Subject: referees report...
From: Diana Worrall <D.Worrall@bristol.ac.uk>
Date: Sun, 15 Apr 2007 12:20:26 +0100 (BST)
To: D.Worrall@bristol.ac.uk, Mark.Birkinshaw@bristol.ac.uk, abridle@nrao.edu, bcotton@nrao.edu, rlaing@eso.org

Dear co-authors,

Having been away, I've just picked up this referee's report which came in a few days ago.

Regards
Diana

=========================

```
>From cg@ras.org.uk Wed Apr 11 11:06:32 2007
X-Spam-Checker-Version: SpamAssassin 3.1.4 (2006-07-25) on sol.star.bris.ac.uk
X-Spam-Level: *
X-Spam-Status: No, score=1.3 required=5.0 tests=AWL,NO_REAL_NAME,
        SUBJ_ALL_CAPS autolearn=no version=3.1.4
Date: Wed, 11 Apr 2007 06:06:19 -0400 (EDT)
From: cg@ras.org.uk
To: D.Worrall@bristol.ac.uk
Subject: MNRAS: MN-07-0322-MJ
Mime-Version: 1.0
Content-Transfer-Encoding: 7bit
X-Errors-To: jr@ras.org.uk
```

Dear Prof. Worrall

I attach the reviewer's comments on your manuscript entitled "The inner jet of radio galaxy NGC 315 as observed with Chandra and the VLA", ref. MN-07-0322-MJ, which you submitted to Monthly Notices of the Royal Astronomical Society.

Minor revision of your manuscript is requested before it is reconsidered for publication.

You should submit your revised version, together with your response to the reviewer's comments via the Monthly Notices Manuscript Central site http://mc.manuscriptcentral.com/mnras . Enter your Author Centre, where you will find your manuscript title listed under "Manuscripts with Decisions." Under "Actions," click on "Create a Revision." Your manuscript reference will be appended to denote a revision.

IMPORTANT: do not submit your revised manuscript as a new paper!

You will not be able to make your revisions to the originally submitted files of the manuscript held on Manuscript Central. Instead, you must delete the original files and abstract and replace them with your revised files. Check that any requests for colour publication or online-only publication are correct. Carefully proof read the resulting PDF and HTML files that are generated.

When submitting your revised manuscript, you will be able to respond to the comments made by the reviewer in the space provided. You should also use this space to document any changes you make to the original manuscript. In order to expedite the processing of the revised manuscript, please be as specific as possible in your response to the reviewer.

Because we are trying to facilitate timely publication of manuscripts submitted to MNRAS, your revised manuscript should be uploaded promptly. If you do not submit your revision within six months, we may consider it withdrawn and request it be resubmitted as a new submission.

Please note that, due to the tight schedule, any post-acceptance changes notified after the paper has gone into production (i.e. the day after the acceptance email is sent) cannot be incorporated into the paper before it is typeset. Such changes will therefore need to be made as part of the proof corrections. To avoid excessive proof corrections and the delay that these can cause, you are strongly encouraged to ensure that each version of your paper submitted to MNRAS is completely ready for publication!

I look forward to receiving your revised manuscript.

Regards,

Claire

Claire Geeson (Miss)
Editorial Assistant
"Monthly Notices"
Royal Astronomical Society

email: cg@ras.org.uk
Tel/Fax: +44 (0)20 7734 3307 #212
Tel: +44 (0)1494 793544

Editor's Comments:

I consider that colour enhances the presentation: we will print the figures in colour in the journal.

Reviewer's Comments:

Reviewer: John Wardle

This is an extremely interesting paper that certainly merits publication in the Monthly Notices. I do have some suggestions to clarify certain points, and tighten up the discussion.

(1) This is a general point regarding color images: it is essential that the fundamental information is still apparent when printed in printed in black and white. Not everyone has access to a color printer, and nearly 10% of white males have some degree of color blindness. Also, the transition from one color to another is visually very striking, and appears to give a sharp edges to what are in fact smooth gradients. Thus the galaxy atmosphere in Figure 1 'looks' flat (red) and then falls off precipitously, which I don't think is the case. In Figure 14, it is very hard to distinguish the thin red and orange lines against the background of varying hues.That is a complicated enough figure that a black and white schematic might be much clearer. The authors might consider whether color conveys the information in the best manner in these images.

(2) The data reduction and analysis are described in meticulous detail. This is in contrast to the rather handwaving theoretical discussion. For several statements it is unclear if they are pure

speculation or are based on physical ideas and processes that have been discussed elsewhere in the literature and could be referenced.

(3) They state at the bottom of the first paragraph that "knowledge of the density and pressure of the hot ISM ....is essential to an understanding of jet dynamics." They derive the radial pressure profile from the x-ray observations and show it in Figure 13, but then, puzzlingly, do little with it other than the paragraph at the end of 5.1. How does their profile compare with what was assumed in the Colvin et al models? As a minimum I would expect that profile to be added to Figure 13. If they are mutually consistent, that would be most gratifying. If they are not, then what is the implication for the radio models? If that will be paper II, they should say so.

(4) It is unclear to this reader if the extraordinary radio filament is actually x-ray bright. In Figure 1 the x-ray jet seems to terminate rather abruptly at 8 arcsec from the core, overlapping only the first arcsec of the filament. In Figure 8 the x-ray emission is bright out to 20 arcsec, but that is the emission from the entire jet, not just the filament. The crucial paragraph is at the bottom of page 5. They acknowledge large uncertainties and state that the ratio of masked x-ray counts to total counts is 'consistent' with the ratio in the radio. Is it also consistent with the filament NOT being enhanced in x-rays (since the masked x-rays will still include emission from the body of the jet)? If that is the case, then their argument against the synchrotron instability does not hold up, and in the absence of other plausible ideas, it might be worth considering that instability in more detail. Clarification and error bars would be welcome here.

(5) Smaller questions and comments, in order:

Section 3.1. That is a valient effort to extract the polarization of the filament by itself. How big is the rotation measure correction? Is it constant over the jet? Do they know that the filament has the same rotation measure as the rest of the jet? You can't tell from a 5.5 arcsec RM map, but they may be able to do better than that. If you can convince yourself that you do not have any n.pi ambiguities, then you can drop the lower frequencies and dramatically improve the resolution of the RM map. The point is that it would be very interesting if the filament showed any sign of internal Faraday rotation. I have no idea if they can answer that with the available data.

Page 5, middle paragraph. It would be helpful to show the SED for the jet, including the optical limit, because this makes the case for a synchrotron origin of the x-rays.

Page 7. It would be helpful to write explicitly the expression for the beta model, and give a reference.

Last full senetence on that page: "To estimate ....." I don't understand the 2.5 and 12 arcsec numbers. They are not deprojected anglular distances or they would be bigger than 4 and 20. Please clarify.

Page 8, bottom of first column. "There are physically reasonable...." A somewhat mysterious sounding sentence. Are they talking about cooling flow models? Is there a reference?

By the time the reader gets to the end of section 4, a table of spectral fits, temperatures, spectral indices etc for the several different regions of the source would be welcome.

Section 5.1. How far out of equipartition must the jet be for the IC/CMB model to work? The steep x-ray spectral index does not by itself rule out IC models. You can get that if the scattering electrons are near the upper end of the electron energy distribution.

End of 3rd paragraph: what is meant by "adiabatic models" here --

models with no particle acceleration or models with no additional energy input?

Last line: "It is more likely ..." Another mysterious sentence. Is this just a statement about available energy or an acceleration mechanism that has been studied? Is there a reference? What sort gammas do they need for x-ray synchrotron emmission -- millions, surely? Can shear really produce that?

Sections 5.3-5.4: I get the impression that the boundaries of the jet are straight and symmetrical and show no signs of precession or transverse motion associated with a K-H instability. There is a partial mention of this at the bottom of page 10, but it really informs the whole discussion, and should go earlier.

Section 5.5: It is difficult to imagine what these injectors actually are. Is this just a kinamatic straw man, or do they have a physical model in mind?

Below is our response to the Referee's suggestions.

>(1) This is a general point regarding color images: it is essential
>that the fundamental information is still apparent when printed in
>printed in black and white. Not everyone has access to a color
>printer, and nearly 10% of white males have some degree of color
>blindness. Also, the transition from one color to another is visually
>very striking, and appears to give a sharp edges to what are in fact
>smooth gradients. Thus the galaxy atmosphere in Figure 1 'looks' flat
>(red) and then falls off precipitously, which I don't think is the
>case. In Figure 14, it is very hard to distinguish the thin red and
>orange lines against the background of varying hues.That is a
>complicated enough figure that a black and white schematic might be
>much clearer. The authors might consider whether color conveys the
>information in the best manner in these images.

We sympathize with the points made by the referee, and our choice to use colour for specific figures was made only after satisfying ourselves that this provided added value. We are pleased with the editorial decision to print the figures in colour in the journal.

With regards Figure 1, the inner region is saturated in order to use the full dynamic range of the display to show the X-ray jet. We have added this point to the caption together with a reference to the radial profile of galaxy emission (albeit from an earlier shallower observation) in Worrall et al. 2003. The galaxy emission from this new observation will be discussed in more detail in a forthcoming paper (see point 3 below).

We agree with the referee that it was hard to distinguish the thin red and orange lines in Figure 14, and we have remade the figure as two panels, separating the two kinematical descriptions. Use of colour in this figure is worthwhile intrinsically and to relate to Figure 1. We have revised the caption accordingly.

Typo in caption

>(2) The data reduction and analysis are described in meticulous
>detail. This is in contrast to the rather handwaving theoretical
>discussion. For several statements it is unclear if they are pure
>speculation or are based on physical ideas and processes that have
>been discussed elsewhere in the literature and could be referenced.

The discussion is generally based on physical ideas and we have added references where possible. We have shortened the more handwaving discussion at the end of section 5.5 (and see below).

>(3) They state at the bottom of the first paragraph that "knowledge of
>the density and pressure of the hot ISM ....is essential to an
>understanding of jet dynamics." They derive the radial pressure
>profile from the x-ray observations and show it in Figure 13, but
>then, puzzlingly, do little with it other than the paragraph at the
>end of 5.1. How does their profile compare with what was assumed in
>the Colvin et al models? As a minimum I would expect that profile to
>be added to Figure 13. If they are mutually consistent, that would be
>most gratifying. If they are not, then what is the implication for the
>radio models? If that will be paper II, they should say so.

We now say at the end of Section 4.2 that the external gas pressure will be combined with the kinematical models of Canvin et al into a dynamical model for the jet in a forthcoming paper. The Chandra data are best for describing the inner gas distribution, and are uniquely required for the jet results presented here, but to construct dynamical models we will combine Chandra results from this paper with those from lower-spatial-resolution XMM-Newton data.
In the last paragraph of section 5.1 we now refer to the 'conservation-law analysis' as work in preparation.

>(4) It is unclear to this reader if the extraordinary radio filament

>is actually x-ray bright. In Figure 1 the x-ray jet seems to terminate
>rather abruptly at 8 arcsec from the core, overlapping only the first
>arcsec of the filament. In Figure 8 the x-ray emission is bright out
>to 20 arcsec, but that is the emission from the entire jet, not just
>the filament. The crucial paragraph is at the bottom of page 5. They
>acknowledge large uncertainties and state that the ratio of masked
>x-ray counts to total counts is 'consistent' with the ratio in the
>radio. Is it also consistent with the filament NOT being enhanced in
>x-rays (since the masked x-rays will still include emission from the
>body of the jet)? If that is the case, then their argument against the
>synchrotron instability does not hold up, and in the absence of other
>plausible ideas, it might be worth considering that instability in
>more detail. Clarification and error bars would be welcome here.

The relevant analysis is indeed at the bottom of page 5, and we have now provided more information to support the reality of the filament X-ray detection. The 90 counts we gave were net counts, with our best estimate of the background from the diffuse jet subtracted. We now provide values for the gross (184), background estimate (94), and net (90) counts, and say why we think the regions used to sample the background from the diffuse jet should be representative. Measuring 184 counts where 94 are expected is highly significant from statistics alone. The systematic uncertainty in the background estimate is hard to quantify, but we do not think the estimate can be low by the factor of 2 needed to render the filament undetected in X-rays. Thus the data do indeed support the filament being bright in X-rays, consistent with the X-ray to radio ratio in the surrounding diffuse jet.
For this reason the argument against the synchrotron instability holds up.

We have amplified the text to make it clearer that diffuse-jet background is subtracted, and we now give separately the number of gross, estimated background, and net counts. We now say: '... and 184 X-ray counts (0.3-5 keV) were summed over the mask. Since the mask allows through both filament and diffuse-jet X-ray counts, it was necessary to estimate and subtract the background contributed by the diffuse jet by sampling regions within the jet envelope that are adjacent to the filament. The wiggly nature of the filament means we sampled diffuse regions at similar transverse distances from the jet axis as the filament. The background estimate was 94 counts, giving a statistically significant 90 net X-ray counts. Within the rather large statistical uncertainties (13 per cent), and harder to quantify systematic uncertainties in the diffuse-jet contribution, this is consistent with being the same fraction of total jet emission as in the radio.'

>(5) Smaller questions and comments, in order:
>
>Section 3.1. That is a valient effort to extract the polarization of
>the filament by itself. How big is the rotation measure correction? Is
>it constant over the jet? Do they know that the filament has the same
>rotation measure as the rest of the jet? You can't tell from a 5.5
>arcsec RM map, but they may be able to do better than that. If you can
>convince yourself that you do not have any n.pi ambiguities, then you
>can drop the lower frequencies and dramatically improve the resolution
>of the RM map. The point is that it would be very interesting if the
>filament showed any sign of internal Faraday rotation. I have no idea
>if they can answer that with the available data.

We have repeated the analysis with a 2-frequency RM image at the higher resolution of 1.5 arcsec (Fig 11d of Laing et al. 2006a), using the 5.5-arcsec RM image to resolve ambiguities. The results differ by at most 3 degrees in apparent magnetic field angle, some component of which is due to noise. The choice between the two versions is not obvious --- the higher-resolution image is noisier --- but we follow the suggestion and now show the higher-resolution version in Fig 2.

The maximum RM variation across the jet at 1.5 arcsec resolution corresponds to 2.4 degrees of rotation at 5 GHz, compared with an average value of 16 degrees. Also, we see no depolarization (Laing et al. 2006a; section 5.2 --- actually, there is a slight but not significant increase of degree of polarization with wavelength) so there is no reason to suspect either internal depolarization or large unresolved RM gradients across the filament.

We have elaborated on these issues in Sec 2.2.

>Page 5, middle paragraph. It would be helpful to show the SED for the
>jet, including the optical limit, because this makes the case for a
>synchrotron origin of the x-rays.

We have not added this plot because the case for synchrotron radiation is more strongly made on the basis of departure from equipartition (see below), and an SED with just radio, X-ray and optical upper limits is mostly empty. We have, however, now quoted an optical upper limit in the text based on ground-based observations which are more sensitive than HST for this purpose, given the size of the jet.

>Page 7. It would be helpful to write explicitly the expression for the
>beta model, and give a reference.

Done as a footnote.

>Last full senetence on that page: "To estimate ....." I don't
>understand the 2.5 and 12 arcsec numbers. They are not deprojected
>angular distances or they would be bigger than 4 and 20. Please
>clarify.

Since this is a data paper, the jet figures show what is actually seen, i.e., 'projected' angular scales. We must deproject the jet to get the correct comparison angular radius for the gas. We have modified the text, in particular by inserting the word 'projected' before 'distances along the jet between 2.5 and 12 arcsec', in order to avoid confusion.

>Page 8, bottom of first column. "There are physically reasonable...."
>A somewhat mysterious sounding sentence. Are they talking about
>cooling flow models? Is there a reference?

Rather than cooling-flow models for the gas, the comment was about our modelling of the underlying mass distribution. On reflection, it is not really relevant to the current paper and the length of text that would be required to explain it properly would be disproportionate to what it might add. We have therefore deleted the comment.

>By the time the reader gets to the end of section 4, a table of
>spectral fits, temperatures, spectral indices etc for the several
>different regions of the source would be welcome.

We have added two tables -- one for the power-law components from the core and jet, and one for the run of gas parameters with distance from the nucleus.

>Section 5.1. How far out of equipartition must the jet be for the
>IC/CMB model to work? The steep x-ray spectral index does not by
>itself rule out IC models. You can get that if the scattering
>electrons are near the upper end of the electron energy distribution.

The factor is large. We are now more quantitative.
We have removed the phrase which says that the steep X-ray spectrum supports synchrotron emission, since the electrons scattered by the dominant photon field (the CMB) are at energies below those emitting the observed radio synchrotron emission, and we can't rule out a steepening of the electron spectrum at low energies.

Section 5.1 now reads:

`...significantly underpredict the observed X-ray emission. For the diffuse jet region over which the X-ray spectrum is extracted, the magnetic field strength, B, would need to be at least a factor of 45 below the minimum-energy value, $B_{\rm me}$, increasing the total energy in the source by at least a factor of 280 as compared with minimum energy. This is in contrast to the range $0.3 B_{\rm me} < B < 1.3 B_{\rm me}$ typically measured for diffuse radio-emitting plasma where the X-ray emission {\it is} reliably associated with inverse Compton scattering \citep{croston}. The results therefore support a synchrotron ...'

>End of 3rd paragraph: what is meant by "adiabatic models" here --
>models with no particle acceleration or models with no additional
>energy input?

We have amplified the text to say 'with models in which the relativistic particles change energy only by adiabatic losses and the magnetic field is frozen into the flow'

>Last line: "It is more likely ..." Another mysterious sentence. Is
>this just a statement about available energy or an acceleration
>mechanism that has been studied? Is there a reference? What sort
>gammas do they need for x-ray synchrotron emmission -- millions,
>surely? Can shear really produce that?

We have added a reference to Stawarz and Ostrowski as a paper presenting an example of the acceleration process we are invoking.

>Sections 5.3-5.4: I get the impression that the boundaries of the jet
>are straight and symmetrical and show no signs of precession or
>transverse motion associated with a K-H instability. There is a
>partial mention of this at the bottom of page 10, but it really
>informs the whole discussion, and should go earlier.

We have added a sentence at the beginning of paragraph 2 of 5.3 to say 'The jet envelope is relatively symmetric about the jet axis. Within this, the appearance of the filament suggests....'
We have also reworded the first paragraph of section 3.1 to say the jet ...'contains a prominent oscillatory filament displaying a number of'

>Section 5.5: It is difficult to imagine what these injectors actually
>are. Is this just a kinamatic straw man, or do they have a physical
>model in mind?

It's a kinematic straw man. We don't see the issue so much as a problem with material being available in the centre of an AGN. What would need to be demonstrated is how the injected material could survive from pc to kpc scales. We have shortened the last paragraph of the section and removed the word 'model' from this part of the text.

Subject: Re: Comments on NGC315 paper
From: rlaing@eso.org
Date: Wed, 30 Aug 2006 14:38:32 +0200 (CEST)
To: Bill Cotton <bcotton@nrao.edu>
CC: Alan Bridle <abridle@nrao.edu>

On Wed, 30 Aug 2006, Bill Cotton wrote:

> Could not the
> filament/knots be the result of turbulence or other instabilities causing either variations in pressure, magnetic field strength or local motions, some of which would have favorable Doppler boosting in our direction? I would think that such instabilities could possibly result in apparent large scale structures in the enhanced regions but am not prepared to back this up with modeling.

Dear Bill

Agreed. Variations in all of n, B and beta are almost inevitable, I think. One could start to think about the constraints imposed by the constancy of the radio/X-ray ratio, but without some constraints on the acceleration process it would be very hard to get anywhere.

Have a good trip - even my sketchy knowledge of US geography says that Virginia to Oregon is a long way.

Regards

Robert

Subject: Re: Comments on NGC315 paper
From: Bill Cotton <bcotton@nrao.edu>
Date: Wed, 30 Aug 2006 08:04:41 -0400
To: Robert Laing <rlaing@eso.org>, Alan Bridle <abridle@nrao.edu>
CC: Bill Cotton <bcotton@nrao.edu>

Alan, Robert,

I concur with your analysis. The high resolution image of 3C296 does look so much like NGC315 that it's hard to claim that NGC315 is in any way unique - except that the enhanced regions appear like a spiral. The magnetic field along the filament does suggest some sort of coherent feature but this needen't be something wound around the outside. The relatively constant radio/X-ray spectral index make it difficult to distinguish between "filament" and "jet" in a region which is clearly different from the rest of the jet.

The apparent tendency for FRIs to show this sort of feature in the flaring region must be telling us something more general than just the case of NGC315. Since this is also the region where strong deceleration of the jet flow and strong particle (re)acceleration takes place, it's hard to image that the actual flow in the jet is as smooth and well behaved as in the kinematic model. Could not the filament/knots be the result of turbulence or other instabilities causing either variations in pressure, magnetic field strength or local motions, some of which would have favorable Doppler boosting in our direction? I would think that such instabilities could possibly result in apparent large scale structures in the enhanced regions but am not prepared to back this up with modeling.

Since the 3C296 result is further along in the publication process than this NGC315 paper, it would be useful to reference it in a more general discussion of off-axis knots in the flaring region.

I'm afraid that I'm going to be even more out of contact than while at the IAU until late next week when I'm going to a workshop in Greenbank. I'll be helping my daughter drive to Oregon so have about the same internet access as Alan's canoe. You may use my general support for steering the discussion towards the more general case as you will with Diana. I'll be back in "normal" mode on the 11th.

-Bill

Subject: Various NGC315-related matters
From: Robert Laing <rlaing@eso.org>
Date: Tue, 22 Aug 2006 15:31:36 +0200 (CEST)
To: Alan Bridle <abridle@nrao.edu>

Dear Alan

1. First to pick up the thread of getting our act together on radio stuff for NGC315 ...

You will have seen my message on relative resolution for 3C31, 296 and NGC315, but you might want to have another look at the images, as there is an element of subjectivity.

Would you agree with the following? I think we have better transverse resolution (in the sense of jet width/fwhm) where the jet brightens in NGC315 than we have in the other two objects. Also the region with bright, non-axisymmetric structure is relatively longer. So we probably have a better chance of recognising a filament in 315. But there are regions in both of the other sources where we could have seen a filament (say >5 or 6 beamwidths across the jet) but do not.

2. I have been concentrating recently on trying to quantify the relation between the X-ray and radio. My thought was that the more solid information we could provide the less the fluffy interpretation would matter. I hope you think this is a good strategy - any other suggestions welcome.

I think you have probably been copied most of the correspondence on this, but to summarise: the basic story is much clearer from the profiles. We have a high X-ray/radio ratio in the faint inner jet, which drops where the radio turns on and becomes much smaller in the deceleration region, although the X-rays are still definitely present. I think that this is more-or-less what we see in 3C31 and 296. We should play around with transverse profiles and averaging a bit more in those sources too. [In fact, it occurred to me that the papers on X-ray jets in FRI's are often a little subjective about where the X-ray emission ends. Comparing the X-ray flux within the outer radio isophote with a corresponding background region ought to do better.] The X-ray and radio transverse profiles in NGC315 are remarkably similar when averaged over the region where both are bright. It is a shame that the signal is too low from 18 - 30 arcsec to say whether the profiles differ at the edges where we saw the start of the spectral flattening - there certainly isn't any significant difference that I can see. It will be a challenge to the theorists to explain the similarity in averaged transverse profiles over such a big region given the synchrotron lifetime issues - should we make something more of this? I guess we probably want a lifetime estimate for the diffuse X-ray emission as well as the example knot currently quoted?

3. Re the filament: I had another look at the Bristol group's paper on 3C346 and was struck by their completely different fluffy explanation for something that looks superficially quite like the NGC315 filament. I fear that mentioning this would be unwise, however. I don't think it is out of the question that the 3C346 "jet" is actually the brightest part of a wider structure.

Cheers

Robert

Subject: Re: Longitudinal profile of filament emission
From: Diana Worrall <D.Worrall@bristol.ac.uk>
Date: Mon, 21 Aug 2006 18:44:21 +0100
To: D.Worrall@bristol.ac.uk, rlaing@eso.org
CC: Mark.Birkinshaw@bristol.ac.uk, abridle@nrao.edu, bcotton@nrao.edu

Dear Robert,

Thanks for the new transverse profiles. The agreement (especially for
the inner jet) between the X-ray and radio is impressive, and so it's probably now worth including the figure in the paper.

As for the jet, I anticipated the issues you mention when I suggested we just made the comparion for various knots.
I would use your region descriptors and extract X-ray flux and local background for each knot (or knot complex) individually.

> the same area - in other words, the filament is sitting on top of a lot of
> more extended emission which the modelling has tried to remove. It would
> be quite tricky to play the same sort of tricks to isolate the filament in
> the X-rays. Did you do this in order to estimate the X-ray flux in the
> filament?

To estimate the fraction of X-rays in the filament I used the radio mask as a filter, and shifted it down (still in the envelope of diffuse emission) to measure the background for subtraction.
For individual knots I'd use regions symetrically about the axis, or adjacent regions where the filament crosses the axis.

> I'm not sure that this exercise is telling us anything very useful.

I think we'd probably find the ratio of X-ray to radio is not the same over
the various knot regions (see image comparison)
-- although without checking I don't know if it's supported by the statistics.
I don't think it's a critical point, though. Since we believe the X-rays are synchrotron and we've got particle transport and acceleration throughout, it would seem highly unlikely that the (radio) dogs would have an identical (X-ray) tails throughout.

Best wishes
Diana

Subject: Re: Longitudinal profile of filament emission
From: Diana Worrall <D.Worrall@bristol.ac.uk>
Date: Mon, 21 Aug 2006 23:09:19 +0100
To: D.Worrall@bristol.ac.uk, rlaing@eso.org
CC: Mark.Birkinshaw@bristol.ac.uk, abridle@nrao.edu, bcotton@nrao.edu

Dear Robert,

> Incidentally, the X-ray
> normalizations are done by eye - do you want something less
> subjective?
> The normalization factors are different for the two panels, of
> course, as
> required by the longitudinal profile. Might be worth translating to
> \alpha_RX for those interested in such things.

We give the value of alpha_rx for the region used for the jet spectrum in Section 3.2.2. I think it's OK for the profiles to have an arbirary X-ray normalization, but we should say what the alpha_rx is separately for each.

To do this, I really need to use the same region descriptors as you have used for both on-source and background.
If you have anything you can send me, please do, otherwise I'll define something as best I can to match the regions in (a) and (b). What are the net X-ray counts in each of these figs -- if I get something similar I will have done the regions OK.
What are your radio flux densities for (a) and (b)?

>> To estimate the fraction of X-rays in the filament I used the
>> radio
>> mask as a filter, and shifted it down

> sorry - down in which coordinate system - do you mean in -y on the
> current
> grid?

Yes, roughly, and slightly to the left so similar distance down the jet.

Regards
Diana

Subject: Longitudinal profile of filament emission
From: Robert Laing <rlaing@eso.org>
Date: Sun, 20 Aug 2006 16:44:02 +0200 (CEST)
To: Diana Worrall <D.Worrall@Bristol.ac.uk>
CC: Alan Bridle <abridle@nrao.edu>, Bill Cotton <bcotton@nrao.edu>, Mark Birkinshaw <Mark.Birkinshaw@Bristol.ac.uk>

Dear Diana

This is a first go at profiles of the radio and X-ray emission in the area of the filament. What I did was to make a mask based on the model of the filament alone, apply it to both radio and X-ray and then plot the longitudinal profiles, with background subtraction as before. The results are the full line (radio) and points (X-ray).

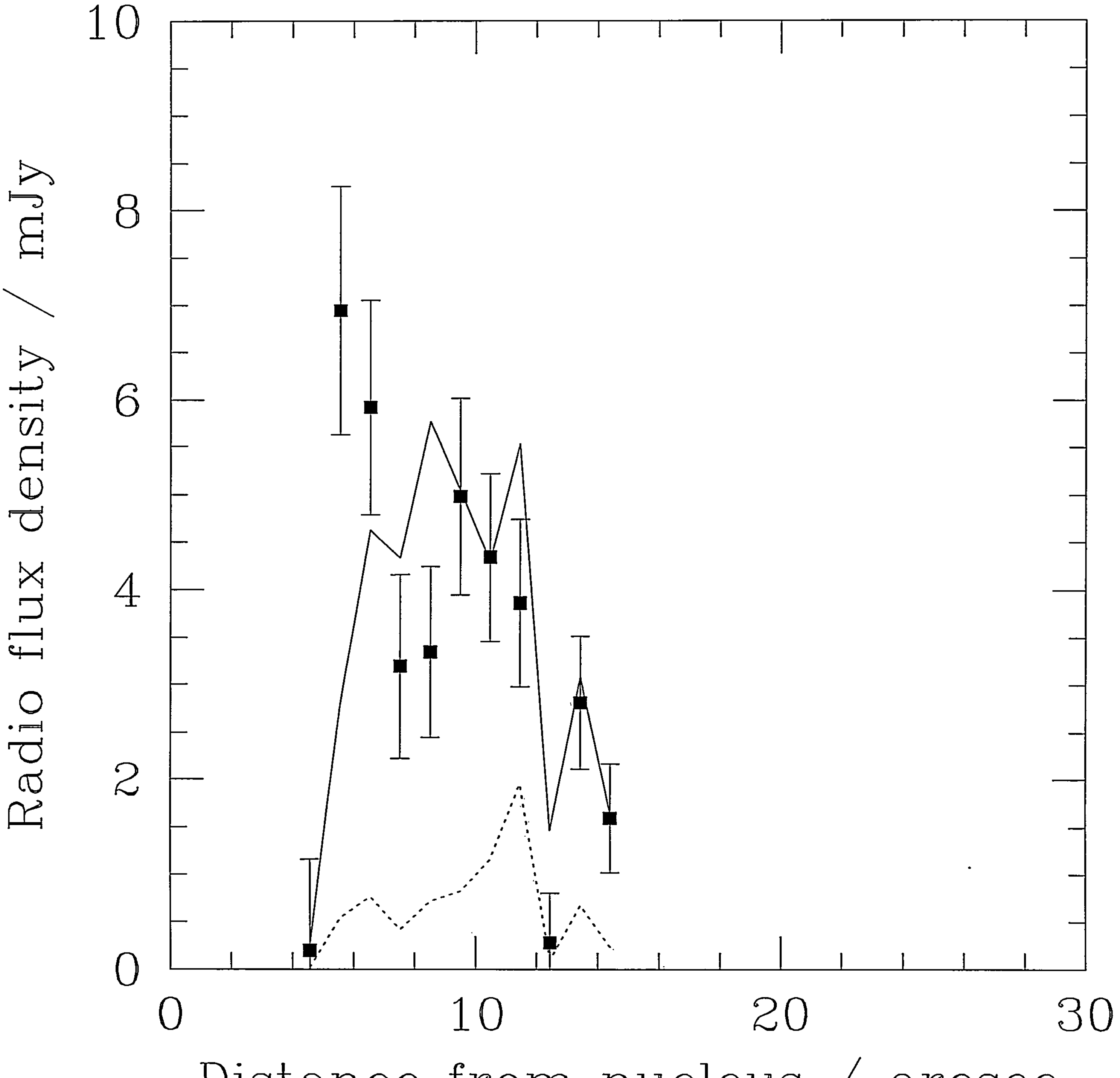
The trouble with this approach is that it does not isolate the filament properly. The dotted profile is for the filament model in the radio. As you will see, it has a great deal less flux than the radio profile over the same area - in other words, the filament is sitting on top of a lot of more extended emission which the modelling has tried to remove. It would be quite tricky to play the same sort of tricks to isolate the filament in the X-rays. Did you do this in order to estimate the X-ray flux in the filament?

I'm not sure that this exercise is telling us anything very useful.

[Incidentally, the X-ray counts are scaled as in the other longitudinal profiles I sent, so the X-ray/radio ratio in the area of the filament is very close to that in the jet as a whole over this range of distances. But that isn't very surprising, given that much of the flux comes from the area of the filament, but at least it is a consistency check.]

Regards

Robert



Radio flux density / mJy
0
2
4
6
8
10
0
10
20
30
Distance from nucleus / arcsec

Subject: Re: Section 5
From: Robert Laing <rlaing@eso.org>
Date: Fri, 18 Aug 2006 19:52:33 +0200 (CEST)
To: Robert Laing <rlaing@eso.org>
CC: Alan Bridle <abridle@nrao.edu>

On Fri, 18 Aug 2006, Robert Laing wrote:

> > Also I was a bit
> > surprised by the assertion that observations of 3C31
> > could not detect a filament like that seen in NGC315.
> > Where does that come from? Fig. 5 of our first (2002)
> > 3C31 paper actually shows data at higher linear resolution
> > than we are showing here for NGC315, and also shows
> > a complex of knots in the bright region of the jet, some
> > clearly off-axis. So I'm not sure why this comment
> > got in.
>
> It's mangled from a comment of mine and I haven't got round to
> complaining. I think it probably is correct that we would not have
> identified a filament in 3C31 or 296. The linear resolution in 3C31
> is better, but the width of the flaring region is also less than in
> NGC315. It doesn't take much to lose the appearance of a filament
> (e.g. the 1.5 arcsec images of NGC315). But 3C296 has a structure
> which could well turn into a filament given another factor of 2 in
> resolution. I think the point that has gone missing is that we see
> non-axisymmetric structures in ALL of the objects with adequate
> resolution - we should reiterate this.

I think I was suffering from false memory syndrome. I have just checked the images for 3C296, 3C31 and NGC315. At comparable distances (where the non-axisymmetric stuff is) they have jet width/FWHM = 5, 7 and 8, respectively. So NGC315 isn't much better resolved.

R

Subject: Re: Section 5
From: Robert Laing <rlaing@eso.org>
Date: Fri, 18 Aug 2006 18:40:11 +0200 (CEST)
To: Alan Bridle <abridle@nrao.edu>
CC: Robert Laing <rlaing@eso.org>

On Fri, 18 Aug 2006, Alan Bridle wrote:

> Dear Robert
>
> I'm starting to get stuck into NGC315 hi-res Section 5 text, which I think needs to be abbreviated as the detailed physics will remain unclear when it's done anyway. I'd like us to sort out the purely radio parts among ourselves first anyway, so as not to distract from any points where we may need to arm wrestle our X-ray colleagues into a less elaborate discussion.

Good idea

> There's a couple of things in the penultimate para of Section 5 introduction that bother me. One is where it describes the NGC315 "filament" as a "structure of relatively uniform brightness". That's "relative to M87" but isn't that rather like describing a six-foot human being as "relatively short" compared to an Olympic basketball player? I'd sooner emphasize that M87 has extreme brightness fluctuations, e.g. its Knot A.

I'm not sure of the relevance of this aside. It seems perverse to
compare
structures which occupy the entire width of a jet - like the M87 knots
- with a single filament. I think the sentence could go.

> Also I was a bit surprised by the assertion that observations of 3C31 could not detect a filament like that seen in NGC315. Where does that come from? Fig. 5 of our first (2002) 3C31 paper actually shows data at higher linear resolution than we are showing here for NGC315, and also shows a complex of knots in the bright region of the jet, some clearly off-axis. So I'm not sure why this comment got in.

It's mangled from a comment of mine and I haven't got round to complaining. I think it probably is correct that we would not have identified a filament in 3C31 or 296. The linear resolution in 3C31 is better, but the width of the flaring region is also less than in NGC315. It doesn't take much to lose the appearance of a filament (e.g. the 1.5 arcsec images of NGC315). But 3C296 has a structure which could well turn into a filament given another factor of 2 in resolution. I think the point that has gone missing is that we see non-axisymmetric structures in ALL of the objects with adequate resolution - we should reiterate this.

> The main difference with 3C31 and 3C296
> s that they clearly show the "arcs" and other
> non-axisymmetric further out before they deflect through
> large angles, while NGC315's jet has a relatively smooth
> brightness distribution and stays straight much longer.

This is true, but on scales larger than we cover in the present paper.

> So there's some stuff leading into the filament mechanism
> discussion for NGC315 that strikes me as a little odd.
>
> Also there's some dichotomy between what's said in
> places about the filament getting close to the edge of the jet
> (5.1 para 4)

That must be left over from an earlier draft, I think. I've gone to some trouble to redo the figures partly because of this point - the Bristol people had got the impression that the filament was close to the edge because the edge was difficult to see in the grey-scale.

> and it being at 60% of the "radius" of the jet
> (5.1 para 3).

That's correct, I believe.

> I think we may need to standardize our own picture of what
> we want to emphasize here before trying to redo Sections
> 5.1 and 5,2 (which I am inclined to try to combine into a
> single, briefer, discussion) that deals with
>
> (a) synchrotron instability
> (b) K-H instability
> (c) advection down the jet of a bright feature that rotates
> either (1) with the jet (excluded by Doppler) or (2) with a
> moving source within a stationary jet (needs to be something
> in an orbit close to the BH which conveniently turns on
> and off on the right time scale.
>
> My gut feeling is that the idea that some helical instability
> mode has been forced from a rotating source, so the KH
> growth rates aren't determining the whole picture seems
> a tad more likely than tying this all back to some specific
> orbiting "hot spot" close to the BH.

I find it extremely hard to believe that something is injected close to the hole, or even the torus (if any) and persists to large scales. What could it be? Relativistic particles lose energy and expand; field also expands, cold matter gets ripped up and mixes.

We know that non-axisymmetric structures in the flaring regions, after the jets brighten, are generic (cf. dicussion earlier). It may be that filaments are quite common; or maybe NGC315 is just the most obviously coherent example. So I don't buy an explanation that makes the source very special. I think that these structures have to be associated with whatever turns on the radio emission. The fact that the filament is bright in X-rays as well as radio and the initial correspondence is quite good does suggest to me that we might have a magnetic structure into which particles of various energies diffuse rather than the sort of pressure enhancement associated with a hydrodynamic instability like KH (where the X-ray emitting electrons would probably lose their energy quite quicky). We know that a mechanism to accelerate electrons to gammas of 10^7 - 10^8 must be working over the whole of the jet, not just at isolated sites. I don't know what mechanism is capable of producing a magnetic structure like this.

That's enough rambling for now - I must go and do some shopping otherwise I will have no dinner.

Cheers

Robert

Subject: Re: Transverse profiles
From: Diana Worrall <D.Worrall@bristol.ac.uk>
Date: Fri, 18 Aug 2006 17:48:10 +0100
To: rlaing@eso.org
CC: D.Worrall@bristol.ac.uk, Mark.Birkinshaw@bristol.ac.uk, abridle@nrao.edu, bcotton@nrao.edu

Hi Robert,

Agreed. Taking an average bkg is fine for the transverse profile. Figure not needed in the paper.

There are more statistics in the region of the filament, and so comparing the transverse profiles there might be interesting as long as not too close to the core.

I've checked the X-ray, and within uncertainties agree with the radio estimate of about 10% of the emission in the filament -- have added something to the draft on that.

Regards
Diana

Subject: Transverse profiles
From: Robert Laing <rlaing@eso.org>
Date: Fri, 18 Aug 2006 17:43:32 +0200 (CEST)
To: Diana Worrall <D.Worrall@Bristol.ac.uk>
CC: Alan Bridle <abridle@nrao.edu>, Bill Cotton <bcotton@nrao.edu>, Mark Birkinshaw <Mark.Birkinshaw@Bristol.ac.uk>, Robert Laing <rlaing@eso.org>

On the question of whether the transverse profiles of X-ray and radio emission
are different in the region where we see spectral index gradients between 1.4
and 5 GHz.

I looked at a region of the jet between 18.3 and 30 arcsec from the nucleus
(measured along the axis). I then averaged along sectors defined by radii from a
point on the axis 8.5 arcsec from the core (this is defined by the opening angle
of the outer isophote in the region of interest). I binned in 5 deg intervals
from the axis. The X-ray image had a constant background subtracted, determined
over the same range of distances (I think it is actually pretty constant that
far out).*

The results are in transprof.ps. Radio is the curve, X-ray (arbitrarily scaled)
the points; errors are Poisson.

The answer is that the X-ray profile is certainly consistent with the radio, but
we couldn't rule out differences at the edge. I'm not sure whether we want to
show this plot - it is reassuring to me that the X-ray profile can be determined
at all in this region and it bolsters our case for a secure detection far from
the nucleus.

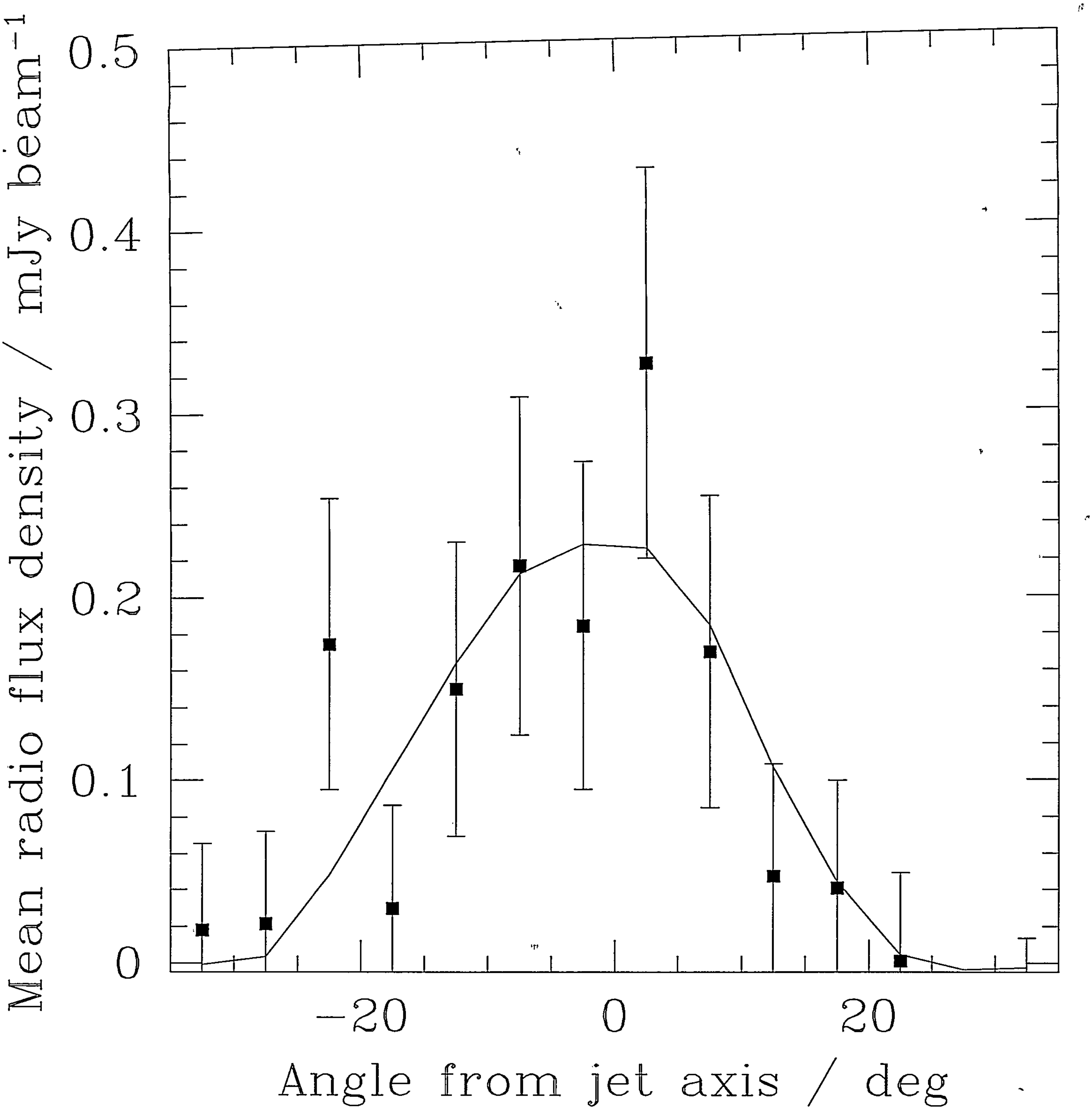
I guess the next (and final?) thing to try is a longitudinal profile for the
filament alone.

Cheers

Robert

* I looked again at the longitudinal profile with a constant

background value
determined at and used for distances >18 arcsec rather than the smaller 10.7
arcsec I used before. This is in good agreement with the profile using local
averaging for the background. I don't want to subtract a varying background
for the transverse profile if I can help it - are you happy with this?



Mean radio flux density / mJy beam^{-1}
0.5
0.4
0.3
0.2
0.1
0
−20
0
20
Angle from jet axis / deg

Subject: Section 5
From: Alan Bridle <abridle@nrao.edu>
Date: Fri, 18 Aug 2006 11:58:42 -0400
To: Robert Laing <rlaing@eso.org>

Dear Robert

I'm starting to get stuck into NGC315 hi-res Section 5 text, which I think needs to be abbreviated as the detailed physics will remain unclear when it's done anyway. I'd like us to sort out the purely radio parts among ourselves first anyway, so as not to distract from any points where we may need to arm wrestle our X-ray colleagues into a less elaborate discussion.

There's a couple of things in the penultimate para of Section 5 introduction that bother me. One is where it describes the NGC315 "filament" as a "structure of relatively uniform brightness". That's "relative to M87" but isn't that rather like describing a six-foot human being as "relatively short" compared to an Olympic basketball player? I'd sooner emphasize that M87 has extreme brightness fluctuations, e.g. its Knot A. Also I was a bit surprised by the assertion that observations of 3C31 could not detect a filament like that seen in NGC315. Where does that come from? Fig. 5 of our first (2002) 3C31 paper actually shows data at higher linear resolution than we are showing here for NGC315, and also shows a complex of knots in the bright region of the jet, some clearly off-axis. So I'm not sure why this comment got in. The main difference with 3C31 and 3C296 s that they clearly show the "arcs" and other non-axisymmetric further out before they deflect through large angles, while NGC315's jet has a relatively smooth brightness distribution and stays straight much longer.

So there's some stuff leading into the filament mechanism discussion for NGC315 that strikes me as a little odd.

Also there's some dichotomy between what's said in places about the filament getting close to the edge of the jet (5.1 para 4) and it being at 60% of the "radius" of the jet (5.1 para 3).

I think we may need to standardize our own picture of what we want to emphasize here before trying to redo Sections 5.1 and 5,2 (which I am inclined to try to combine into a single, briefer, discussion) that deals with

(a) synchrotron instability
(b) K-H instability

(c) advection down the jet of a bright feature that rotates
either (1) with the jet (excluded by Doppler) or (2) with a
moving source within a stationary jet (needs to be something
in an orbit close to the BH which conveniently turns on
and off on the right time scale.

My gut feeling is that the idea that some helical instability
mode has been forced from a rotating source, so the KH
growth rates aren't determining the whole picture seems
a tad more likely than tying this all back to some specific
orbiting "hot spot" close to the BH.

What do you think?

A.
Alan

Subject: Re: Projections
From: Diana Worrall <D.Worrall@bristol.ac.uk>
Date: Wed, 9 Aug 2006 15:22:33 +0100 (BST)
To: D.Worrall@bristol.ac.uk, rlaing@eso.org
CC: Mark.Birkinshaw@bristol.ac.uk, abridle@nrao.edu, bcotton@nrao.edu

Hi Robert,

> Suppose we had a cylindrical jet with a spiral filament of constant pitch angle
> wrapped around it and moving with the flow. Then the moving filamentary pattern
> appears rotated by the aberration angle, so the appropriate angle to the line of
> sight to use for a projection calculation is theta', where
>
> sin theta' = D sin theta
>
> theta is the angle to the line of sight in the observed frame (38 deg for
> NGC 315, we assert) and D is the Doppler factor. If beta = 0.8, as would seem
> reasonable for a filament about half way out in the jet, then D = 1.63 and
> theta' = 88 deg. This is interesting, because the spiral pattern would appear as
> if it was nearly side-on. But this means that we would not see the asymmetry in
> the pattern across the ridge-line (i.e. no cusps corresponding to knot E).

There is something about 90 deg in the rest frame being special that I just don't follow.

The intensity goes not as "sin theta", but as "sin theta dtheta" coupled with time dilation and K corrections, i.e., the origin of the intensity proportional to delta to some power (2+alpha, 3 or 3+alpha) depending on the situation. In the rest frame the emission is isotropic -- why does it matter which specific angles are thrust forward into the observer's frame? -- it's always some of them. I don't see what makes 90 deg special.

The way I look at it, the only thing that would distort the observed emission from a fixed filament in the flow is the extent to which the value of delta is different for different parts of the flow (because the cone is opening up and the jet slowing down). Theta will be slightly smaller on the near side than the far side, but we are talking about rather small differences for beta=0.8.

> if it was nearly side-on. But this means that we would not see the
> asymmetry in

> the pattern across the ridge-line (i.e. no cusps corresponding to
> knot E).

Surely at the cusp the change in delta is negligible, and so the intensity should stay fairly uniform.

What is it in your argument that I'm not taking into account?

Cheers
Diana

Subject: Re: Projections
From: Diana Worrall <D.Worrall@bristol.ac.uk>
Date: Tue, 8 Aug 2006 18:30:55 +0100 (BST)
To: rlaing@eso.org
CC: Mark.Birkinshaw@bristol.ac.uk, abridle@nrao.edu

bcotton@nrao.edu, dmw

> 2. Check radio spectral-index image over DS9 box. I can do this if you tell me
> the coordinates of the box in some system that I can translate.
>
> 3. Check effect of changing beam on 73 mJy flux density. Likewise, I can do this
> given coordinates.

00:57:48.303,+30:21:14.96,3.48046",12.9783",314.901
(centre, lengths in arcsec, PA)

It shouldn't change any arguments, but would be more correct to use matched regions in the X-ray and radio.

> 4. Profile of X-ray/radio ratio (integrated over boxes of some suitable
> size). Need to subtract galaxy contribution in X-rays.

Rather than a profile, if we really want to do something on this I think
you would need to convolve the radio data down to the resolution of the X-ray data (say 0.6 arcsec FWHM) and measure the radio flux density
in each knot -- sending me the un-deconvolved beam parameters you use.
I could then detemine a background-subtracted X-ray count rate for each knot, where the background is measured to subtract out the galaxy. This would give us a radio to X-ray ratio for each knot (plus the diffuse jet emission). Since the radio is higher resolution, it needs to define the regions used for the measurements.

We aren't making use of this information, so I'm not sure if it's really worth doing it.

> 5. Gaussian fits to knots A and B (not quite sure why). Tried this 2 ways, on
> the "filament only" image with no baseline and on the original image with
> a single Gaussian + flat baseline. Latter gave more reasonable answers, which
> are:
>
> Peak Integrated Ang siz (deconvolved)

	(mJy)	(mJy)	(arcsec)
A	0.40	0.43	0.16 x <0.12
B	0.49	0.64	0.33 x <0.11

B isn't very Gaussian, though.

I think I was interested in the pressure comparison here, since these are knots with clearer X-ray counterparts than knot E, that was all. Probably not so important, but thanks for the numbers anyway.

6. DW to do knot $p_{\rm sync}$ and τ. Replot external pressure on larger scale. RL to write words relating to conservation-law analysis. Mention X-ray lifetimes explicitly.

Not quite sure what was meant here. The knot E example is done. The point about the conservation-law analysis might have been that we found the base of the flaring region to be significantly overpressured for 3C31 (via an argument independent of the synchrotron minimum pressure). But the minimum pressure for E isn't that much above external anyway. Is this relevant here?

Probably should discuss the flaring-point pressure comparison in the next paper, where we also have the X-ray pressure fit out to larger radii.
We already did some model fitting to the fall-off to the beta model at large radii. At some point we need to stick it on to the XMM pressure analysis that Judith is doing. The temperature structure might then become a bit of a problem

7. Must be clear about where the filament starts (resolution, sidelobes, etc.).

[No idea what this meant.]

Is there any evidence for the filament in the region before the jet flares --- i.e., what constraints can you place on it?
A humble X-ray astronomer looking at Fig 1 would say that the jet width is very comparable to the beam size before the flaring region, so how could we tell if there was or wasn't some filament closer to the core?
It has always looked to me from fig 1c that the oscillatory structure (with amplitude related to jet width) does extend closer to the core than just the flaring region.

8. Critically assess detailed correspondence between radio and X-ray

emission.

Point 4, but then what do we do with it? I think we'll be hard
pressed
to measure X-ray/radio spatial offsets in the knots. A decline in
X-ray to
radio brightness down the jet is apparent, but then that isn't usual,
and
must be related to a decline in effectiveness of the acceleration
process in knots A-C relative to further out.
The fact that there are diffuse X-rays in the region the jet
is decelerating seems a logical consequence of a lot going on there.

Cheers
Diana

Subject: Re: Projections
From: rlaing@eso.org
Date: Tue, 8 Aug 2006 17:39:09 +0200 (CEST)
To: Diana Worrall <D.Worrall@bristol.ac.uk>
CC: Mark.Birkinshaw@bristol.ac.uk, abridle@nrao.edu, bcotton@nrao.edu

Dear Diana

A few leftovers from checking my old notes, which say:

1. DW to add comment on source at 00 57 38.71, 30 22 46.99 in caption to
   Fig. 5. Cross-reference Laing et al. (2006a). This was to confirm positional
   coincidence with what we think is a background radio source.

2. Check radio spectral-index image over DS9 box. I can do this if you tell me
   the coordinates of the box in some system that I can translate.

3. Check effect of changing beam on 73 mJy flux density. Likewise, I can do this
   given coordinates.

4. Profile of X-ray/radio ratio (integrated over boxes of some suitable
   size). Need to subtract galaxy contribution in X-rays.

5. Gaussian fits to knots A and B (not quite sure why). Tried this 2 ways, on
   the "filament only" image with no baseline and on the original image with
   a single Gaussian + flat baseline. Latter gave more reasonable answers, which
   are:

	Peak (mJy)	Integrated (mJy)	Ang siz (deconvolved) (arcsec)
A	0.40	0.43	0.16 x <0.12
B	0.49	0.64	0.33 x <0.11

B isn't very Gaussian, though.

6. DW to do knot $p_{\rm sync}$ and τ. Replot external pressure on larger
   scale. RL to write words relating to conservation-law analysis. Mention X-ray
   lifetimes explicitly.

Not quite sure what was meant here. The knot E example is done. The point
about the conservation-law analysis might have been that we found the base of
the flaring region to be significantly overpressured for 3C31 (via an
argument independent of the synchrotron minimum pressure). But the minimum
pressure for E isn't that much above external anyway. Is this relevant here?

7. Must be clear about where the filament starts (resolution, sidelobes, etc.).

[No idea what this meant.]

8. Critically assess detailed correspondence between radio and X-ray emission.

Regards

Robert

Subject: Re: Projections
From: rlaing@eso.org
Date: Tue, 8 Aug 2006 14:12:49 +0200 (CEST)
To: Diana Worrall <D.Worrall@bristol.ac.uk>
CC: Mark.Birkinshaw@bristol.ac.uk, abridle@nrao.edu, bcotton@nrao.edu

Returning to the question of whether the filament is moving with the bulk flow
.....

Suppose we had a cylindrical jet with a spiral filament of constant pitch angle
wrapped around it and moving with the flow. Then the moving filamentary pattern
appears rotated by the aberration angle, so the appropriate angle to the line of
sight to use for a projection calculation is theta', where

sin theta' = D sin theta

theta is the angle to the line of sight in the observed frame (38 deg for
NGC 315, we assert) and D is the Doppler factor. If beta = 0.8, as would seem
reasonable for a filament about half way out in the jet, then D = 1.63 and
theta' = 88 deg. This is interesting, because the spiral pattern would appear as
if it was nearly side-on. But this means that we would not see the asymmetry in
the pattern across the ridge-line (i.e. no cusps corresponding to knot E).

Now this calculation is more than somewhat inaccurate for an expanding flow like
NGC 315's (obviously you can't just change the projection angle), but at least
it gives a qualitative insight into what a proper sum would show. My suspicion
is that the pattern must be moving more slowly than the underlying flow if it is
indeed a regular spiral.

It belatedly occurred to me that I have a script designed to compute
trajectories for blobs in SS433 (courtesy of Katherine Blundell) which I could
adapt to look at the problem of a filament wrapped around an exactly conical jet
- not quite what we need but the next level of approximation.

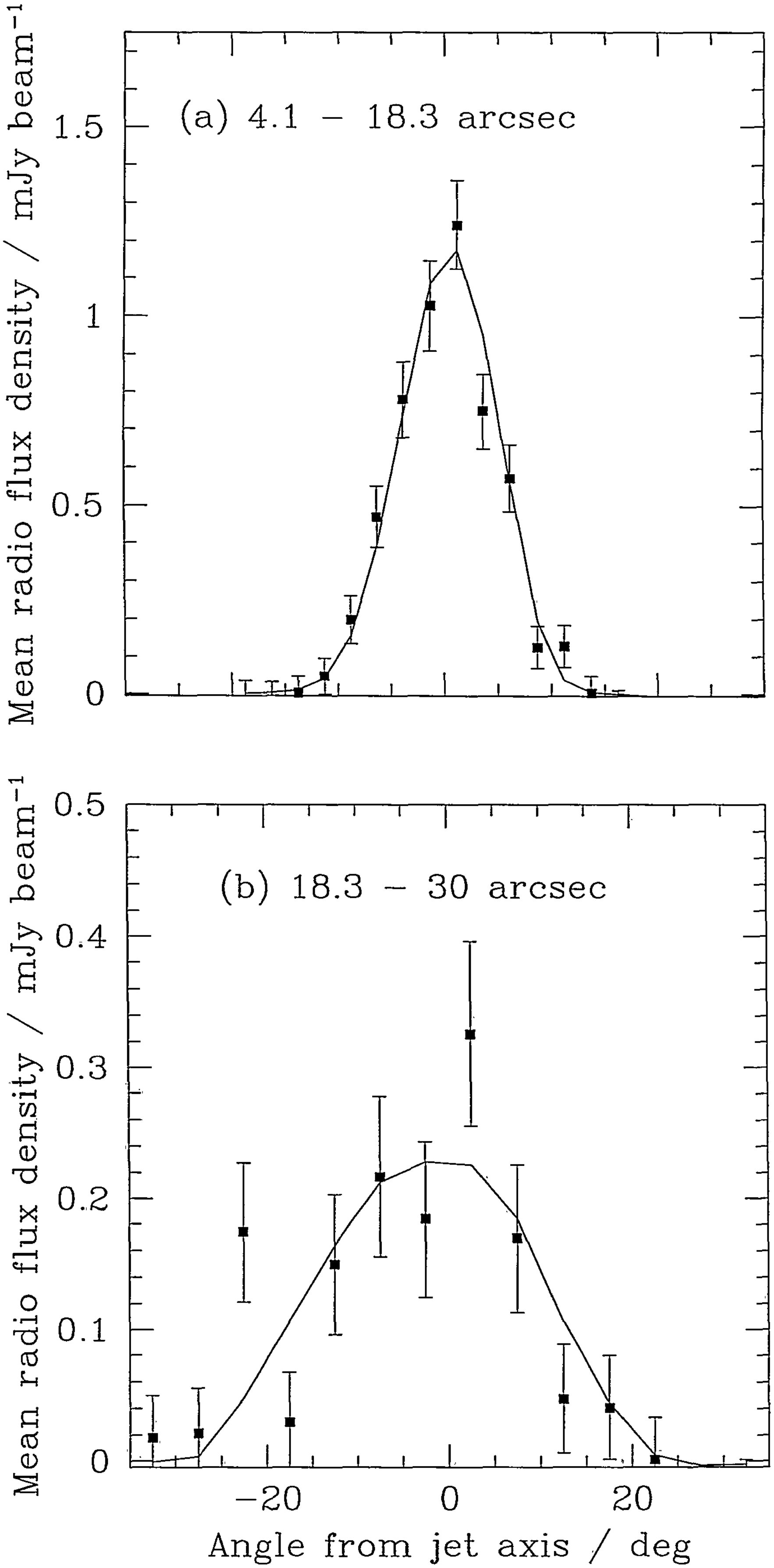
Should I do this? It's less work than I thought (as I can edit an

existing
script) but more complication.

Cheers

Robert

Transverse Profiles



Subject: updates based on RAL comments
From: Diana Worrall <D.Worrall@bristol.ac.uk>
Date: Sat, 5 Aug 2006 21:27:49 +0100 (BST)
To: D.Worrall@bristol.ac.uk, abridle@nrao.edu, bcotton@nrao.edu, mb1@star.bris.ac.uk, rlaing@eso.org

Dear Robert,

Many thanks for the updated versions of figures and for your comments. I've made a revised version of the paper with the revised figs 2, 3, and 12 and addressing your suggestions. It's in the usual place (http://www.star.bris.ac.uk/dmw/n315).

Assume your wording or something close is adopted if there is no comment below.

My comments:

I'm not convinced about the colour version of the radio data. I like fig 1c, and the colour version doesn't look so good in black and white (things like the dashed line around the core are a bit odd, for example). Unless it's all or nothing it makes for a very odd set of figures in the hardcopy. Fig 1 would be the obvious one for colour, if any, but I think we are probably safer with greyscale. I'm sticking with the original of Fig 1, since the new version loses detail of the radio/X-ray comparison.

The correct way of deciding on the reality or not of 'knot X' is from the X-ray counts --- it's easy enough to do, and because of that I haven't done it yet, trying to give priority on getting agreement on the discussion. It it's not significant, all the comments on it will just be removed. I prefer not to swap figs 1 and 2. They will be close together, but as the X-ray observations are not previously discussed, and appear first in the paper, I'd prefer to have the figure showing the X-ray and radio images first.

> Could mention in the captions that radio beams are shown in bottom
> rh corner,
> although it"s obvious.

I think it's obvious. Captions are already long.

> I wonder whether we might dispense with more of the RA labelling of
> knots in
> favour of the letters. For now I have labelled the first X-ray knot
> (X) in panel
> b - easy to remove if you don't like it.

> Last para of this subsection: a case for labels on the 1.5 arcsec
> superposition?
> I haven't done this yet, but it would be easy and might make things

> clearer.

I'd rather have no labelling on Figs 1 and 3. It gets in the way of seeing the data.

> I don't understand why the detection of (presumably scattered) H alpha from the
> nucleus is evidence AGAINST the X-rays being viewed through the torus. Rather
> the reverse, surely, as polarized implies scattered rather than seen directly?

Agreed. I'm not sure the optical spectra tell us anything useful about the central components. I've rephrased.

> In any case, I'd suggest ending the previous para at "... currently unknown."
> and incorporate the rest in the following para after the first sentence, since
> it is directly relevant there.

The following para is entirely the X-ray argument, so I prefer to keep it separate for reasons of flow, but I've revised the connecting words.

> Move last para to following sub-section?

I haven't because it's the result from the core spectrum. Have clarified that.

> I question whether the sentence "Offsets ..." is really appropriate at this
> point. We don't associate X with any radio knot, so can we really say that there
> is an offset? I'd suggest going straight to:

Have changed the word 'offsets' since I think you are reading it as requiring discrete knots. I think the (common) differences in the X-ray/radio profiles (if you'd rather think of it that way) are interesting and worthy of comment. The parag will go if knot X is not real.

> Para 3 Main point here is the shortness of the synchrotron lifetimes so start
> with that rather than the overpressure?

Have broken into separate paragraphs but not re-ordered, for simplicity.

> New para at "In jets for which the optical ..." since this is an entirely
> separate point?

I think it works connected, since it's all related to particle acceleration

> In M87, I thought that the idea was that the apparent field was orthogonal to
> the axis at the leading edge of the OPTICAL knots and longitudinal further
> downstream whilst the radio tended to be closer to longitudinal and come from
> further out in the jet. But we are talking about RADIO polarization and our
> spatial resolution (relative to the width of the filament) is much worse than in
> M87 (relative to the width of the jet).. So are the situations really that
> different? Neither would I want to be dogmatic as to whether the optical knots
> in M87 actually are shocks. But in any case it appeared to me from the recent
> astro-ph Perlman et al. (incl W & B) paper on optical polarimetry of jets that
> the picture suggested previously for M87 was not necessarily general. So I'm not
> sure I follow the logic of this argument.

I think the emphasis is 'strong' shocks. The case in 3C 15 is perhaps better than M 87.

> One other thing that we don't address is whether the filament is actually moving
> with the flow. Clearly the emitting material must be moving, but what about the
> patterm?

Should say something about that in sec 6 when sec 5 is agreed. For K-H (5.1) it would probably move. Wtih the injection model in 5.2 it clearly moves. Mark is thinking about an alternative possibility of an external structure pushing into the jet, possibly creating more of a stationary pattern.

> Owen, Hardee & Cornwell (1989; ApJ 340, 698) talked extensively about filaments
> in the M87 jet (their idea was that most, if not all of the emission came from a
> filamented surface layer). Sort of related to the Lobanov et al. "double helix"
> but not the same. Do we really think that the Lobanov et al. "double helix" is
> real?

No, don't really believe it, but it's more closely related to the K-H instabilities we are discussing.

Subject: Re: New version
From: Bill Cotton <bcotton@nrao.edu>
Date: Tue, 8 Nov 2005 11:52:50 -0500 (EST)
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@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.
>
> Dear Bill
>
> Yes, I agree. In fact, the observed X-ray densities are very different too. The
> only question is whether to put a short reference here or to reserve it for the
> paper on conservation-law analysis (where it will get heavily emphasised).
>
> What do you (and others) think?

If it's going to be covered in detail elsewhere, there's not alot of point in putting anything here.

-Bill

Subject: Re: Suggested figure and astrometric niceties
From: Mark Birkinshaw <Mark.Birkinshaw@bristol.ac.uk>
Date: Thu, 04 Aug 2005 18:34:30 +0100
To: D.Worrall@bristol.ac.uk, rlaing@eso.org
CC: Mark.Birkinshaw@bristol.ac.uk, abridle@nrao.edu, bcotton@nrao.edu

Robert,

I've taken a look at the truncation model, and tried fitting the model that you suggest to the X-ray data. The fit is terrible. A truncation model does work, but with rather different parameters. In fitting the X-ray data it looks like there is a considerable degeneracy between r_t and r_a in the truncation function,

$$f(r) = \begin{cases} 1 & r <= r_t \\ e^{(r_t - r)/r_a} & r >= r_t \end{cases}$$

thus I get adequate and almost identical qualities of fit with

r_t = 40 arcsec, r_a = 240 arcsec
r_t = 60 arcsec, r_a = 160 arcsec
r_t = 80 arcsec, r_a = 80 arcsec

and so on. I can explore a little further, if necessary.

Now, the logic of this model is simply to get a reasonable fit to the gas density based on the X-ray structure, but it is also interesting to look at the implied underlying mass model. For an isothermal gas, the mass is given by a hydrostatic equation. To get a roll-down of the gas density of this type, the mass needs to increase more rapidly at $r > r_t$ than at $r < r_t$ ... that is, there needs to be a shell of dark (or luminous) matter of increased density just outside r_t compared with just inside r_t. and the fall-off of total dark matter density at large radius needs to go as r^{-1} not r^{-2}.

This might be regarded as somewhat implausible. Perhaps a better thought is that the hydrostatic equation ceases to be relevant on scales more than about 1 arcminute (r_t), because the external medium starts to affect the atmosphere ("evaporating" gas into the group-scale medium, which must be there somewhere), or because this gas is freely escaping from the gravitational potential well at a significant fraction of the sound speed.

It would be good to know if the altered values of r_t, r_a above affect the types of solution you get for the jet.

Cheers

Subject: Re: Suggested figure and astrometric niceties
From: Mark Birkinshaw <Mark.Birkinshaw@bristol.ac.uk>
Date: Thu, 04 Aug 2005 22:03:42 +0100
To: D.Worrall@bristol.ac.uk, rlaing@eso.org
CC: Mark.Birkinshaw@bristol.ac.uk, abridle@nrao.edu, bcotton@nrao.edu

Dear Robert,

The issue about the ridgeline plot was something that I suggested to make it clear that the radio and X-ray wobble around together. But the 4-panel figure that you've made is clear enough that I don't think we need to worry about that any more.

The figure about the outer structure doesn't seem convincing to me at the moment, principally because a cursory look at it suggests that the X-ray blob in the bottom left is the X-ray core. Of course it isn't, but the precise relationship of this to the overall X-ray (or overall radio) structure is far from clear. Maybe extending the plot to the south-east, to pick up more of the jet, would make it clearer.

Also, it's almost impossible to see the fainter X-ray emission on this plot. The "bright" point source at 00:57:46.5 certainly stands out,
and
it can be seen that it has no particular relationship to the jet, so
we
can make the point that it's a background object, but the fainter jet structure at 00:57:47.1 and the low-brightness "streamers" of X-rays to the south and east of it don't show up very well.

I don't really know what to make of the southern X-ray structure that seems to run almost E-W across the bottom of the radio contours, or the apparent jet continuation that runs almost S-N at the left side of the picture. There may be a story to tell here, but there aren't a lot of counts to help us to tell it, and the structure isn't too clear on this picture. Maybe more X-ray smoothing is needed?

All the best

Mark

Subject: Re: Suggested figure and astrometric niceties
From: rlaing@eso.org
Date: Mon, 8 Aug 2005 17:08:21 +0200 (CEST)
To: Mark Birkinshaw <Mark.Birkinshaw@bristol.ac.uk>
CC: D.Worrall@bristol.ac.uk, <abridle@nrao.edu>, <bcotton@nrao.edu>

On Thu, 4 Aug 2005, Mark Birkinshaw wrote:

> Robert,
>
> I've taken a look at the truncation model, and tried fitting the model that you suggest to the X-ray data. The fit is terrible. A truncation model does work, but with rather different parameters. In fitting the X-ray data it looks like there is a considerable degeneracy between r_t and r_a in the truncation function,
>
> ```
>          { 1                     r <= r_t
>   f(r) = {
>          { e^{(r_t - r)/r_a}     r >= r_t
> ```
>
> thus I get adequate and almost identical qualities of fit with
>
> ```
>    r_t = 40 arcsec, r_a = 240 arcsec
>    r_t = 60 arcsec, r_a = 160 arcsec
>    r_t = 80 arcsec, r_a =  80 arcsec
> ```
>
> and so on. I can explore a little further, if necessary.

Dear Mark

This isn't really going in the right direction, unfortunately and the fits are not very satisfactory with these numbers. The problem is generically similar to the one I had with your original functional form: the conservation-law approach would like the pressure to fall more steeply than in the basic beta model from around 50 arcsec and then level out a bit around 100 arcsec (hence the parameters I suggested for the exponential). Your original suggestion for the functional form was actually better except for the extremely rapid (and, I submit, unphysical) drop to zero at (r_a^2 + r_t^2)^{1/2}.

....

> This might be regarded as somewhat implausible. Perhaps a better thought is that the hydrostatic equation ceases to be relevant on scales

> more
> than about 1 arcminute (r_t), because the external medium starts to
> affect the atmosphere ("evaporating" gas into the group-scale
> medium,
> which must be there somewhere), or because this gas is freely
> escaping
> from the gravitational potential well at a significant fraction of
> the
> sound speed.

Indeed, something like this must be happening. Should we be taking the "group"
gas as something like a constant background density on the scales of interest?
That is effectively what we did for 3C31, as the core radii of the group and
galaxy were very different. If so, what are appropriate densities and
temperatures? Would a "group" component have got removed by the background
subtraction procedure?

Cheers

Robert

Subject: Re: NGC315 cons law
From: Diana Worrall <D.Worrall@bristol.ac.uk>
Date: Mon, 15 Aug 2005 19:48:00 +0100
To: D.Worrall@bristol.ac.uk, Mark.Birkinshaw@bristol.ac.uk, abridle@nrao.edu, bcotton@nrao.edu, jcanvin@physics.usyd.edu.au, rlaing@eso.org

Dear Robert,

I've been looking a little at the issue of group gas. I've made a figure
that illustrates the problem

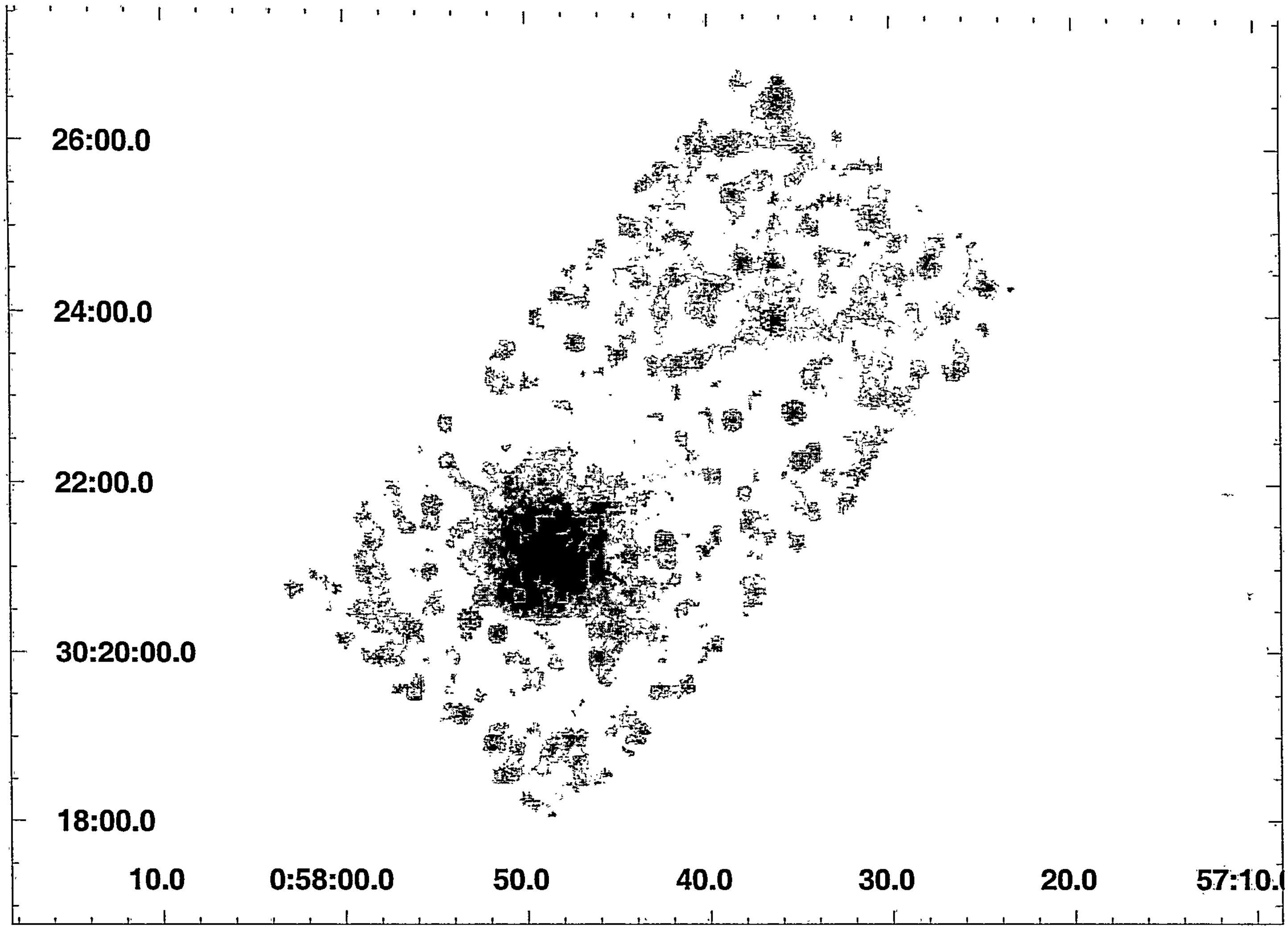
At http://www.star.bris.ac.uk/dmw/n315/
(n315, chandra)

see
bin3-normcts-0.3-7-top15.ps

This shows the whole of the exposed part of the chip containing NGC 315.
The data are in 1.476 arcsec x 1.476 arcsec pixels, and
have been adaptively smoothed with a top-hat filter and a minimum
of 15 counts per smoothing kernel.

The data are corrected for exposure (including vignetting) but no
background has been subtracted. I've selected the 0.3 - 7 keV energy band
to avoid the worst of the particle background. There is no obvious gradient
beyond the galaxy atmosphere, and so separating any group component
from the real background (dominated by particle-induced background at
the higher energies, and diffuse X-rays in our Galaxy at the lower energies)
is not really possible in this case. The group gas
must be faint (it's easily seen as a gradient of emission
in data for some other sources), but putting
a level on it here is highly uncertain. The best way normally is from the
data themselves. Using a different CCD adds systematic errors, although
I will take a look at that too.

Best wishes
-Diana



26:00.0
24:00.0
22:00.0
30:20:00.0
18:00.0
10.0
0:58:00.0
50.0
40.0
30.0
20.0

Subject: Re: Suggested figure and astrometric niceties
From: Mark Birkinshaw <Mark.Birkinshaw@bristol.ac.uk>
Date: Thu, 11 Aug 2005 16:38:29 +0100
To: D.Worrall@bristol.ac.uk, rlaing@eso.org
CC: Mark.Birkinshaw@bristol.ac.uk, abridle@nrao.edu, bcotton@nrao.edu

Robert

Thanks for the new pictures. The two large-scale images show better the overall relationship between the X-rays and the radio jet, I think. The peculiarity of the structure just outside the four-panel figure becomes more apparent, and the bright source at 00:57:46.6 is also quite clear enough. The appropriate one of these to choose will be the one that looks best when printed ... I'm not sure what that'll be, but the first has fewer black/white transitions in the radio contours.

The polarization plot looks good, with the twisted apparent field directions in the brighter part of the jet showing up well.

Cheers

Mark

Subject: Re: Suggested figure and astrometric niceties
From: rlaing@eso.org
Date: Thu, 11 Aug 2005 15:49:17 +0200 (CEST)
To: Diana Worrall <D.Worrall@bristol.ac.uk>
CC: Mark.Birkinshaw@bristol.ac.uk, <abridle@nrao.edu>, <bcotton@nrao.edu>

Dear Diana et al.

Here are two new overlays of a wider field (OVERLAY0.5-8.BIG2.PS, OVERLAY0.5-8.BIG.PS) including the inner and outer parts of the jet. They
differ only in the grey-scale range. They are designed to address Mark's points
by:

- showing the whole field to avoid confusion about the location of the core
- indicating the area of the 4-panel figure with a box
- compressing the grey-scale range
- overlaying fewer radio contours

In addition, I have made a new grey scale of 0.4 arcsec I and vectors with
magnitudes proportional to p and direction of apparent B field. This is
I0.4.BVEC.PS. It covers the same area as the individual panels in the mosaic.

Cheers

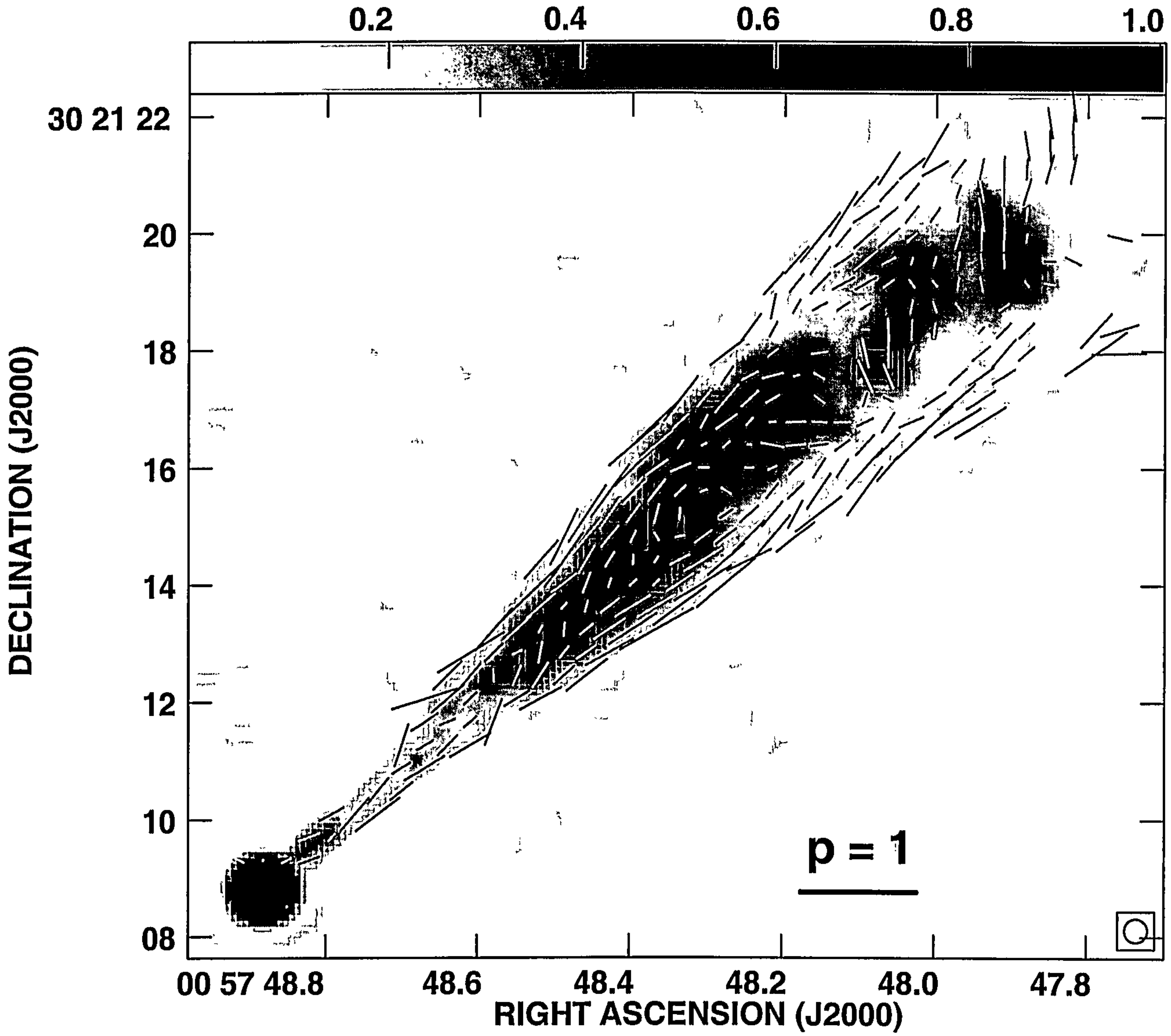
Robert

--
Robert Laing
European ALMA Instrument Scientist

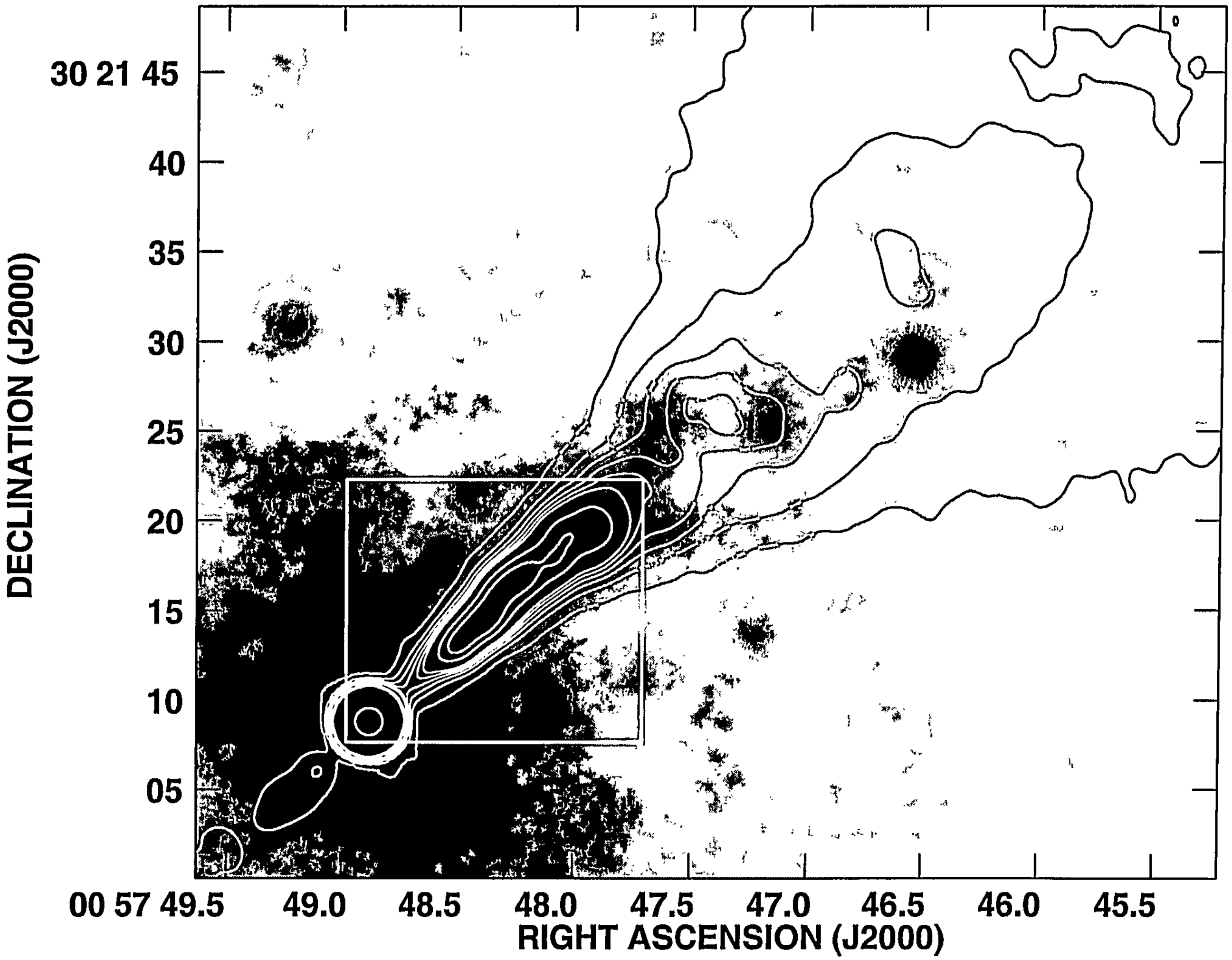
European Southern Observatory
Karl-Schwarzschild-Strasse 2
D-85748 Garching-bei-Muenchen
Germany

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

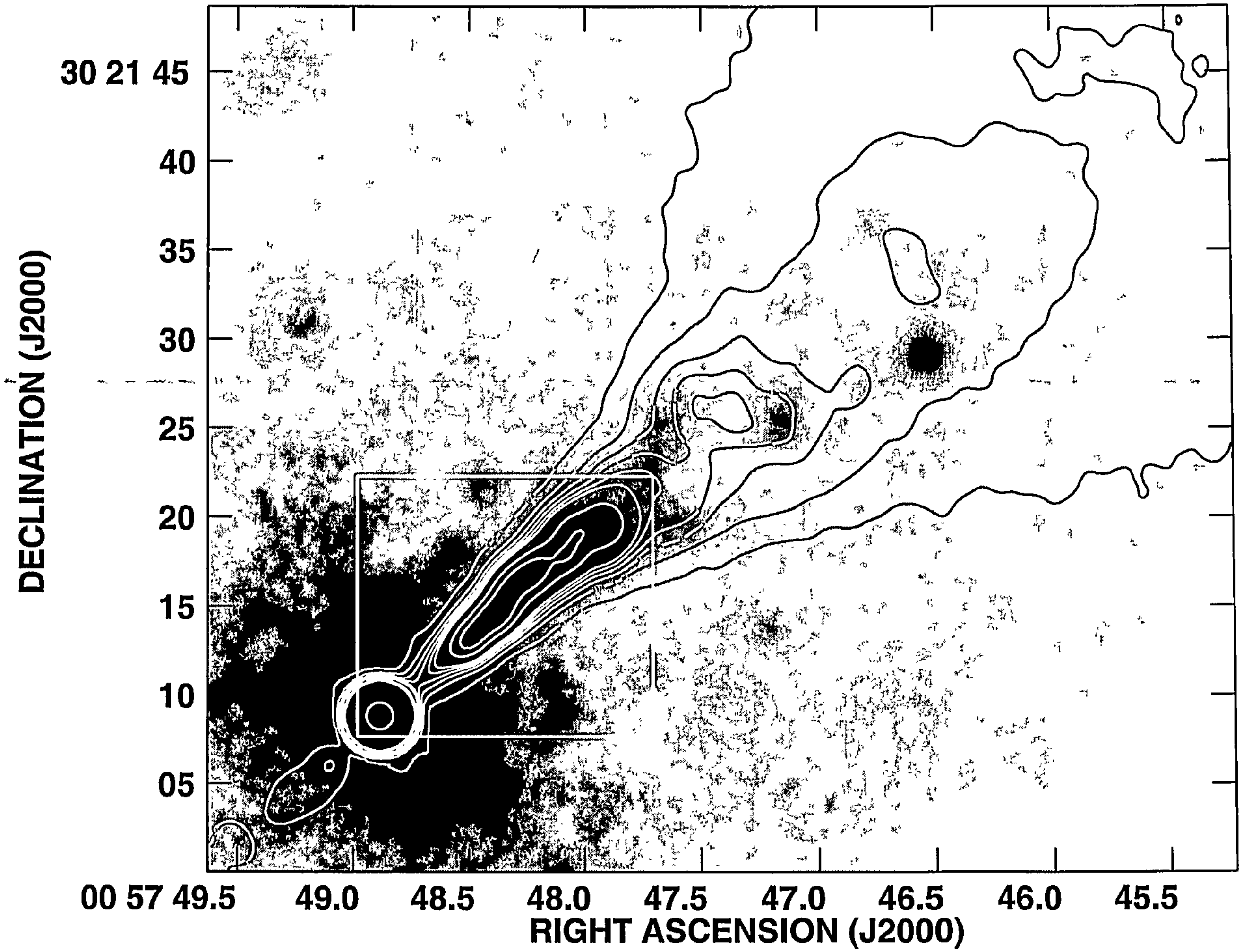
rlaing@eso.org



0.2
0.4
0.6
0.8
1.0
30 21 22
20
18
16
14
12
10
08
DECLINATION (J2000)
00 57 48.8
48.6
48.4
48.2
48.0
47.8
RIGHT ASCENSION (J2000)
p = 1



30 21 45
40
35
30
25
20
15
10
05
DECLINATION (J2000)
00 57 49.5
49.0
48.5
48.0
47.5
47.0
46.5
46.0
45.5
RIGHT ASCENSION (J2000)



30 21 45
40
35
30
25
20
15
10
05
DECLINATION (J2000)
00 57 49.5
49.0
48.5
48.0
47.5
47.0
46.5
46.0
45.5
RIGHT ASCENSION (J2000)