

Fig.12 discussion: "The fit to a lambda-squared law is good everywhere: two examples are shown ..." because the examples are of the goodness of fit, not of something else, as implied by "and"

DONE

Page 12, second para, first sentence. Can we drop 'effectively" and use "are" instead of "appear"? And in the last para of Section 5.1, "We can image these smaller scale fluctuations directly only at the bright base of the main jet." and "Data are plotted only where the rms ...."

DONE

In Section 5.3 on the origin of the RM, our point is that the component of the fitted RM gradient transverse to the jet is somewhat ill-determined, so the "discrepancy" with Simonetti and Cordes is not significant. I think that's a little obfuscated by the language. Could we say "The magnitude of the component of the fitted gradient transverse to the jet axis is uncertain, however, so this discrepancy is not significant. Rather, both the linearity of the variation of the RM along the jet axis [Fig. 13(a)] and the fact that the maximum gradient appears not to align with the jet axis or the minor axis of the galaxy suggest that most of the Faraday rotating medium is not associated with the jets or with the host galaxy. In either case, we would expect ..."

DONE

The "residual" at the start of the second paragraph of Sec 5.3 deserves some emphasis, italicize it? Also delete the "a" before 'distributed magnetoionic material" (or substitute "medium" for "material").

DONE

Perhaps insert "plasma" before "component" or "of the medium" after it, in the penultimate sentence of this paragraph?

DONE ("hot plasma" rather than just "plasma")

At the end of Section 5, perhaps say "suggests" rather than "shows" as the evidence in Fig.13 is closer to "suggestive" than to "overwhelming"?

Oh all right then. DONE

In the first para of Section 6, should we remind people of the scale by adding "approximately 1 arcmin from the nucleus" after "the flaring region"?

(up to $\approx$70 arcsec from the nucleus) DONE

In Section 7, para.2 couldwe say "This region is associated with strong X-ray emission in the main jet, high radio emissivity, complex filamentary structure, and fast $\beta \approx 0.9$ flow (refs)"

DONE

In para.3 of that section "We have also detected small residual fluctuations" and "not in the known X-ray-emitting halo, whose core radius is too small."

DONE

In para 4 of Section 7, start "We also derive"

DONE

Acknowledgement, thank Mack for the "327-MHz WSRT image".

DONE

I have not had time to check the references yet. Have you done that yourself recently?

No. [Other than for grotesque misspellings] Would appreciate help with this if you have time.

Subject: Re: Comments on NGC315 large-scale paper
From: Robert Laing <rlaing@eso.org>
Date: Fri, 28 Oct 2005 15:40:28 +0200 (CEST)
To: Bill Cotton <bcotton@nrao.edu>
CC: Robert Laing <rlaing@eso.org>, Alan Bridle <abridle@nrao.edu>, James Canvin <jcanvin@physics.usyd.edu.au>

On Fri, 28 Oct 2005, Bill Cotton wrote:

> Robert,
>
> The paper looks in very good shape, nice job! My comments are mostly very minor. The only substantial comments are inspired by figure 2. There are two striking feature we don't comment on. The first is that the jet (counterjet also?) has a very constant width. Since the external medium is very sparse (from X-ray data) and
> can't be constant over this huge range, the confinement must be largely self generated. Could this be by the largely toroidal component of the magnetic field?

Yes, interesting point. I've thought about this for 1553+24, but forgot about it in this context. It's correct that the jet is most unlikely to be free with a big Mach number, so either it is propagating in some entirely uniform medium which we can't detect, or it is holding itself together. I will do some rough sums, estimate the current etc. I wonder whether a jet can go from being pressure confined to self-confined? Don't see why not - some similarities to the solutions that Alan looked at with Chan & Henriksen some lifetimes ago.

> The other striking feature of figure 2, especially in the counter
> jet is the banding. This is presumably the result of periods of enhanced activity in the core. Since you have a good dynamical model,
> it should be possible to estimate the age and duration of these features.

Yes. I don't suppose one can match them up on the 2 sides - that would be too much to hope for.

> Minor comments:
>
> - page 2, middle col 1. This states that we will see if the fluctuations of the RM are consistent with coming from the host galaxy
> gas. We knew at the beginning that this wasn't possible due to the X-ray measurements. This should really say that the test was to see

> if the RM fluctuations were consistent with coming from the group
> IGM.

Agreed

> - page 5, col 2, bottom. is "before and after zero-level correction
>
> is <0.01" correct or should this be 0.1?

My memory is tha it is correct and that this condition didn't make much difference, but I will check.

> - page 13, 3rd from last line of para 1. The units on 0.22 should
> be
> meters not cm.

Well, ALMA is the day job

> - page 16, 2nd column line 7-8. Shouldn't "two different emission
> mechanisms" be "two different acceleration mechanisms"? We assume
> it's all incoherent synchrotron.

Indeed it should.

Thanks

I'll put in the small corrections while I remember and think about the other two.

Cheers

Robert

--
Robert Laing
European ALMA Instrument Scientist

European Southern Observatory
Karl-Schwarzschild-Strasse 2
D-85748 Garching-bei-Muenchen
Germany

Telephone (direct)   (+49) 89 3200 6625
          (secretary)   6631/6234/6678
Fax   6611

rlaing@eso.org

Subject: Re: Comments on NGC315 large-scale paper
From: Bill Cotton <bcotton@nrao.edu>
Date: Fri, 28 Oct 2005 09:18:51 -0400
To: Robert Laing <rlaing@eso.org>
CC: Alan Bridle <abridle@nrao.edu>, Bill Cotton <bcotton@nrao.edu>, James Canvin <jcanvin@physics.usyd.edu.au>

Robert,

The paper looks in very good shape, nice job! My comments are mostly very minor. The only substantial comments are inspired by figure 2. There are two striking feature we don't comment on. The first is that the jet (counterjet also?) has a very constant width. Since the external medium is very sparse (from X-ray data) and can't be constant over this huge range, the confinement must be largely self generated. Could this be by the largely toroidal component of the magnetic field?

The other striking feature of figure 2, especially in the counter jet is the banding. This is presumably the result of periods of enhanced activity in the core. Since you have a good dynamical model, it should be possible to estimate the age and duration of these features.

Minor comments:

- page 2, middle col 1. This states that we will see if the fluctuations of the RM are consistent with coming from the host galaxy gas. We knew at the beginning that this wasn't possible due to the X-ray measurements. This should really say that the test was to see if the RM fluctuations were consistent with coming from the group IGM.

- page 5, col 2, bottom. is "before and after zero-level correction is <0.01" correct or should this be 0.1?

- page 13, 3rd from last line of para 1. The units on 0.22 should be meters not cm.

- page 16, 2nd column line 7-8. Shouldn't "two different emission mechanisms" be "two different acceleration mechanisms"? We assume it's all incoherent synchrotron.

-Bill

Subject: Another try
From: rlaing@eso.org
Date: Wed, 11 May 2005 16:52:51 +0200 (CEST)
To: Alan Bridle <abridle@nrao.edu>, Bill Cotton <bcotton@nrao.edu>, James Canvin <jcanvin@physics.usyd.edu.au>

Dear NGC315 people

Here is a revised draft incorporating comments so far.

Ready to go?

Cheers Robert

General changes:
----

1. References to conservation law section deleted and appropriate changes to
   intro and summary.

2. Section on helical filaments removed; candidate for inclusion in
   high-resolution paper.

Other miscellaneous tidyings, including references to large-scale and high-resolution/X-ray papers, spell check, minor MNRAS style things, \theta
defined before it is used, correct Fig 4 caption (of each jet -> of the main
jet), define free model.

Alan's comments

NGC 315 modeling paper
======================

This is a very good first draft, but of course it has a
good "pedigree" too

  Best in show at Crufts in the shaggy dog class?

I'm not sure about waiting for the X-ray data and section
6. What's said here stands on its own, much as our "free
modeling" paper did for 3C31. I think if we still do not
have the X-ray stuff by the time we are all happy with this
"as is" then we should go ahead with it and write up the
conservation law stuff in a second paper, as was done for

3C31. It would be better to do it "all in one" this time, but this paper already has a number of good points to make and I don't think we should actually delay it for the X-ray data.

I think we all agree with this

I'll go through my comments in order in the paper, as that may be easier for Robert.

p.2 line 5, add "simultaneously" after I,Q and U for emphasis?

moved the "simultaneously", but point taken

p2., para 1, next to last line "previously" instead of "so far"?

I like "so far" OK

End of para. Cosmology. Yes, but can we also say that nothing in what we're saying depends much on the cosmology, only on the value of H_0? These sources are close and we have bigger sources of error ...

I'm a dedicated follower of fashion. And my software is more accurate than some of the rubbish you find on the web. [It has to be confessed, however. that there was a small error in the computation of physical sizes in earlier versions of the model program. Too small for anyone ever to notice, fortunately]. ADDED A NOTE

Section 2.1

First sentence, both "radio" jets (several of these requirements are of the radio data) but then The nearby "giant elliptical" galaxy NGC315, whose "large-scale" radio structure was first imaged .."

Then I'd like to move up the redshift sentence so we can say

"The radio sources is associated with a galaxy at a red shift of 0.01468 (ref) giving a scale of 0.335 kpc/arcsec for our adopted cosmology. The overall extent of the radio source is nearly one degree, or about 1.2 Mpc in projection, but the area to be modeled (see Fig.1) is limited in extent by the slight bends in the jet at roughly

Fine as done by RAL

70 arcsec (23 kpc) from the nucleus."

Done, except for "the galaxy has a redshift"

Section 2.2, last para, four lines from end:

"fit to Stokes I, Q and U directly, but our plots of
the degree (sp) of polarization ..." (just to
reinforce the point already being made) DONE

Section 3,3

I'm worried that Table 2 is a "big gulp" for
everyone except James, Robert and me, and that
we are not the best judges of its magnitude --
I'll be interested in Bill's comments on that.
However,I do not see how to soften the blow, and
I feel the level of detail reached in the text is
about right.

This worried me too, but I had to compromise between repeating C&L and not
giving enough information. I think the table has to be there to explain the
symbols, and the references to earlier stuff are given.

end of first para:

"summarize their forms for completeness and to
highlight ...." DONE

p.3, last line

but "in this case" we model only the flaring
region ... DONE

page 4, end of first para.

"our ability to fit an axisymmetric model
directly to to the observed data" DONE

(at least to first order, it's a complication in the geometry, not an intrinsic
complication of the modeling, in that we might be able to fit an axisymmetric
model if we could just de-bend the co-ordinate system a little).

One day we will work out how to "de-bend the co-ordinate system"
Not sure whether anyone will believe the results, mind you ..

Section 3.3.4,

second para. I'd prefer to use words "five"
and "four" over 5 and 4 here , but maybe

MNRAS has rules about that.

  Dunno. I've followed your preference for now

I'd also prefer
to say "in the interests of greater clarity"
rather than to impugn the clarity of CL

  DONE: anyone have more memorable names for the boundaries?

Last sentence of third para., too narrow
"for our data" to constrain ... think EVLA-2.

  DONE

Figure 5(a) is in danger of losing the
counterjet altogether in the model panel.
Did you try contours? It's in danger of
sending the wrong message if the lowest grey
scale does not come out clearly.

  This is quite tricky. Contours are very ratty for the data. The point, really,
  is to show that the observed counter-jet is: (a) brighter than predicted and (b)
  knotty in the wrong places. The main jet is better represented by contours, I
  think, as in Fig 4. Fig 5 could just show the counter-jet, I suppose, but I'd
  rather keep it as it is.

It also does not really show the "fine scale structure close to the nucleus"
well as we say in Section 4.1.

  I've modified this to say

Fig.~\ref{fig:ismall} shows grey-scales of the inner 8\,arcsec of both jets with
levels chosen to emphasize the fine-scale structure observed in the counter-jet.

Bottom of page. Have we ever made the point
that the functional forms in Table 2 are capable
of producing things that look nothing like the
observed radio jets ... the point in the last
sentence is really important to the overall
crfedibility of what we are doing ... "The number
of independent points .... is sufficiently large
that we are confident that the main features of
the models are well constrained," Although we
have some parameters with large errors, there's a
strong family resemblance emerging here, especially

when you consider how little like real radio jets
most of the things that Table 2 can produce
actually look.

Could you suggest some words? ⇐ in discussion section?

Fig.6, I like what's been done here, we might
tweak the last sentence of the first para of 4.1
to say "The small offset between the observed and
model profiles visible in both these Figures is
caused by slight bends in the jets ..." DONE

Bullet (iv) "transverse" before profile, for clarity?

Obviously not very clear, as the longitudinal profile was meant!
I've said
this explicitly. 8/c

Bullet (vi): Figure 3c also contributes to showing
this. ✓

Changed to a generic reference to Fig 3.

Bullet (vii) Add "Fig." before 7) DONE ✓

Section 4.2

Also reads well, I'd like to refer to the Figures
here everywhere in the bullets though.

Add "by the model" after "well described" just before
the bullets. DONE ✓

Bullet (ii) refer to Fig. 10. ✓

Bullet (iii) refer to Fig. 10 ✓

Bullet (vi) refer to Fig. 8(c). ✓

ALL DONE

page 10, first sentence use "the sidedness ratio"
instead of "this ratio", for clarity. OK ✓

5.2 first sentence of col.2, can we specify the
opening angle limit on the "well-collimated" inner
region to make this more quantitative?

The form of the curve we now use makes this difficult, as the
opening angle
formally approaches 0 at the nucleus and its maximum allowed value
is not
easily deducible from the numbers in the Table. I guess we could
quote the

fitted opening angle at (say) 1 kpc: is this what you have in mind?

last sentence of 5.2: replace "on even larger scales" with "on scales >500 arcsec in projection"? OK

5.3.2. Acceleration section, Robert asked if this was too much, I don't think so, the only thing I would drop is the clause at the bottom of p.13 about "and this phenomenon may be common in FRI jets", as we have already given the examples. OK

It might be worth inserting "our best-fitting inclination angle of" before the theta = in the right column of text on p. 12. OK

The :impossible to reconcile" statement in the middle of the last para on p.13 might need to be softened, e,g, by "without further velocity stratification across the jet" after "1 pc"?

No: I explicitly wanted to contrast bulk acceleration reaching a speed of 0.7c
with a flow in which we see only a 0.7c component with faster stuff hidden. I
thought I had done this in the first sentence of the para, but the point
obviously didn't get through: any suggestions for rewording? — "anywhere in the jet"

Section 5.5.1 reads well to me, I have no suggestions to add.

Section 5,5.2 (filaments) is a good synopsis of some things we've discussed casually (at least Robert and I have) bbut I'm a bit worried that the pictures we're showing here don't quite motivate it the way the ones we've looked at on the TV do. Figure 4(b), being contours, doesn't hit the reader in the eye with it, Figure 11(b) comes closer. We really need 4(b) as a grey scale to do the job, but I wonder if the "high-res" paper might not end up being the best place for this?

I've removed this subsection pending its inclusion in the high-res paper; also
refs to it elsewhere in the text

Section 5.6 The sentence on p.17, line 6 of left column, "qualitativelt as expected but qualitatively inconsistent" must mean "quantitatively" inconsistent. DONE

Section 7.1 We should emphasize again that this is just the flaring region, perhaps just by inserting that in the first sentence "the flaring region of the jets" in 7.1 and ("23 kpc" in projection) in the first sentence of 7.1.1

in these cases drop the in projection, arcsec are arcsec

You've lost me here. 70 arcsec is in projection. I don't like using linear units for projected distances unless the context is very clear - the idea was to keep kpc for things in the jet frame.

On Robert's questions,

1. Won't the chi-squared and goodness of fit take care of this?

Yes, although we traditionally have a problem close to the core with low signal and narrow jets. Not a serious issue, I think.

2. Only James can answer that one, I think.

OK as is

3. The scale factor is what we know at the moment, I'd prefer to see the X-ray data before going \further

No mods made

4. I think it reads ok without a separate "discussion"

5. Did not strike me as too much, except that the filamentation part might be better done in the next paper.

Cf. above

6. I think we have the correct crew, and I again have no trouble with the order. James and Robert have done a great job on putting this together and apart from Section 6 and perhaps what we do with 5.5.2, I think this first draft is already very close to something we could send off. And that's another reason for not waiting too long for the input to Section 6.

I think we just send it off and copy it to Diana as an incentive.

Bill's comments

"A relativistic model of the ..."

Alan's done alot more on this topic than I have, his name should preceed mine. DONE

The modeling description seems mostly complete (without haveing tried to program it) except that nothing is said about the radiative transfer calculations themselves. I presume that since all the emission is optically thin this is pretty straightforward. This still probably warrants a paragraph given the level of the detail in the
rest of the description.

Added a bullet in assumptions section

- I would think it worthwhile making a bigger deal of the difference in the model and observed values in Figure 7. It's currently mostly buried in sect 5.3.3 with a few waffles thrown in. If this is in fact
what it appears, it's telling alot about the velocity structure of the
jet. It's also in the summary in sect 7.1.2 but might be worth a mention in the abstract.

This prompted some changes:
- Fig 7 now has 3 panels, showing that the effect is present over 2, if not 3
distance bins.
- Fig 3 has 2 additional panels, showing grey-scale of sidedness in the range 0
- 5. This shows the effect much better, at the cost of Latex having a
prolonged breakdown over the figure locations. They are back in the right order
now, but some in odd places w.r.t. the text.
- Sentence added to abstract.
- Rewrite of transverse velocity section to be a bit more logical and positive.
- Mention in further work (mostly to head off referees who want us to follow up
now).

- p 3. col 1, line 20, dgeree -> degree DONE

- Figure 8. two figure (e)s, nofigure (d) DONE

- p 17 col 1, 1st paragraph
"... is qualitatively as expected but qualitatively inconsistent" =>
"... is qualitatively as expected but quantitatively inconsistent"
DONE

Obscure, I will admit

- p 17 col 2, last line mot -> not DONE

James's comments

> 1. I'm a bit concerned about having used the high-resolution image
> for fitting
> the counter-jet close to the nucleus, as it is pretty dim there.
> But I think
> things probably worked.

True, but if we had not used it I would be worried about having more
data
in one jet than the other and not comparing jet and counter-jet (which
is
critical for the model to work) like for like. In any case, as you say
I
think it all worked out in the end.

Fair enough.

> 2. Although the values and I got by optimizing with the new field
> ratio model
> were entirely consistent with James's, two of the error regions
> were
> seriously different: the inner region slope, which I found to
> have only an
> upper limit and the inner emissivity boundary location, which I
> found to have
> no lower limit. I have tried evaluating chi-squared over various
> regions,
> with essentially identical results. The problem is that the fit
> to the
> counter-jet is poor, because the sidedness ratio in the innermost
> part of the
> jet is inconsistent with the fitted velocity. Therefore, the
> counter-jets's
> contribution to chi-squared is essentially constant and although
> the main jet
> fit is quite well constrained, the chi-squared values never vary
> enough to
> meet the error prescription. I have explained all of this in the
> paper, but
> still don't understand how James got better constraints. All
> other error
> regions are consistent.

I've gone back and run what I think is pretty much the version of code

that produced my thesis results and I get pretty similar numbers to
you!
As I appear unable to reproduce the numbers in my thesis I'm at a loss
as
to where they came from. I agree with your reason as to where these
error
limits come from and your limits seem much more reasonable when I
think
about it.

No mods required

> 3. I haven't said much about the physical scales in NGC315, other than that they
> are bigger than those in other objects by about a factor of 5. If Section 6
> gets added, then that is the place for further comment. Otherwise, some other
> qualitative words should probably be added re low densities.

Agreed, probably best not to put in more than a passing comment until we
have the conservation law stuff done.

> 4. I have not included a discussion section per se. There was one, but it was at
> risk of repeating things said earlier, so I took out the main points and
> attached them to the physical parameters section.

Reads fine without one to me.

> 5. Maybe too much on low sidedness near core, apparent acceleration and all
> that? Also field structure/filamentation? A bit speculative, but might be
> right.

The low sidedness does appear to be cropping up in all the objects so I
think we need to talk about it, I don't think there is too much on it.

Not so sure about the filamentary structure, the model can't really provide any information about it's structure, particularly being non-axisymmetric, so I don't think this paper is the right place for it.

Cf above

> 6. Are there any other people who should be added to the author list? Is the
> order right?

Looks good.

Subject: Large-scale paper
From: rlaing@eso.org
Date: Tue, 10 May 2005 16:42:23 +0200 (CEST)
To: Bill Cotton <bcotton@nrao.edu>, Alan Bridle <abridle@nrao.edu>, James Canvin <jcanvin@physics.usyd.edu.au>

Here's a revised version. Further comments probably not worthwhile until there is more text, but you might like to look at revised Figs 1 and 9.

Notes appended.

Cheers

Robert

Changes to Multi-freq Observation paper
==

Alan's comments

My replies indented; ACTIONS in capitals.

Abstract

"We derive the distribution of the Faraday rotation over the radio source" .... DONE — not done

"These residual fluctuations are smaller ..." DONE ✓

"... they are produced by magnetic fields in a halo of hot plasma that surrounds the radio source." DONE ✓

  All fine

Introduction, line 6

Add "also" before "been", "has also been" DONE ✓

  OK

Sec 2.1, line 1

Add "were obtained" after "VLA data". DONE ✓

 OK - a verb would seem to be indicated

Table 1:

My VLA pre-archive data were from AB100 1413 A config 21-22 Dec 1980 at 25 MHz bandwidth.

 That's in the table; I don't particularly want to add proposal codes

I had actually missed that they were included. how chron order? - Beer, then config asc desc

There was also a C Band B config run on 27 Jun 1981 at 50 MHz, but this may not have been needed/included, this is just FYI.

That wasn't included in the 5 GHz dataset (we applied for and got new C configuration data - did we miss this at the time?)

Sec. 2.2 second para midway

"add "deconvolution" between "two" and "methods" DONE

OK

Section 3.

Yes, let's include the Mack et al. image for overall context, Figure 1 doesn't give a very good look at the higher levels, should we also show contours, or another transfer function as well?

I'm not too concerned about the higher levels, since they are emphasised in later plots. ok

lower levels ok

NEW COMPOSITE FIGURE MADE INCLUDING 327 MHZ IMAGE FROM MACK ET AL. 1997, COURTESY OF A VERY FAST RESPONSE FROM K-HM. MAYBE A BIT CRAMPED IN 1 COL?

Did James check the optical field at the position of "B"? (might be worth quoting the position, though it's a side-issue in this paper). POSITION ADDED

ADDED REFERENCE TO SOMETHING VISIBLE ON DIGITAL SKY SURVEY - MORE?

Is anything to be made of the labeled "ring" in Fig 4b? If not, do we need to label it?

Yes, it should be described in the (unwritten) Section 3. Text gratefully received.

Section 4

Can anything be safely said about tranverse s.index gradients in this case? Does C Band center-frequency question affect gradients at all?

Analysis in progress. I don't think the centre frequency is a problem (effects on I are less than on PA, since dependence is nu^-0.55 rather than nu^-2.

Section 5

Reads well and makes good sense, very few comments

Figure 6 caption

"Data are plotted only where ..." DONE now done

OK

Bottom of p.6, refers to a Fig. 7(d) when I think it means 7(b). DONE - LABELS FOR RM PROFILE FIGURE NOW CHANGED

Yes

Fig. 9, it might be helpful to put the angular
scale on panels (b) and (e) as well, but not at the
expense of destroying fit to single page format.

  Not sure about this. The grey scales are marginal at present and any smaller
  would cause problems. I'm inclined to delete the labelled wedges from the RM
  images as they are repeats of a bigger figure. Then there would be more room
  and less confusion. DONE: REMOVED WEDGES AND LABELS; GREY-SCALES NOW
ATTACHED TO
  RELEVANT PROFILES AND NOT LABELLED SEPARATELY; ALL PANELS HAVE X-AXIS
LABELS;
  LINE WEIGHTS CONSISTENT.

improved a lot, but "small central boxes" hard to see

Section 7.1

line 4, re-iterate value

Where's the white cross (not mentioned in the caption)

"The bulk of the mean RM of -75.7 rad m^-2 is
therefore likely ..." DONE

  OK

⇒ See Fig. 8

Insert "total" before "fitted gradient" in line
7, left column, p.9?

explain component of the gradient vs. total gradient.

  Don't understand, sorry

Also insert "of this gradient" after "component"
and before "transverse" on line 8? DONE

  Yes

instead of "much smaller amplitudes", say "amplitudes
that are 4-10 times smaller than those in 3C31 on
similar angular scales." DONE

  Good idea

Intent of the final boldface comment in this section
wasn't clear to me, Galactic contribution to what?
To the residual fluctuations? How could we
estimate that?

  To the residual fluctuations, yes. From structure functions in Simonetti &
  Cordes. Not sure whether this can be done accurately enough.

I think Section 7 should somewhere mention the
remarkable degree of orderliness of the jet magnetic
field that is shown in Figure 11. This strikes
me as a spectacularly well-organized that must
be connected to the general smoothness of the
intensity distributions in the jet (are there
ANY features above noise in a Sobel-filtered
image. for example? It might be worth remarking
on the similarities and differences from Figs.
20 and 21 of the 3C31 paper, as we have
globally similar field distributions but much
more local "weather" in 3C31 that strongly
correlated brightness gradients with local field
organization. Perhaps magnetic field orientation

deserves a short subsection of its own?

It actually has a section of its own (6), but no text as yet. Should include all of your points, and some connection to the model field structure as well.

Bill's comments

- It looks like the rotation measure is only given over the region where the 5 GHz data were useful, we should probably say this. Was there anything useful that could be derived from the outer parts of the jet from the 20 cm data alone? This might help strengthen the argument that most of the RM is from the Galaxy. This might be especially useful when comparing with the X-ray emission.

I've added a comment to the effect that the extent of the usable RM region at 5.5 arcsec is determined essentially by the 5 GHz primary beam, rather than the short-spacing coverage. It looks to me as if a lower resolution at L band could get something useful in the main jet only. Have added a \bf comment as a reminder and will check.

- p 6. last paragraph the reference to Fig 7d is really 7b DONE

Other tinkering

Added CO reference in Intro.

Acknowledge Karl-Heinz

Clarify meaning of "noise" in Fig 6 caption

Change notation for corrected \sigma_RM to avoid confusion with derivative

----

Cheers

Robert

rlaing@eso.org wrote:

Dear NGC315 people

Here (ngc315model.ps.gz) is a paper on the NGC315 model based on James's thesis work, with some (relatively minor) modifications.

Two changes to be aware of:

> The main change to James's model is the introduction of transverse field ratio variations. This improved the fit, and I think it will also help with 0326+39.
>
> The values of reduced chi-squared and numbers of points I quote are for a region bounded strictly by the model area. The original optimization used a fixed region which has a lot of blank sky included. Fortunately this makes no difference to the optimization or the errors, but it does make the number of points too big and the chi-squared too small.

The paper is, I think, complete with the exception of a completely missing section on conservation law analysis. I still don't have an X-ray model to work with, so we need to decide whether to wait. The paper is quite long as it is. Programs are ready to go, and have been tried with old X-ray data. I haven't talked to Diana recently, but was at a meeting with Mark Birkinshaw recently. I had hoped to visit Bristol in May, but my schedule requires circumnavigating the globe round about then.

would be good to have this, but it could also be deferred to 3C296

Comments please, especially:

1. I'm a bit concerned about having used the high-resolution image for fitting the counter-jet close to the nucleus, as it is pretty dim there. But I think things probably worked.

   won't χ^2 take care of this?

2. Although the values and I got by optimizing with the new field ratio model were entirely consistent with James's, two of the error regions were seriously different: the inner region slope, which I found to have only an upper limit and the inner emissivity boundary location, which I found to have no lower limit. I have tried evaluating chi-squared over various regions, with essentially identical results. The problem is that the fit to the counter-jet is poor, because the sidedness ratio in the innermost part of the jet is inconsistent with the fitted velocity. Therefore, the counter-jets's contribution to chi-squared is essentially constant and although the main jet fit is quite well constrained, the chi-squared values never vary enough to meet the error prescription. I have explained all of this in the paper, but still don't understand how James got better constraints. All other error regions are consistent.

   Can James comment on this?

3. I haven't said much about the physical scales in NGC315, other than that they are bigger than those in other objects by about a factor of 5. If Section 6 gets added, then that is the place for further comment. Otherwise, some other qualitative words should probably be added re low densities.

4. I have not included a discussion section per se. There was one, but it was at risk of repeating things said earlier, so I took out the main points and attached them to the physical parameters section.

   OK.

5. Maybe too much on low sidedness near core, apparent acceleration and all that? Also field structure/filamentation? A bit speculative, but might be right.

6. Are there any other people who should be added to the author list? Is the

order right?

Latex and individual plots on request.

I have done some work on a second paper, describing the structure on larger scales, RM, spectrum etc. This is very incomplete, but I've attached it for background (ngc315ls.ps.gz). The RM analysis is most complete. My earlier agonisings over depolarization have now been resolved. The RM plots do show that the CJ side variance is larger, I think, at least on scale probed by panel (e). Panel (f) is iffy. Looks right, but depends on errors output by RM being correct. Maybe repeat using Bill's software? Comments on this, the data reduction section (Bill? - some of the 5GHz history has vanished so I have guessed a few things) and the pictures would be useful; the rest is a construction zone. Assistance appreciated. I have done nothing towards high-resolution/X-ray comparison paper other than assemble a few bits and pieces.

Cheers

Robert

Misc working notes (not complete)

Figs based on those in JRC thesis. Changes as follows:

- Figures in this directory use optimised TRANSPOL model.

- New geometry sketch showing rough modelled region.

- Negative contours included in ilo.eps and ihi.eps (panel b in both).

- New transverse sidedness profile. Shows interesting discrepancy (noted in text).

- Corrections for Ricean bias. Currently made to ivec.eps (panels b and d) and pol.eps (panels b, c, e and f).

- Composite of degree of polarization grey-scales and profiles (pol2.eps) split from vector plots for legibility.

- Polarization grey scales and profiles are not blanked on polarized flux. This is important, because otherwise one can't tell whether a region has low p or just low I. Vector plots remain blanked on P to avoid spurious PAs.

- new grey-scale + vectors, model only, to show B perp region in CJ near core.

- Misc changes in labelling style.

- Average transverse polarization profiles in transp.eps. Essentially as in JRC thesis, but with TRANSPOL model and slightly different blanking strategy: - average over p profile; then blank where model I < 5*sigma_I (50 microJy).

- Cosmology formulae changed to Omega_m = 0.3, Omega_lambda = 0.7 (and angular size distance corrected). Use H0 = 70. Redshift assumed to be 0.01648.

- Scale =   0.335379786 kpc/arcsec
  1 grid unit = 69 arcsec = 23.141 kpc projected on the sky
  This is equivalent to 23.141/sin(37.87964) = 37.689 kpc along the jet axis.
  Images of intrinsic quantities are 226*.5*0.335379786 = 37.898 kpc across.
  I guess this is because the nucleus is at x = 1 and the end of the grid at x = 226; hence separation is actually 225 pixels = 37.730 kpc, which is indeed the closest pixel. I think we can ignore this.

- Try to keep same aspect ratio (0.72) as plot of geometry (edge.ps) and use same for contours of velocity. This would require 226 x 163 pixels, i.e. BLC 1 20;TRC 226 182.

- Query inconsistency in plot of velocity off-axis: we are plotting at the same value of SCOORD and streamline indices 0 and 1. For NGC315 the jet has not recollimated, so this is slightly non-obvious, although not a big effect.

- Fixed code for ademiss.f which wasn't scaling the reference emissivity correctly. Now returns 1 at the reference position (or almost, the condition isn't quite correct and there is no interpolation). Within the accuracy of the plot, however.

- Composite prof_schema.eps. Shows boundaries and errorson edge plots about velocity and emissivity profiles.

- Velocity contours. Grey-scale hopeless.

- Checked effect of allowing different variations of field ratios with streamline index. Linear appears to give the best fit for both. Stick with this. Code now allows use of RTINDEX and LTINDEX, but these can be fixed at 1 to force linear variation.

- Remade profiles of field component fraction on-axis. Technical problem: how do we calculate the edge values? First attempt is to calculate the position of the edge pixel and read from arrays ABSBETA, BxARR in makerestframe. This doesn't quite work because of rounding errors (i.e. the jet is <1 pixel wide at the start). Note that this make model predictions close in suspect because of undersampling - address sometime.

- Therefore calculate quantities explicitly for given ZJ and SL = 0.0. Looking at this part of makerestframe uncovered an inconsistency in the calculation of SCOORD for spine and shear layer, originating in JRC v22 (see ~/model/v24/doc/v24.new). No published results were affected.

- Composite plot to show velocities from Cotton et al., (corr H0 = 70), model, and near-in sidedness ratio. Comments on acceleration.

- Field ratio grey-scales + schematic as well as profiles for edge and axis.