From: Robert Laing <rl@ast.cam.ac.uk>
To: Alan Bridle <abridle@NRAO.EDU>
Subject: Things
Date: Mon, 6 Oct 1997 19:29:06 +0100 (BST)

Dear Alan

Just to reassure you that I haven't forgotten about 3C31 and to ask for some advice about aips on pc's.

I recently went to the Ringberg workshop on M87 and had the chance to talk to a number of theorists. They were all of the opinion that we should look at the ability of "adiabatic" models to fit the emissivity and (perhaps also) field structure distributions given the velocity field. It turns out that the machinery to do this incorporating shear in the non-relativistic case is in Matthews & Scheuer (1990) MNRAS 242, 616 (they were interested in the case of a dynamically unimportant magnetic field in a numerical simulation). I am currently trying to get my head round the modifications needed to incorporate relativity and our geometry. It may be that the shear is so great that the assumption that the field is dynamically unimportant is violated, in which case reconnection presumably takes over, but this isn't obvious.

A much easier intermediate stage is to assume that the particles behave adiabatically and to calculate maps of the field strength and particle density separately on this assumption - I shall do this first. It also incorporates conservation of relativistic particles, which gives people a warm feeling of confidence.

The final thing I propose to do is to integrate the momentum flux across the jet and so estimate the rate of entrainment (it probably isn't a good idea to worry about transverse variations since we don't know how stresses are transmitted).

Does this seem reasonable to you?

I am thinking of buying myself a PC to replace the wheezing RGO machine I have at home. One obvious application is to run AIPS under Linux, and I vaguely recall that you were acting as a guinea-pig for something along these lines. Any recommendations? The configuration I have in mind at the moment is:

200 MHz Pentium or AD6
32 or 64Mb of memory
6.4Gb disk
17in monitor
Some reasonably fast graphics card

Since I'll have the machine at home, the question of data transfer is a bit of a nuisance. DATs are expensive (and I'm not sure about driving them from AIPS - do they need a SCSI card?) Unfortunately, we don't have a cheaper alternative (e.g. Jaz drive) at RGO at the moment.

Any advice appreciated.

Regards

Robert

From: Robert Laing <rl@ast.cam.ac.uk>
To: Alan Bridle <abridle@NRAO.EDU>
Subject: Re: Pictures – first sketch
Date: Wed, 14 May 1997 20:46:24 +0100 (BST)

```
On Wed, 14 May 1997, Alan Bridle wrote:

> Robert Laing writes:
>  > OK.  That one is a proper scale drawing.  Does it need further annotation?
>
> I don't think so
>

Fine.

>  > Would a couple of representative streamlines be helpful?
>
> No, I think we want to keep it simple, the streamlines aren't that subtle
> anyway?
>

No; they are fairly obvious once you have seen the boundary.

>  > What did you
>  > have in mind for the profiles?  One, or >1 example?  And plotted against
>  > distance from the jet axis, or angle subtended at the nucleus?
>
> One versus angle to show the functional form, with the spine-layer
> boundary marked so it's visually clear where the break occurs.
>

That's easy.

>  >
>
>  >  - the radial field in the boundary layer is somewhat more than a detail,
>  >    although the functional dependence is certainly ad hoc;
>
> But I think it's useful to talk about the fit without it (noting where
> it predicts excess polarization and the physical significance of that,
> then put it in as an ad hoc step.  I think it's a case where taking the
> reader through the history of our own thinking on this can be useful.
>

I see what you mean .... I am trying to draft the description of the model
as

  (1) basic description of the model
  (2) model parameters and confidence limits
  (3) an account of why certain features in the model are required by
      the data, irrespective of the detailed functional form.
The third part has a section which goes along the lines you suggest, and
points out that the radial field component at the edge is required in
order to keep the edge polarization down without messing up the
counter-jet (and that this is quite hard to do).

So the explanation will be there, but after the tables summarising model
parameters.  I guess we could invert the order, and have the descriptive
bit first?  I'll try and post off a version of this before I leave,
although I plan to concentrate on jet things in Bologna too, of course.
```

```
>
>
> You betcha.  Will be a good test of how long it takes us to get another
> model somewhere near convergence.  Rick was asking me if the code was
> ready for him to just pop 3C449 in and get the answer out and went
> kind of pale (electronically that is) when I told him how we'd sweated
> over getting 3C31 started....it will be very interesting to see how much
> adjusting your code could actually achieve on its own!
>
```

Well if IBM can beat Kasparov ...... I guess we should have a push-button jet modelling program in AIPS++ by the time PPARC decides what to do with the RGO. Well after both of us are in our wheelchairs at the present rate of progress.

Robert

From: Robert Laing <rl@ast.cam.ac.uk>
To: Alan Bridle <abridle@NRAO.EDU>
Subject: Re: Pictures
Date: Wed, 14 May 1997 12:56:19 +0100 (BST)

```
On Tue, 13 May 1997, Alan Bridle wrote:

> Robert Laing writes:


> I'm not at all sure we need a scale in the diagram itself, we could do
> just as well with a caption saying the images are 10"x 20" and 30" x
> 50" across.  I agree that the tic marks are in awkward places
> sometimes.

OK, I'll do that.

>
> You've done pretty well with the color scheme, in fact you've achieved
> something I rarely can manage which is to get some light blue and definable
> green into the output, how did you do that?

I've no idea!  Just did TVALL + TVPS + TVTRA with a linear transfer
function.  My annoyance was with the printed version, not what was visible
on the screen - I'll try it with a better printer.

>
>  > Is any of this overkill, do you think?
>
> No.  I showing this stuff to people I have come to the conclusion that
> the profiles are a very good idea for showing how good the fit is
> globally.  You can see how well levels and gradients in levels
> are being matched when you look at a profile, and a color scheme
> can be fudged to cover up a lot of stuff that the profile plots
> show is actually very well fitted indeed.  You also see a few of
> the flaws (like the initial degree of polarization on the counterjet
> side) most clearly on the profiles.
>
> I think the answer is to grin and bear it and put it all in, once
> we're past the threshold of a "short" paper we might as well do it
> right I guess.....

I think so too.  Do you prefer 0.75 or 0.25 arcsec versions of the I
profile superposition?  I thought that the latter was slightly more
useful; but P/I and sidedness require the lower resolution and it might be
better to stick with the same beam for all three(?)

>
>  >
>  > That leaves the questions of describing the model (do we need a sketch?)
>
> Yes, it's a great help to people first visualizing what we're doing.
> I've so far only used a 2-d sketch of the regimes and then your
> old picture of a compressed field to get the basic field
> configuration across.  I suggest we still keep those two separate,
> i.e. one picture to identify the regimes, angles, and velocity
> field structure and another to illustrate the basic squashed-field
> concept.

I've got 2 sketches hanging around (one of which I think you have already
```

seen). Elements of these could be combined (provided that the interaction between graphics packages doesn't drive me insane - I am fairly close after the effort required to get some pictures into a DTP system running on a Mac). I'll send the current versions in the following 2 messages. The first is drawn with the correct curves for the boundaries (using pgplot, so the labelling is a bit tricky); the second was made using xfig, so it's easy to label, but the curves are freehand. I think we need some sort of mix: any thoughts?

As for the field structure, I don't know how to do the 3D case! I presume you mean the old squashed-field picture, generated freehand with a pen? I'm not sure my artistic ability is up to anything better, even "helped" by a computer.

>
>
> > and the extent to which we need diagrams to describe the results.
> > Possibilities include:
> > - longitudinal and transverse velocity profiles or
>
> > - vector map of velocity (a little difficult to read, but has more info)
>
> I'm not too worried about the vector part, unless I'm missing something
> from that, but a velocity magnitude image that is quite easy to read
> is useful.

The vector map is quite difficult to read, because the lines are almost all in the same direction, and you cannot get enough dynamic range before they overlap. I guess it is less important if the sketch shows some streamlines. The velocity magnitude image does illustrate the curious dodges necessary at the base, which are difficult to explain in words. What do you think about profiles? (Incidentally, I'd better generate these directly, since AIPS slices through the velocity image have unpleasant ringing effects at abrupt changes of gradient).

>
> >
> > - emissivity image and/or
>
> I think that is well worth it, it hammers the basic point across
> very effectively.
>

OK

> > - profile of log (emissivity) vs log (distance) with "adiabatic" curves
> > superposed.
>
> for the spine only?
>

The point I was hoping to illustrate is that the fall-off of emissivity with distance from the nucleus might be consistent with adiabatic losses dominating in the transition region, but not further out. That requires a comparison of the fall-off for the spine and the centre of the shear layer.

> >
> > Last, but not least, I think we need a montage of the appearance of the
> > model at various angles. I wonder whether the best way to present this
> > might be to autoscale to the core flux density, and so to give an

```
>  > impression of the effects of finite dynamic range.
>
> Depends what we are trying to show.  Do we want to fix the angles and the
> total flux to show how sensitive the model is to the angle, or to do
> fix the model and show how different this very jet would look when viewed
> from different angles?  They both have a role, but I guess we should
> pick one of them in order not to confuse people.
>

The second, I think.  I'd like to demonstrate the qualitative similarity
to other sources; hence

 90 deg for 3C449, PKS1333-33
 60 deg for 3C296, 3C66B, etc.
 30 deg for some fairly bright-cored ones (maybe B2 0206+35, 3C264?)
 as low as we can go for BL Lacs.

I wanted to make the point that you would really only identify the
approaching jet base in BL Lac objects, and that this would be highly
projected => short.  The outer jets get very spread out and muddled with
any lobe emission.  This gets lost if you display with a look-up table
optimised for 3C31, hence my comment about dynamic range.


 >  > Is 30/60/90 degrees
>  > enough?
>  >
>
> If not, how about 20, 40, 60, 80.  90 is a bit dull.
>

OK, but worth it for the comparison with 3C449?

Cheers, Robert
```

From: Robert Laing <rl@ast.cam.ac.uk>
To: Alan Bridle <abridle@NRAO.EDU>
Subject: P.S. – minor cockup
Date: Tue, 13 May 1997 19:50:16 +0100 (BST)

```
You may have noticed that the model images have the y axis labelling
backwards.  The problem was on the x axis, but I seem merely to have moved
it.  CDELT2 ought to have its sign changed - I'll modify the code
sometime, but PUTHEAD is necessary for now.

Robert
```

From: Robert Laing <rl@ast.cam.ac.uk>
To: Alan Bridle <abridle@NRAO.EDU>
Subject: Re: More on dissipation
Date: Wed, 14 May 1997 12:14:24 +0100 (BST)

On Tue, 13 May 1997, Alan Bridle wrote:

>
> I have been wondering if reconnection could play a role in that if
> that is part of how B-parallel is prevented from increasing
> indefinitely.

Yes. There was, in the early 80's, a certain amount of theoretical speculation that jets are like accretion disks, in that the viscosity is described by the mysterious parameter alpha: shear stress = alpha * pressure. This is (allegedly) a good model for magnetic and turbulent viscosity. The characteristic reconnection scale is alpha*v_alfven/(dv/dx), provided that the velocity difference across this length is > v_alfven.

> More generally, I wonder if there could be any
> diagnostics for whether the extra emissivity in the shear layer is all
> in the fields, or in both the fields and the particles. I guess
> the spectra would not help us much as higher-field regions might correspond
> to radiation at lower particle energies and some spectral flattening
> that would mimic replenishment of the high-energy particles.
>
>

The only independent diagnostics I can think of are Inverse Compton X-ray emission (too faint for now) and synchrotron self-absorption turnover frequency (too low a frequency).

Robert

From: Robert Laing <rl@ast.cam.ac.uk>
To: Alan Bridle <abridle@NRAO.EDU>
Subject: Pictures
Date: Tue, 13 May 1997 19:45:22 +0100 (BST)

I am trying to come up with a set of pictures for the 3C31 paper. What I have so far is:

I, 0.75 arcsec, data + model, 0 -> 2.5 mJy, +/-27 arcsec in x to avoid convolution nasties.
P/I, as above but 0 -> 0.7
Sidedness, 27 arcsec, 0 -> 20

I, 0.25 arcsec, data + model, 0 -> 0.8 mJy, +/- 10 arcsec in x.

I have used LTYPE = 8 to get a scale, but the tick marks are inconvenient in places and a plain border might be better.

My current attempts (gzipped) are in the usual ftp area as *.PS.gz.
The models from which they came are *.FITS.gz (these may be the same as the last lot you have - I've lost track).

I'm not very happy with the results (+ am having printer trouble).
Would it be better to use another colour scheme? Do you think that you could have a go at these pictures (or others that you feel might be better)?

The other comparison figures I have in mind are:

- Selected contours + vectors (+/-27 arcsec at 0.75 arcsec; +/-10 arcsec at 0.25 arcsec); data + model
- I profile; data + model superposed; 0.25 arcsec; +/-27 arcsec
- P/I profile; 0.75 arcsec
- sidedness profile; 0.75 arcsec.

Is any of this overkill, do you think?

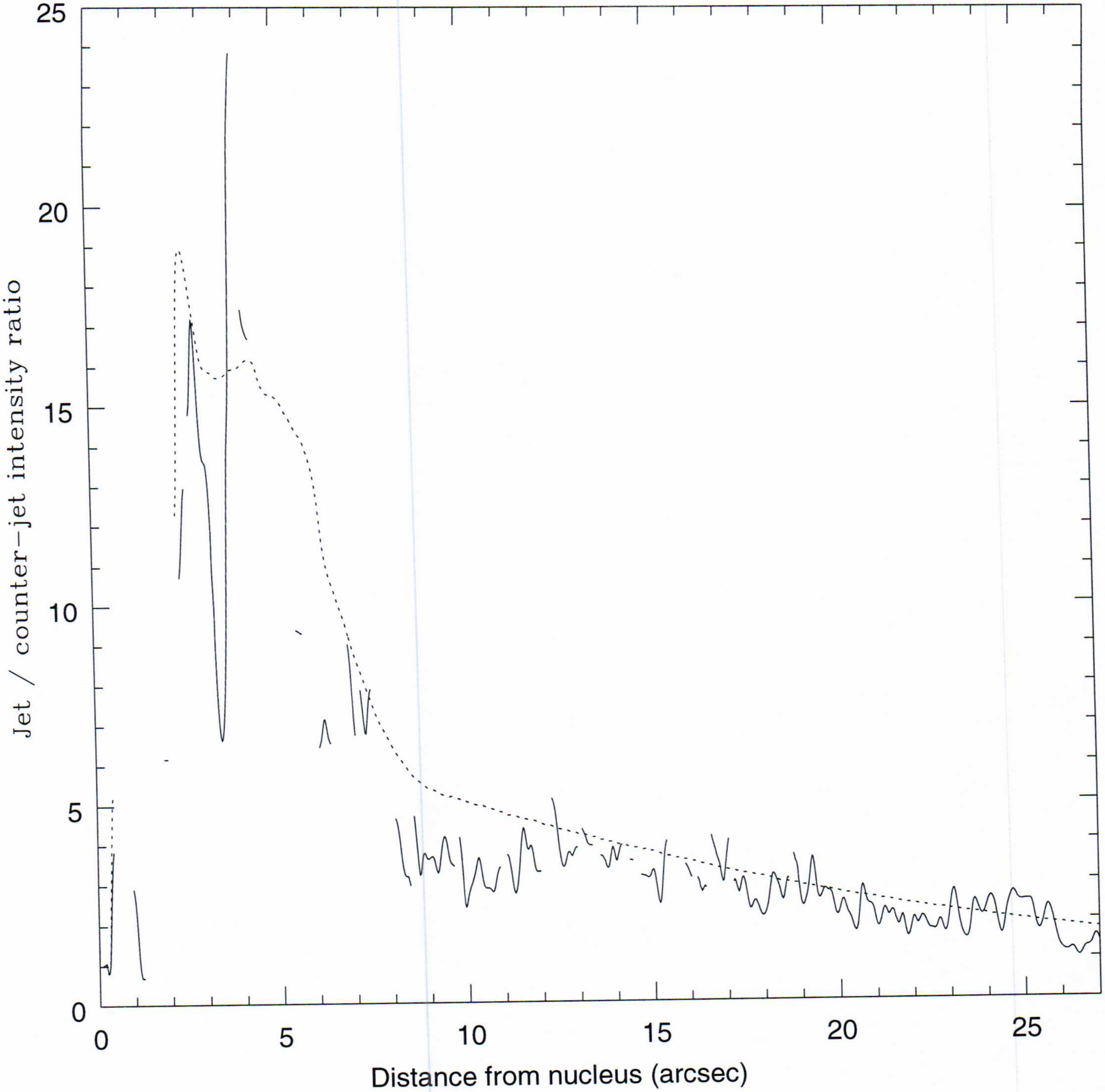
That leaves the questions of describing the model (do we need a sketch?) and the extent to which we need diagrams to describe the results.
Possibilities include:
- longitudinal and transverse velocity profiles or
- vector map of velocity (a little difficult to read, but has more info)

- emissivity image and/or
- profile of log (emissivity) vs log (distance) with "adiabatic" curves superposed.

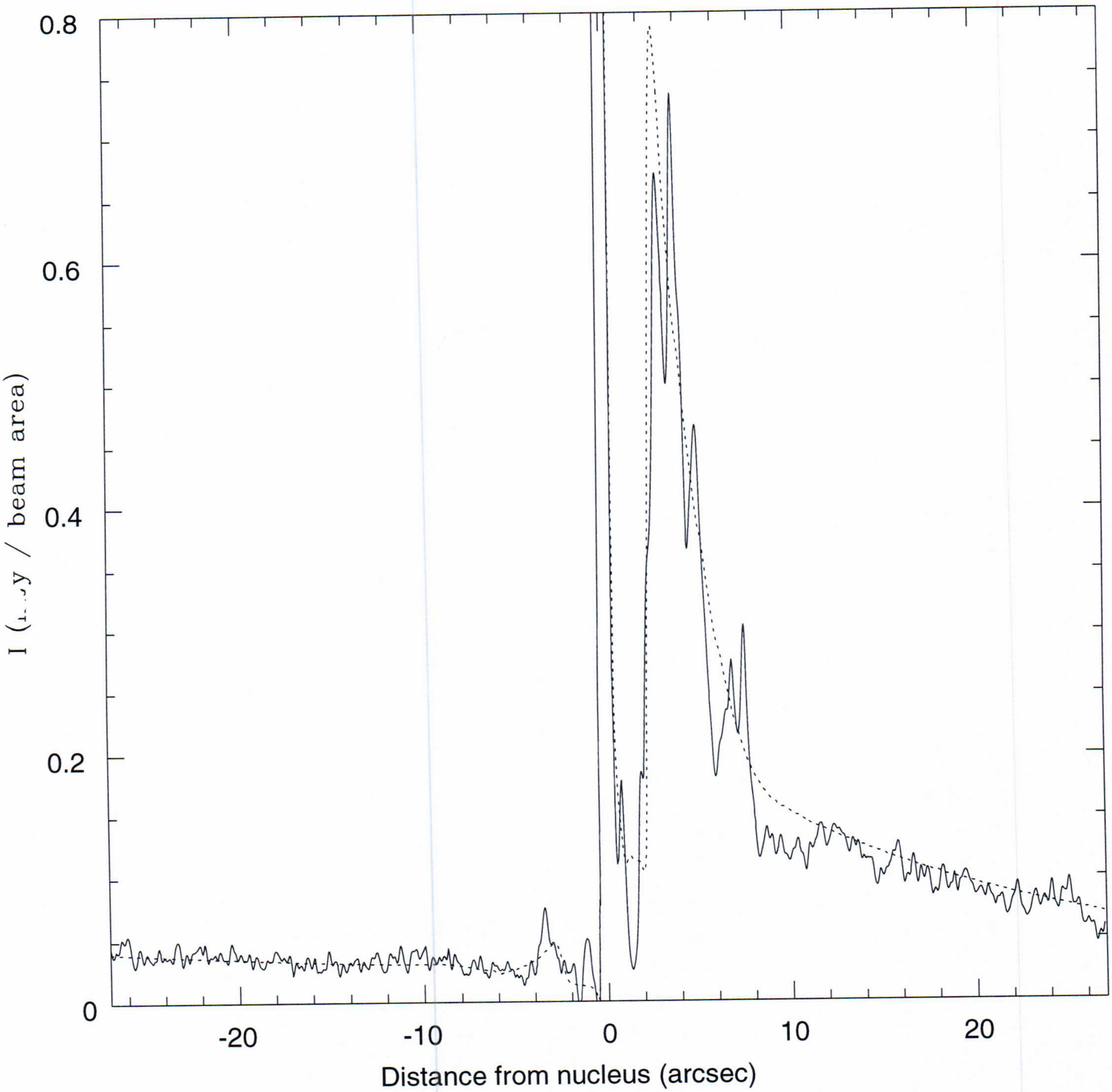
Last, but not least, I think we need a montage of the appearance of the model at various angles. I wonder whether the best way to present this might be to autoscale to the core flux density, and so to give an impression of the effects of finite dynamic range. Is 30/60/90 degrees enough?

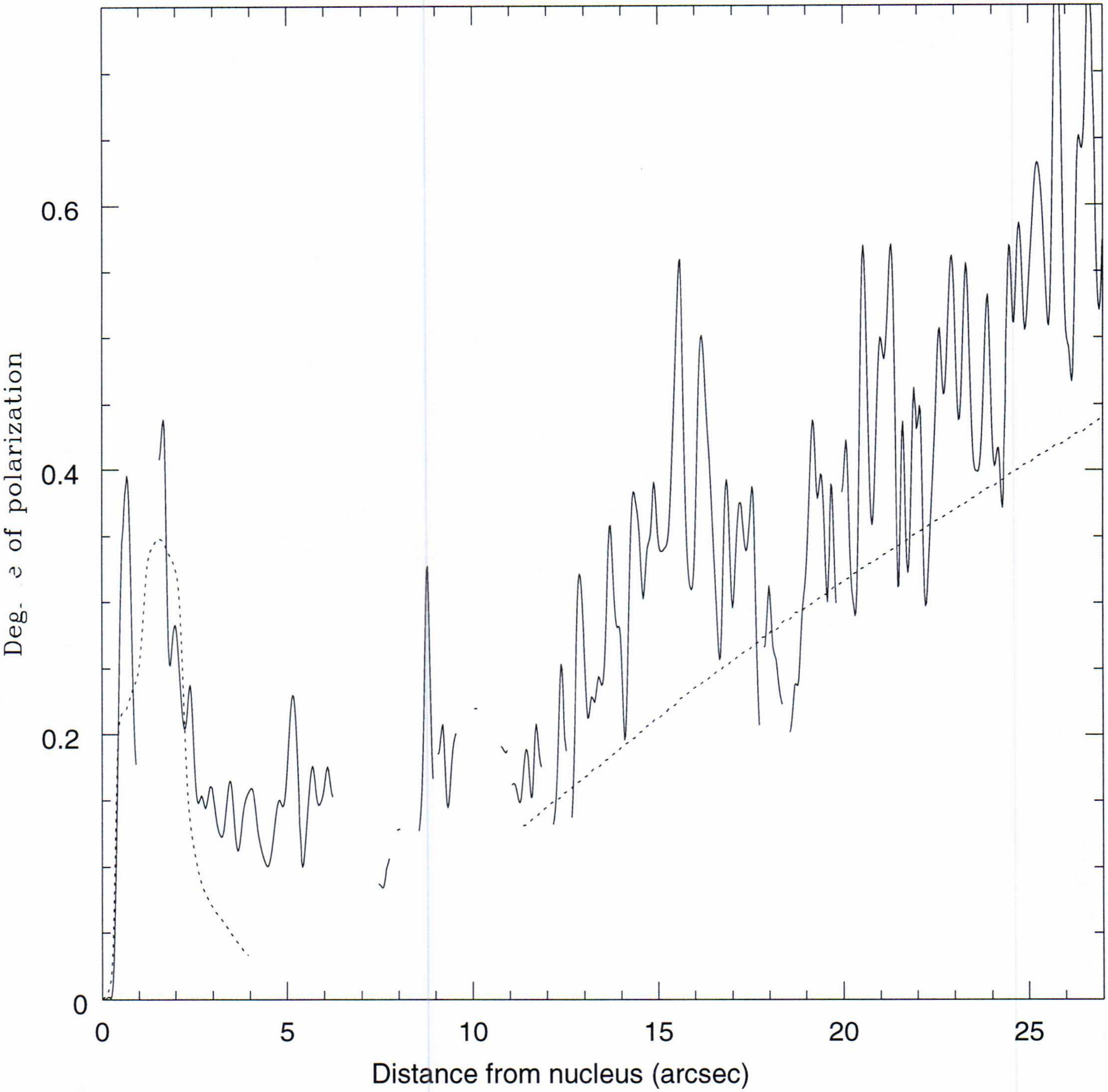
Advice appreciated.

Robert



Jet / counter-jet intensity ratio
Distance from nucleus (arcsec)
0
5
10
15
20
25


0.8
0.6
0.4
0.2
0
-20
-10
0
10
20
Distance from nucleus (arcsec)
beam area)



Deg. e of polarization
0
0.2
0.4
0.6
0
5
10
15
20
25
Distance from nucleus (arcsec)

From: Robert Laing <rl@ast.cam.ac.uk>
To: Alan Bridle <abridle@NRAO.EDU>
Subject: Re: Notation
Date: Mon, 12 May 1997 21:14:48 +0100 (BST)

On Mon, 12 May 1997, Alan Bridle wrote:

>
> I guess we will get horribly confused if we rationalize them
> to anything that also uses I, F, 1 or 0 but is not the same order. i
> and f seem fair enough but maybe we just put them in order and make
> them a,b,c,d?
>
> I think having 1 followed by 0 will confuse the readers, but to invert
> them now will just confuse us forever, so I'd sooner go to something
> that we'll recognize ourselves as the "paper version" and avoid messing
> with our own heads down the road.....
>
> A.
>

Precisely my worry. We already wasted a morning by getting the VMIN's muddled up. I quite like the idea of using A, B, C, D to refer to locations, with the same letter as a suffix on the variable. As you say, i and f are OK, but I can't think of anything very memorable for the other two.

I have referred to the 3 regions as inner, transition and outer: any objections to this stunningly original description?

I spent some time last week going over the references on dissipation and viscosity in jets. It's all very phenomenological. How about the following as a summary:

- we have clear evidence for a velocity gradient - there is substantial shear and we have every reason to suppose that the flow is dissipative, at least in places;
- the "adiabatic" model is a limit in which dissipation is negligible (the magnetic field may end up dynamically dominant);
- alternatively, particles and fields may end up close to equipartition, but this requires dissipation;
- we know that the simplest adiabatic models don't work because the field structure isn't evolving in the right way (not a surprise because of shear);
- nevertheless, it might be that adiabatic losses dominate in the transition region where the emissivity is decreasing rapidly;
- the emissivity fall-off in the outer region is probably slow enough to require some additional energy input;
- viscosity has to be magnetic, turbulent or both;
- the relation between shear stress and energy dissipation is very uncertain: guesses have been made (e.g. proportional, as for accretion disks), but they are just guesses.

Given this uncertainty, what I'd suggest doing is:

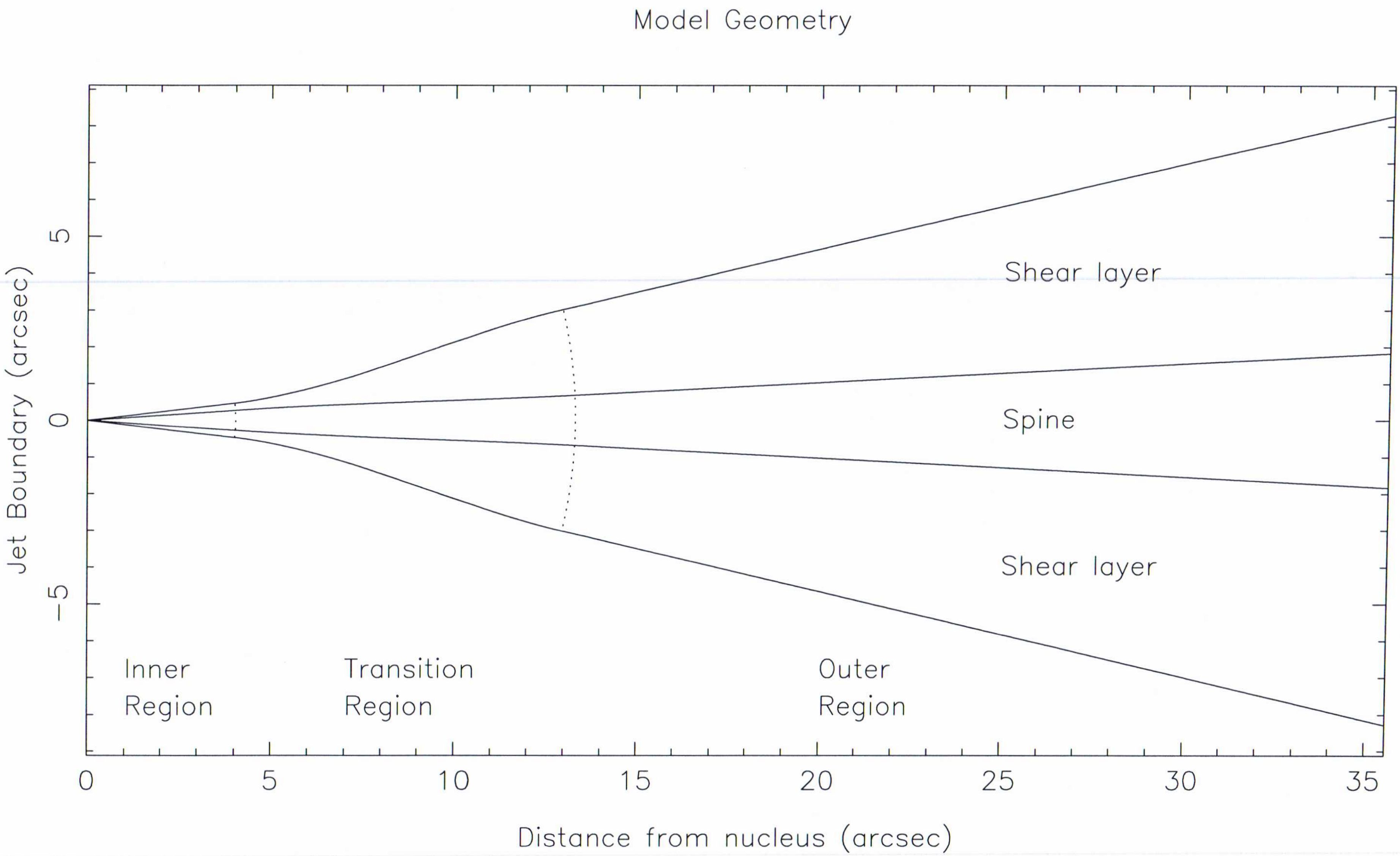
- for the transition and outer regions separately, work out the expected emissivity fall-off given the model central velocity for pure Bperp and Bpar cases;
- compare these with the modelled fall-off.

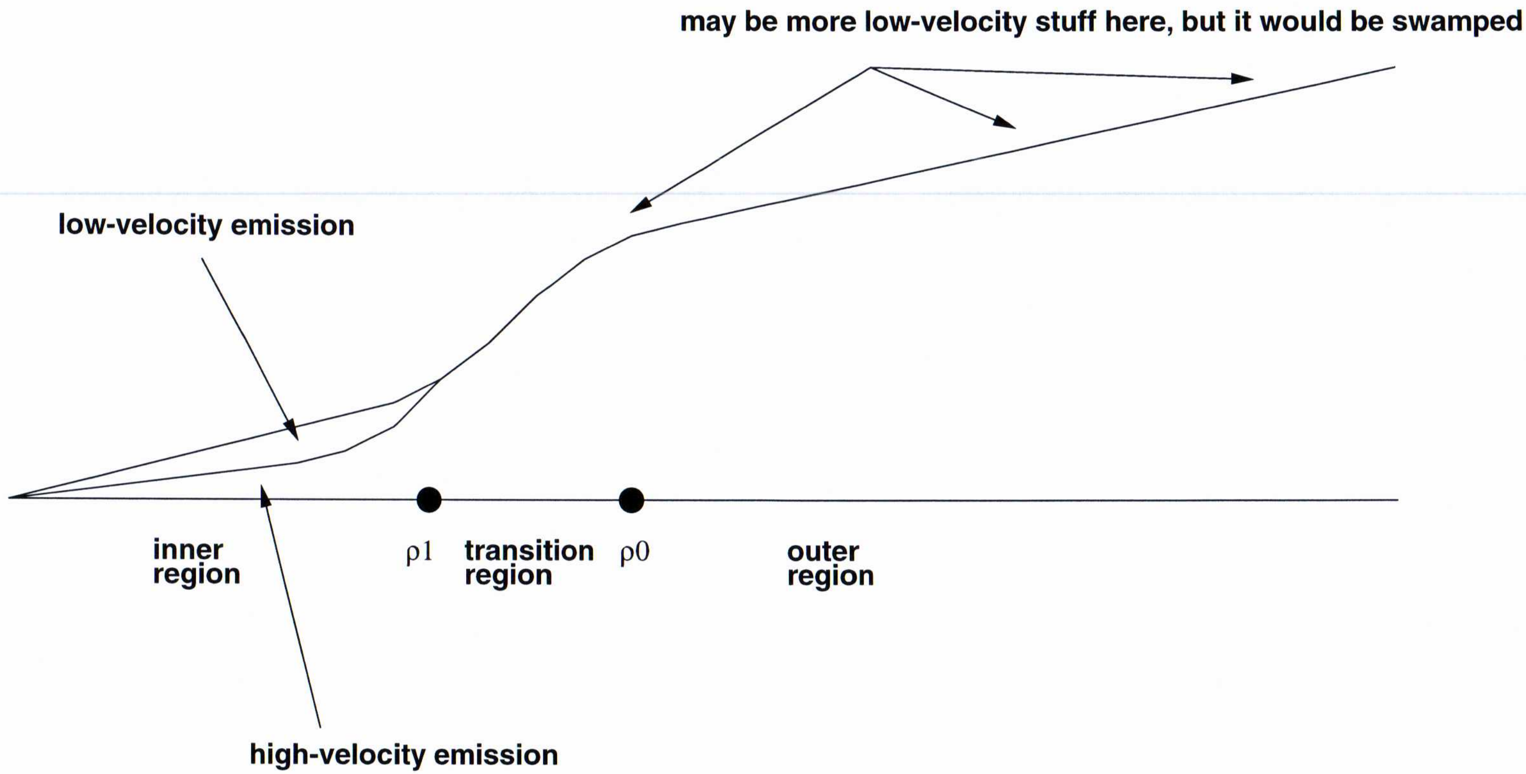
I think we will be able to conclude that dissipation is necessary in the outer region, but not in the transition. Begelman has some interesting speculation on the reasons for this in his IAU97 paper.

Then we can emphasise that any proper model has to fit our velocity profile (+ just possibly quote Baan to the effect that a turbulent jet ought to have a low-velocity wing).

How does this sound to you?

Robert


Model Geometry
Shear layer
Spine
Shear layer
Inner
Region
Transition
Region
Outer
Region
Jet Boundary (arcsec)
Distance from nucleus (arcsec)
5
0
−5
0
5
10
15
20
25
30
35


may be more low-velocity stuff here, but it would be swamped
low-velocity emission
inner region
ρ1
transition region
ρ0
outer region
high-velocity emission

From: Robert Laing <rl@ast.cam.ac.uk>
To: Alan Bridle <abridle@NRAO.EDU>
Subject: Re: Units
Date: Thu, 20 Mar 1997 20:35:53 +0000 (GMT)

> It's actually a bit hard to say, because I was thinking of this in terms
> of changes in the shape of the transverse velocity profile...what does
> the boundary between the high- and low-velocity emission in your
> diagram correspond to? I agree your diagram this is qualitatively what we we
> are saying through the velocity field image, I guess the key really is
> how the resdirtribution of the jet "area" into high and low velocity
> regions as you sketched them comes about...what sort of changes in the
> transverse velocoity profile are involved, and whether it is that or changes
> in the filling-up of that profile with radiating particles. In other
> words, is the "swamping" of the low-velocity stuff in the outer regions
> something to do with geometry, or with Doppler boosting, or with where
> relativistic particles are allowed to get to? I am pretty much ruling
> out the last of these because it seems to me if we have relativistic
> particles all the way to "the edge" to start with, then we would be
> seeing the velocity distribution "illuminated" for synchrotron radiation
> all the way along.

I agree: we should probably proceed on the assumption that radiating
particles fill the jet. I think the new emissivity and velocity images
are useful here.

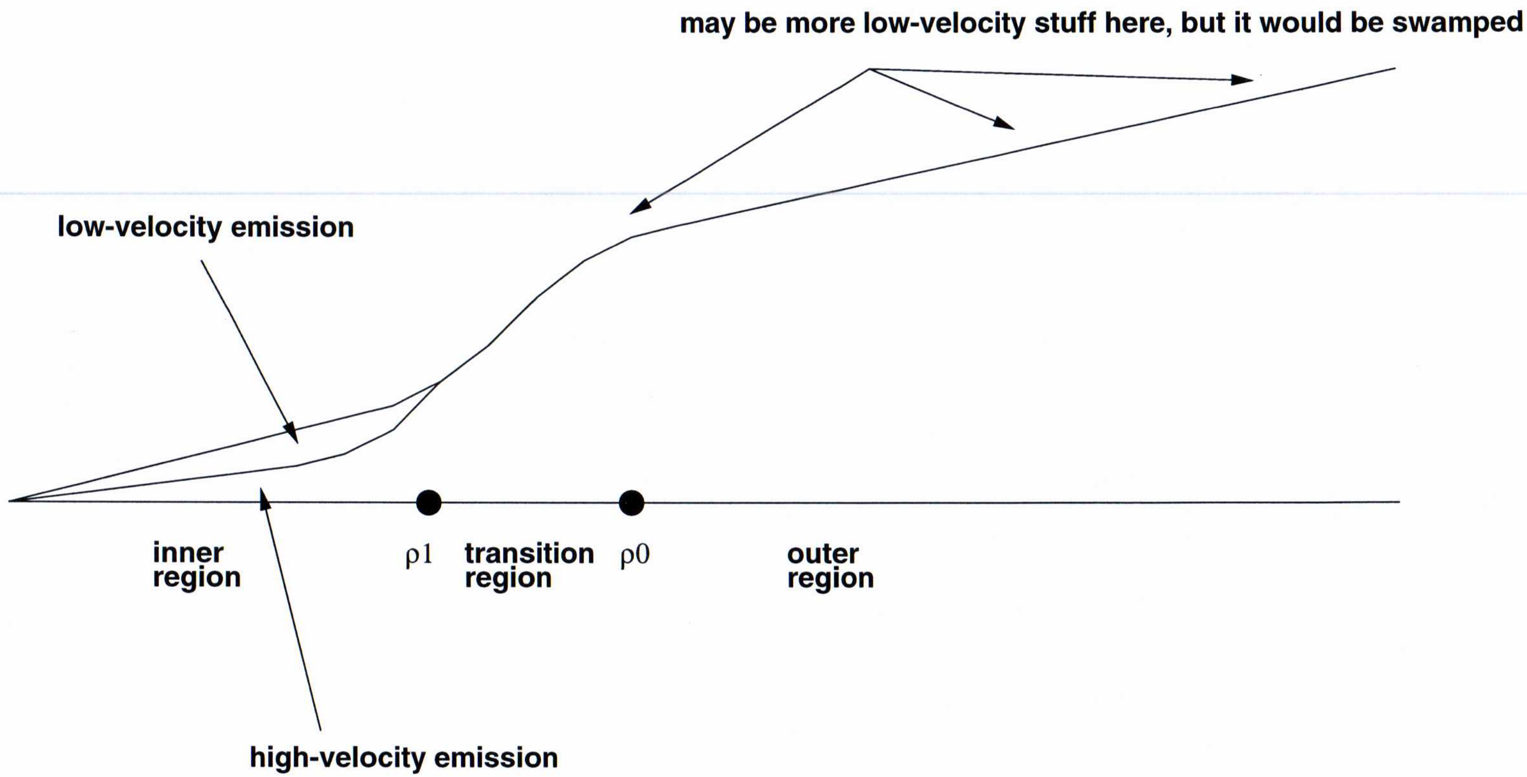
> We had been thinking of the profile modification
> more as an inward diffusion of the low velocities, but this result
> seems to be better described (at least in the transition zone) as an outward
> diffusion of the high velocities! I am putting it this way because the
> model has calculated the boosting effects, so what we should be seeing
> is the underlying velocity distribution, without any reweighting factors
> still to be added from that. So we do seem to need an initial
> "rectangular" sort of velocity profile, but going down to a low value well
> inside the jet, then as the jet propagates and the velocity discontinuity
> "softens" the average velocity along some streamlines near the edge of the
> jet goes actually increase...at the expense of the deceleration in the center.
>
> Does this make sense to you?
>

I see what you mean, but I'd put it a bit differently. The model has
material on continuous, smooth streamlines, and those at the edge of the
jet appear to experience a sudden acceleration. I'm sure this is wrong,
and I think the reason it happens is that the model is contrained not to
have sharp changes. I suspect that this is wrong close to RHO1. I think
that what really happens is that the fast material is on streamlines which
start to diverge rather rapidly close to RHO1. At the same time, a
combination of a real increase in emissivity and a deceleration (=>
removal of Doppler dimming) cause it emission to increase dramatically.
The weak, slow emission we see close to the nucleus may still be there,
but it has too low a flux to make much difference to the modelling. Thus
all of the emission we have modelled for RHO > RHO1 comes from material
which had a high velocity closer to the middle. We would have trouble
seeing any continuation of the low-velocity material unless its emissivity
went up, since there is nothing more to be gained from the Doppler factor.
The model wants an increase in emissivity for material moving outwards
past RHO1, and a decrease in velocity. How about a shock creating a
discontinuity there? Suppose that the high-velocity part of the flow
decollimates and decelerates abruptly at RHO1, and that the new velocity

gradient is created by the shock. Thereafter, we might get more of the appearance of the low velocities diffusing inwards (as indeed appears to be the case).

I think we are on the same track about the velocity profile: my attempt is attached. Curves 1, 2 and 3 are meant to be in the inner, transition and outer regions, respectively, normalized to the extreme edge of the jet. I've drawn a low-velocity tail on all of them, to make it clear that nothing actually accelerates. (Incidentally, there is quite a discontinuity in velocity at RHO0 too, although not much effect on the profile).

Robert



may be more low-velocity stuff here, but it would be swamped
low-velocity emission
inner region
ρ1
transition region
ρ0
outer region
high-velocity emission

From: Robert Laing <rl@ast.cam.ac.uk>
To: Alan Bridle <abridle@NRAO.EDU>
Subject: Re: Units
Date: Thu, 20 Mar 1997 16:20:01 +0000 (GMT)

```
>  > .....  I think that we are probably seeing a faint, low-beta
>  > edge in the innermost jet, the remainder being quite fast (cf. above).  At
>  > RHO1, the assumed geometry is wrong, and we don't have enough resolution
>  > to sort it out.  I suspect that the spine widens suddenly (you have
>  > probably noticed that the jet starts brightening and expanding just before
>  > RHO1), and that all of the emission we see in the transition zone is
>  > actually decelerated stuff from the inner jet spine.  The very low
>  > velocity stuff at the edge is probably swamped for RHO > RHO1.
>  >
>
> This is something I still find a bit odd.  If there is such a range of
> velocities present in the innermost jet, why isn't more of the flow
> beamed so that can see it better?  There may be something being said
> about the changes in the velocity distribution here, as if in the inner
> region the velocity profile is more weighted toward "high" and "low"
> values without much in between, while after RHO1 it is better approximated
> by the linear ramp. At least when weighted by where the radiating particles
> are...

I wonder whether the profile for RHO < RHO1 is basically rectangular, with
just a little low-beta edge, and that the main effect at RHO1 is that the
central (previously uniform) part develops a significant gradient (I need
a sketch here, but you may be able to intuit what I mean).

>
> I wonder if this low-velocity appearance in the inner jet of the FRI's
> has anything to tell us about FRII's.  My first guess was "not"
> because the interactions with the environment and the (fast) particle
> content of the environment is probably different in the two cases, But
> we are basically saying that the appearance of these slender inner
> jets in FRI's is pure "weather", with "climate" determining only the
> sign of the sidedness now not its amount.

Exactly.  I think that the emission from FRI narrow base/FRII jets is
probably dominated by patchy, low-velocity material until you get to
fairly small angles to the line of sight.

>
> I do think the way in which this develops, from the VLBI scale outwards,
> is a genuinely interesting problem for the baseline range between the
> VLA and VLBA!
>

I agree entirely, and a big jump in sensitivity will be needed.

>  > The trouble is that energy is being redistributed between different field
>  > components and between fields and particles, so it isn't a simple
>  > adiabatic expansion.  Either you have no velocity gradient across the jet
>  > or you have to include the effects of shear.  It would be straightforward
>  > to do the case where the velocity and field structure have no radial
>  > variation, but that isn't very interesting.  I'm not all that keen on
>  > working out the effects of shear just yet!
>  >
>
> Okay, I see the sense in which you were saying that now for the shear layer.
```

> We should take a look at how far from adiabatic the spine is, though..
>

Yes, that's straightforward.

> ..... Did you have another look at Baan's paper? I thought it had
> > some useful ideas.
> >
>
> Yes. It puzzles me that we seem to be seeing something more like his
> turbulent velocity profile at RHO<RHO1, and his electron-ion-
> viscosity velocity profile at RHO>RHO1. I would have guessed the
> other way round, which shows how little I presently understand this if
> his profiles are right!
>

I'd be surprised if electron-ion viscosity was a significant effect.
I think that the two main contenders are magnetic and turbulent.

> It has often struck me that Baan's paper was a little far ahead of its
> time, and I was always surprised that he didn't do much more along
> these lines. Maybe he was just waiting 15 years for everyone to catch
> up....
>
>

Probably got discouraged when he found that we didn't know any of the fluid parameters to better than a factor of 1000!

I have done a set of models with the new inner jet parameters (BETAI/VMINI). These look much the same as the old ones for the spine/shear layer cases and more sensible for the Gaussian model. The velocity images are much the same for all 3, and the major differences in emissivity are due to the steeper spine exponent for the full SSL model, which is directly connected to its ability to fit a flat-topped outer jet. So I think we can tell a consistent story.

There are some associated code changes - would you prefer just the affected routines, or a complete package?

Robert

From: Robert Laing <rl@ast.cam.ac.uk>
To: Alan Bridle <abridle@NRAO.EDU>
Subject: Re: Units
Date: Wed, 19 Mar 1997 21:49:57 +0000 (GMT)

```
>
>  > It occurred to me that we could get a slightly neater formulation for the
>  > jet base by using a VMINI parameter set to (approx) zero.  This allows us
>  > to do away with BETAISL, and to use the velocity profile to provide the
>  > low-velocity emission we need, as happens elsewhere.  It avoids the need
>  > to have a low beta for the Gaussian model, where the whole of the jet
>  > appears to accelerate at RHO1.  The 3 models then behave very similarly
>  > for RHO < RHO1, and the only seriously unphysical transition is restricted
>  > to the very edge of the jet.
>
> Okay, I'm presuming that anything that puts the right average beta in
> the shear layer would be consistent as we don't have any transverse
> resolution there, is thst right?
>

Exactly so.

> Are we going to get into the business that M87 also appears to acquire
> higher Lorentz factors as it goes out?  I'd feel happier if this
> depended a little less on exactly how the proper motion data have been
> interpreted there, but one of out "news" items is this inner jump
> thing and I'm not quite sure how much prominence to give it at this stage.
> (Other than to use it as an argument for getting more resolution with
> the VLA extension for these sources!)
>
>

I don't think it is as bad as that (although it could be made to sound so
for a good cause).  I think that we are probably seeing a faint, low-beta
edge in the innermost jet, the remainder being quite fast (cf. above).  At
RHO1, the assumed geometry is wrong, and we don't have enough resolution
to sort it out.  I suspect that the spine widens suddenly (you have
probably noticed that the jet starts brightening and expanding just before
RHO1), and that all of the emission we see in the transition zone is
actually decelerated stuff from the inner jet spine.  The very low
velocity stuff at the edge is probably swamped for RHO > RHO1.


>  > The emissivity image demonstrates very clearly what is making the
>  > difference between the full spine/sl model and the others: we need to cut
>  > the central emissivity in order to keep the jet flat-topped.  This is
>  > perhaps even more obvious further out in the counter-jet: do you know of
>  > any other examples?
>
> Certainly the SSL emissivity image is looks pretty horrible!  In 353 we
> did find that the data were consistent with no emission at all from
> the center half of the jet, though this does not speak to the relative
> effects of Doppler dimming and of emissivity variation in thes case.
> As for other FRI's, that's for us to say.  I think NGC 315 will be
> somewhat different from 31 but I have not been attempting to model it
> yet..
>

The predicted brightness distributions for small theta look a bit odd too.
```

```
>  >
>  > I have been wondering what to say about energetics, adiabatic models etc.
>  > I am inclined to do the following:
>  >  - show the predicted emissivity for a jet of uniform emissivity with a
>  >    velocity equal to the central value in our model and the same outer
>  >    boundary;
>
> By uniform here you mean constant with distance, or across the jet?  The
> latter, I presume, to address the necessity for the "faint spine"
>

Across the jet, indeed.

>
>  >  - say clearly that we do not understand the viscosity mechanism (fields
>  >    or turbulence?) and cannot therefore say how the stresses in the jet
>  >    redistribute energy between particles and fields, and between different
>  >    parts of the jet;
>
> Yes.  It is interesting that we seem to be asking for at least
> approximate (order-of-magnitude) equipartition between the
> longitudinal and toroidal field components in the shear layer.
>

Quite so.

>
>  >  - conclude that adiabatic models are most unlikely in this case.
>  >
>
> I'm finding it a bit awkward to go through and compare with what the
> adiabats actually are, becuase of the different velocity forms.
> It might be interesting to have an option where we specified the
> velocity form only and constrained the emissivity to be adiabatic
> everywhere.  Whether there's aany adiabatuc regime tht even
> approximates what we see would be an interesting question.
>

The trouble is that energy is being redistributed between different field
components and between fields and particles, so it isn't a simple
adiabatic expansion.  Either you have no velocity gradient across the jet
or you have to include the effects of shear.  It would be straightforward
to do the case where the velocity and field structure have no radial
variation, but that isn't very interesting.  I'm not all that keen on
working out the effects of shear just yet!

>
>  > We ought to be able to say something about the entrainment rate by
>  > integrating the particle number and momentum flux over the jet, but we
>  > need at least 3 assumptions:
>  >  - the starting density of thermal matter is known (= 0, probably)
>
> yes, we need to say what the minimum entrainment rate is and this gives it
>
>  >  - the jet is composed of e+/e- or p+/e- plasma;
>
> yes, I have no strong feelings which, though
>
>  >  - the particle and field energies are related in some way
>  >    (otherwise we cannot decouple n and B).
>
```

```
> That's the rub.  Equipartition is one obvious possibility, an adiabat
> would be another. Anything else involves physics we could have no
> hope of specifying (at least not in finite time).
>
>  > For the last assumption, we could either say that equipartition is
>  > maintained or that the particles and fields behave adiabatically (in which
>  > case they cannot be in equipartition everywhere) or something else.
>  >
>
> Interesting question: can the shear layer and the spine be in separate
> equipartition situations with something "driving" the field up in the
> spine (e.g. the velocity shear?).  Presumably the field strength in the
> shear layer limits itself in ways that could accelerate particles there.
>
>
```

It might be that the viscosity mechanisms are different. There are 2 obvious ones: magnetic and turbulent. Which one dominates is an interesting question, and related to the maintenance of velocity gradients. Did you have another look at Baan's paper? I thought it had some useful ideas.

Cheers, Robert

From: Robert Laing <rl@ast.cam.ac.uk>
To: Alan Bridle <abridle@nrao.edu>
Subject: Yet another version
Date: Tue, 18 Mar 1997 17:58:51 +0000 (GMT)

```
Yet another version of the modelling software (v10) is in the usual place.
The changes are minor:
 - there is a new subroutine makerestframe which calculates and outputs
   the velocity and emissivity images (and an associated environment
   variable WRITEVEL);
 - the rotation in the image headers has been changed to 180 degrees to
   get the axis labelling the right way round.

I have optimized the jet base parameters for the Gaussian and simple
spine/shear layer models, so there are now 3 "best buys" (parameters
appended).  I'm in the process of making a standard set of plots for all
three, plus checking that the sensitivity to parameter variations has not
changed significantly.

Robert

* VARS.DAT - input file for jet model (best guess v9 code; full SSL model)
THETA 51.942
JETANG0 16.75
JETANG1  8.511
SPANG0  3.745
SPANG1  4.930
X0 0.2944
X1 0.089
XF 0.8
BETAISP 0.883          initial
BETAISL 0.183          initial
BETA1 0.793
BETA0 0.510
BETAF 0.268
VELINDEX 4.478
VMIN0 0.700
VMIN1 0.695
JUMP1SP 0.281
JUMP1SL 0.057
ESP_IN  2.276
ESP_MID 2.848
ESP_OUT 1.7779
ESL_IN  0.423
ESL_MID 3.193
ESL_OUT 1.482
SPINE_SL 0.783
SLMIN0 0.269
SLMIN1 0.312
SLLTI  1.370           B||/Bφ
SLLT1  1.086           shear
SLLT0  0.766           layer
SLLTF  0.572
SLRTI  0.0   ! Not optimized    Br/Bφ
SLRT1  0.863           shear
SLRT0  0.999           layer
SLRTF  0.015
SPLTI  1.705
SPLT1  0.951
SPLT0  1.247
```

```
SPLTF  0.829
SPRTI  0.000 ! Not optimized
SPRT1  0.000
SPRT0  0.000
SPRTF  0.000

* GAUSS.DAT - input file for jet model (best guess v9 code;
* Gaussian velocity and emissivity profiles)
THETA 50.264
JETANG0 16.75
JETANG1 8.214
X0 0.2944
X1 0.089
XF 0.8
BETAISL 0.423
BETA1 0.816
BETA0 0.504
BETAF 0.269
VELINDEX 3.439
VMIN0 0.608
VMIN1 0.658
JUMP1SL 0.102
ESL_IN  1.307
ESL_MID 3.027
ESL_OUT 1.502
SLMIN0 0.333
SLMIN1 0.408
SLLTI  1.220
SLLT1  1.065
SLLT0  0.832
SLLTF  0.592
SLRTI  0.555
SLRT1  0.638
SLRT0  0.979
SLRTF  0.087

* SIMPLE.DAT - input file for jet model (best guess v9 code;
* Spine and shear layer emissivity and field parameters equal).
THETA 51.100
JETANG0 16.75
JETANG1 8.598
SPANG0 3.265
SPANG1 5.011
X0 0.2944
X1 0.089
XF 0.8
BETAISL 0.407
BETA1 0.833
BETA0 0.501
BETAF 0.267
VELINDEX 4.480
VMIN0 0.670
VMIN1 0.583
JUMP1SL 0.090
ESL_IN  1.326
ESL_MID 3.099
ESL_OUT 1.469
SLMIN0 0.317
SLMIN1 0.380
SLLTI  1.284
SLLT1  1.031
```

```
SLLT0  0.820
SLLTF  0.582
SLRTI  0.500
SLRT1  0.788
SLRT0  1.046
SLRTF  0.036
```

From: Robert Laing <rl@ast.cam.ac.uk>
To: Alan Bridle <abridle@NRAO.EDU>
Subject: Images of velocity and emissivity
Date: Mon, 17 Mar 1997 16:37:36 +0000 (GMT)

I have made images of the magnitude and direction of the velocity vector, and of the rest-frame emissivity. These are in the usual ftp area:

BETA.FITS.gz = velocity, in units of c
ANGLE.FITS.gz = PA of velocity
EMISS.FITS.gz = rest-frame emissivity (un-normalised)

I have sampled these at 0.2 arcsec, which doesn't quite work in the middle, but can easily change this. The maps cover the same area as the observations, but are deprojected, so the sizes are larger. I am not sure that these add a lot to the presentation, but they are useful aids to thought.

I am still somewhat concerned by the "hollow spine', but the data seem to want it ....

Robert

From: Robert Laing <rl@ast.cam.ac.uk>
To: Alan Bridle <abridle@NRAO.EDU>
Subject: Re: More progress
Date: Thu, 13 Mar 1997 19:48:07 +0000 (GMT)

On Thu, 13 Mar 1997, Alan Bridle wrote:

> Hi Robert, yes I think the chi-squared is the good determinant to go
> with in deciding what to emphasize. I have been going through the V8
> and V9 Gaussian model fits and trying to get the feel for what we are
> and are not handling within the chi-squared.

Changing the inner jet parameters does make a small difference to the overall chi-squared (which is why the full optimization didn't do so well). But the number of points affected is also small, so the change are quite significant.

>
> I agree it's interesting that we can get a fairly good fit without the
> spine at all for the outer region, and the fit to the "spine"
> polarization is actually pretty good these dats except that the
> predicted shear layer polarized intensity seems to be either too high
> (in the continuous outer parts of both jets) or too low (where we get
> the arcs). It's okay on average I guess but there are some unfitted
> systematics there still.

We have to interpret the field structure as some sort of crude average over the arcs and the intervening stuff. It may be that much of the enhanced toroidal field comes from the arcs.

> it helps to be showing people the actual
> images of the data and models side-by-side of course, and finally
> there are some folks taking the velocity field idea a lot more
> seriously!!
>

On the matter of pictures .... I have been experimenting a bit. In an ideal journal, I would go for:

- I 0.75 arcsec; full area; colour
- I 0.25 arcsec; centre; colour
- %polarization, 0.75 arcsec, colour
- I+pol; 0.75 arcsec; contours + vectors
- I+pol; 0.25 arcsec; contours+vectors; main jet base only
- sidedness ratio; 0.75 arcsec; colour

Fall-back option would be grey-scales. I haven't managed to generate anything very satisfactory with GREYS: do you use TVCPS for grey-scale output too?

In addition, I have made profiles of I, % and sidedness along the axis at both resolutions, with models and data superposed. The combinations which work are:

0.75 arcsec: I, %, sidedness; full length
0.25 arcsec: I full length; % main jet base only (possibly: the s/n is low, and the observed values are clearly just those where the polarized flux exceeds the clipping level, and are biased upwards)
I haven't managed to make any really good averaged tranverse profiles yet.

In order to display the model velocity field and emissivity, I guess that the best combination would be an image of emissivity with superposed velocity vectors, in a plane containing the jet axis. I think this could be made quite easily.

As if this wasn't enough, there probably has to be a sketch illustrating the geometry.

Any thoughts?

> I agree that in writing it up we should emphasize the generic model that
> we think is needed, that's the main thing to get across with 3C31 just
> as an example....and that the gaussian fits should be esentially a
> sidebar on parameter reduction and simplification. We may get a more
> spinal model out of NGC315 when the time comes, but we really need those
> new data before that is going to be worth modeling in detail, I hope the
> referees smile on it.

They should. It's interesting that the two obvious ways of getting perpendicular field with parallel-field edges (toroidal+ axial throughout or 2D in the centre + axial/toroidal at the edge) are still both possibilities, albeit in different sources.

Cheers,

Robert

From: Robert Laing <rl@ast.cam.ac.uk>
To: Alan Bridle <abridle@NRAO.EDU>
Subject: Re: forwarded message from VLA Operators
Date: Mon, 3 Mar 1997 22:24:07 +0000 (GMT)

I have also been hit by glitches, most recently an infuriating report suggesting a reorganization of our Technology Division along "more commercial" lines (creativity -> 0). This annoyed me so much that I have used up time in replying to it. I guess it's worth the effort(?). I think I prefer being interrupted by a gamma-ray burster.

I also took a side turning when I convinced myself that the inner jet opening angle was only 4 degrees. This doesn't work, primarily because the brightest emission is not modelled properly - hence my questions. The optimization with JETANG1 allowed to float looks as if it is heading for 6 - 7 degrees, which is fine.

My current feeling about which models to show is that we should use the full spine+shear layer (SSL) fit, which works significantly better in the outer parts, but also quote the best values (and chi-squared) for the simpler SSL and Gaussian models to show that we can get away with fewer parameters and that the conclusions are essentially the same. Tomorrow, I'll probably want to use the Gaussian model because it has the fewest parameters. You have the casting vote (the velocity profiles are not very different, of course).

I'd value your thoughts on the outer region fit when you've seen the Gaussian model. The basic difficulty with the 2 simpler models is that the isophotes appear to converge to the jet axis (the bunsen burner look), rather than continuing straight. The more elaborate fit gets round this by having a relatively brighter spine at large distances. I'm not sure how much to read into this, since the arc intervenes at a critical position. What do you think?

I'm not sure whether Nature would regard 3C31 as sufficiently important/general for an article (cf. cloned sheep, monkeys, management consultants, etc.). I'm inclined to give it a try, given that they are more encouraging about the use of colour in the paper, as well as having the cover. It would be relatively straightforward to rewrite for MN. I'd be interested in your prescriptions for effective colour diagrams - I tend to use pseudo-colour with a linear transfer function and a restricted pixrange, and model + observed on the same plot.

One useful pair of diagrams would be a vector plot of the velocity field, and a grey-scale of the rest-frame emissivity, both in a plane containing the jet axis. I'll make these up shortly.

I haven't thought much about the physics recently - what did you think of Baan's paper?

Robert

From: Robert Laing <rl@ast.cam.ac.uk>
To: Alan Bridle <abridle@NRAO.EDU>
Subject: Models
Date: Mon, 24 Feb 1997 19:32:26 +0000 (GMT)

I have put the latest code in the usual directory, together with the output of the optimized model using Gaussian profiles, as FITS images. I am re-running the optimizations for the spine+shear layer models (simplified and full), since the code has changed slightly.

The main changes to beware of in v9 are:

VARYSPINE has mutated to FIXSPINE and means the opposite of what it did before (made the code a bit clearer).

New environment variable GAUSS, for Gaussian model profiles.

ALPHA is now in the constants file, where it belongs. As a consequence, ALPHAC had to be renamed ALPHCORE.

SLMIN -> SLMIN0, SLMIN1 (usual reasons).

NFREE no longer needs to be specified.

Some extra info appears in the log file.

There is a slight change to the emissivity calculation for RHO < RHO1: the program now has uniform emissivity over the shear layer (and spine, if used). There was also a bug (or an unintentional feature, anyway) if FIXSPINE = T: the radial/toroidal field ratio for the spine was set to its value at the edge of the jet, not that at the inner edge of the shear layer. This won't make much difference. It does raise the point that the assumed hard-coded variation of radial/toroidal field is not at all general, although it seems to work.

See what you think of the Gaussian profile model. I am tempted to use it because it has the smallest number of free parameters so far, and to say that we can get a slightly better fit with a more complex spine+shear layer arrangement, but that no new physics emerges as a result, and parts of the model are poorly constrained.

An interesting little point emerged when I was testing sensitivity to parameter variations: it turns out that some field configurations produce a jet brightness profile which is more centrally peaked than that of the counter-jet even if the velocity has no transverse gradient. This must be something subtle to do with the distribution of Doppler factors along the line of sight in an expanding flow (it happens slightly even if the field is isotropic). I haven't got a simple explanation for this. The effect isn't nearly big enough to account for 3C31's jet-CJ differences, but might do so in other sources.

What I'd like to do now is:

- agree on the "best buy" model;
- decide on criteria for acceptable ranges of parameters and complete the sensitivity analysis;
- work out how best to display the results.

I've made some pictures of longitudinal profiles of I, %P and sidedness with data and model superposed - these look quite good. I have had less success trying to make good grey-scales. Are we forced to use colour, do you think?

Robert

From: Robert Laing <rl@ast.cam.ac.uk>
To: Alan Bridle <abridle@NRAO.EDU>
Subject: Simplified models
Date: Mon, 24 Feb 1997 12:51:13 +0000 (GMT)

I now have simplified models with the original transverse velocity and emissivity profiles, and with Gaussian ones. In the latter case, the spine has no independent existence; in the former it is just defined as the flat part of the velocity profile. The fit is very slightly worse than that for the independent spine/shear layer models, but the number of free parameters is greatly reduced (and all will be well determined). None of the conclusions are affected at all. The Gaussian model has the 5 fixed geometry parameters + 22 variables allowed to float in the optimization (I split SLMIN into SLMIN0 and SLMIN1). It looks pretty good and, by eye (rather than by chi-squared) matches the data as well as the spine/shear layer model.

Here is the best-fit Gaussian model:

```
* GAUSS.DAT - input file for jet model (best guess v9 code;
* Gaussian velocity and emissivity profiles)
THETA 50.406
JETANG0 16.75
JETANG1 8.0
X0 0.2944
X1 0.089
XF 0.8
BETAISL 0.406
BETA1 0.794
BETA0 0.508
BETAF 0.273
VELINDEX 3.727
VMIN0 0.593
VMIN1 0.711
JUMP1SL 0.086
ESL_IN  1.358
ESL_MID 3.080
ESL_OUT 1.511
SLMIN0 0.304
SLMIN1 0.375
SLLTI  1.314
SLLT1  1.073
SLLT0  0.835
SLLTF  0.608
SLRTI  0.0
SLRT1  0.670
SLRT0  1.021
SLRTF  0.128
```

It has a reduced chi-squared of 1.52.

Looking at the way the analysis has gone, I think that the original spine + shear layer formulation is more appropriate for other sources, in particular those like 3C296 and PKS1333-33 which have much wider perpendicular-field regions (i.e. thin shear layers). 3C31 (and 3C66B) may have a lot more in common with wide-angle tails.

Anyway, we should decide which of the models to go for. I'll parcel up the v9 code when I have fixed instructions.txt, and will put 3 sets of models in the usual place: independent spine + SL; spine and SL with same emissivity variations along the jet and Gaussian profiles. I'll let you know when they are ready to pick up.

Robert

From: Robert Laing <rl@ast.cam.ac.uk>
To: Alan Bridle <abridle@NRAO.EDU>
Subject: Revised working notes on parameter sensitivity
Date: Wed, 19 Feb 1997 19:16:13 +0000 (GMT)

I think that the ranges for the parameters in the following note are now quite reasonable. I can't get decent constraints on the spine parameters, however. I am very tempted to replace the current formulation with truncated Gaussian emissivity and velocity profiles just to see how much difference this makes.

Robert

Results of varying parameters independently

Chi-squared values are given for each of the variables: number of degrees of freedom = 5345

THETA	41.639	46.639	51.639	56.639	61.639
	11600	8590	7890	9620	13200
SPANG0	1.722	2.722	3.722	4.722	5.722
	8200	8130	7890	7990	8230
SPANG1	1.317	2.317	3.317	4.317	5.317
	7990	7930	7890	7870	7880
BETAISL	0.1	0.2	0.3	0.4	0.5
	7940	7890	7860	7850	7850
	(N.B. slightly different code used for this one)				
BETA1	0.428	0.628	0.828	0.928	0.978
	11900	8600	7890	8500	9430
BETA0	0.313	0.413	0.513	0.613	0.713
	10400	8550	7890	8190	9240
BETAF	0.076	0.176	0.276	0.376	0.476
	14700	10100	7890	92700	13500
VELINDEX	1.595	2.595	3.595	4.595	5.595
	8040	7920	7890	7880	7880
VMIN0	0.217	0.417	0.617	0.817	1.000
	10000	8520	7890	8010	8680
VMIN1	0.288	0.488	0.688	0.888	1.000
	8100	7940	7890	7910	7970
JUMP1SL	0.031	0.061	0.091	0.121	0.151
	7940	7900	7890	7880	7890
JUMP1SP			0.000	0.500	1.000
			7890	7830	7810
ESP_MID	1.472	2.472	3.472	4.472	5.472
	8170	8040	7890	7890	8750
ESP_OUT	0.604	1.104	1.604	2.104	2.604
	9890	8230	7890	7950	8110

ESL_IN	0.568 7910	1.068 7900	1.568 7890	2.068 7860	2.568 7840
ESL_MID	1.165 13900	2.165 10200	3.165 7890	4.165 10500	5.165 23200
ESL_OUT	0.492 30600	0.992 14400	1.492 7890	1.992 14600	2.492 35200
SPINE_SL	0.262 8400	0.462 8030	0.662 7890	0.862 7940	1.062 8170
SLMIN	0.069 8990	0.169 8040	0.269 7890	0.369 8150	0.469 8650
SLLTI	0.000 8050	0.405 7990	0.905 7890	1.405 7880	1.905 7890
SLLT1	0.021 20900	0.521 11700	1.021 7890	1.521 9010	2.021 11600
SLLT0	0.000 21900	0.400 12000	0.800 7890	1.200 11600	1.600 19200
SLLTF	0.169 12500	0.369 9640	0.569 7890	0.969 13700	1.569 30700
SLRTI	0.0 7890	0.25 7890	0.5 7890	1.0 7890	2.0 7890
SLRT1	0.2 7920	0.4 7870	0.6 7840	0.8 7870	1.0 7970
SLRT0	0.094 9050	0.594 8270	0.994 7890	1.594 8810	2.094 10800
SLRTF			0.000 7890	0.5 7950	1.0 15500
SPLT1	0.023 Not yet done	0.523	1.023	1.523	2.023
SPLT0	0.079 8680	0.579 8150	1.079 7890	1.579 7840	2.079 7870
SPLTF	0.065 8230	0.365 8060	0.665 7890	0.965 7840	1.265 7860
SPRT1			0.0 7890	1.0 7940	2.0 8080
SPRT0			0.0 7890	1.0 7930	2.0 8090
SPRT0			0.0 7890	1.0 7890	2.0 7990

Comments on varying parameters

THETA: Mainly constrained by jet/counter-jet ratio for RHO > RHO1 (or,

```
         equivalently, the main jet flux), especially between RHO1 and RHO0.

SPANG0:  Poorly constrained.  By eye, one tends to interpret the
         "arc" in the main jet as part of the spine and therefore to suppose
         than the spine opening angle is larger than the optimized value.

BETAF:   Jet/counter-jet ratio and I profile for RHO > RHO0.

BETA0:   Jet/counter-jet ratio and I profile for RHO > RHO0.

BETA1:   Sidedness ratio and brightness distribution for RHO1 < RHO < RHO0.

VMIN0:   If too small, then the sideness ridge is not wide enough for
         RHO >~ RHO0; equivalently, the outer edge of the counter-jet is
         too faint.   Surprisingly difficult to exclude large values, since
         there is a ridge in the sidedness map even without a velocity gradient
         from centre to edge.  This may reflect the difference in assumed
         field structure between the spine and the shear layer, as well
         as the different Doppler factors along the integration paths at
         the same distances from the core in main and counter-jets (the
         effect is still present, but at a low level, if the emission is
         isotropic).  The most obvious errors if VMIN0 is too large are,
         unsurprisingly, around RHO ~ RHO0, where the sidedness ratio is too
         large at the edge of the jet: further out, the beaming factors at
         the centre and edge are quite similar even if VMIN0 is small.

VMIN1:   Affects CJ brightness and J/CJ ratio for RHO1 < RHO < RHO0.
         Knot in CJ transition region affects conclusions considerably.

VELINDEX: Affects sidedness ratio for RHO slightly less than RHO0.
         Increasing it above about 2 makes essentially no difference,
         but lower values are excluded.

ESL_MID: Brightness distribution for RHO0 > RHO > RHO1; both jets.
ESL_OUT: Brightness distribution for RHO > RHO1; both jets

SLLT0:   Degree of polarization at RHO ~ RHO0.  Deviation in either sense
         causes the edge polarization to be too high.  A low value of
         SLLT0 (less longitudinal) causes excessive central (transverse)
         polarization in the main jet as well.  Well constrained because
         of the need to balance high B perp polarization in the counter-jet
         against low edge polarization in both main and counter-jets.
SLLTF:   Degree of polarization in outer jets.  If too small, predicts too
         wide a B perp region in the main jet.  If too large, underpredicts
         ridge-line polarization on both sides.
SLRT0:   Too small => high parallel-field edge polarization at RHO ~ RHO0;
         too large => .... perp .......................................
SLRT1:   Large values give too little edge polarization in the transition
         region.  Ruling out small values is more difficult, since
         the best model doesn't do a good job of predicting the % polarization
         in the centre of the transition region (the field is diagonal to
         the axis, so it can't).  Best limit may be set by the absence
         of a parallel-field edge to the counter-jet.

Best guesses

Geometry - fixed by outer isophote
--------

X1       2.5 arcsec
X0       8.2 arcsec
```

```
JETANG0 16.75 degrees
JETANG1 isn't well constrained - what do we say about it?

XF      22.4 arcsec is an arbitrary fiducial point


Error estimates
---------------

Either from chi-squared or from comparison with I, % or sidedness maps.

Parameter      Best       Range

THETA          51.6       45   - 57     chi-squared

BETAISP        Not used
BETAISL        0.30       0.15 - 0.45   J/CJ ratio for RHO < RHO1
BETA1          0.83       0.7  - 0.9    sidedness for RHO1 < RHO < RHO0
BETA0          0.51       0.4  - 0.6    sidedness for RHO > RHO0
BETAF          0.28       0.2  - 0.4    sidedness for RHO > RHO0

VMIN0          0.62       0.4  - 0.8    sidedness for RHO >~ RHO0
VMIN1          0.69       0.5  - 0.9    sidedness for RHO1 < RHO < RHO0
VELINDEX       3.60      >1.5           sideness for RHO just < RHO

ESL_IN         1.57                     not adequately constrained
ESL_MID        3.17       2.7  - 3.7    brightness dist for RHO0 > RHO > RHO1
ESL_OUT        1.49       1.2  - 1.8    chi-squared

SLMIN          0.27       0.0  - 0.5    Transverse brightness profile RHO > RHO0

SLLT1          1.02       0.9  - 1.4    % map RHO1 < RHO < RHO0
SLLT0          0.80       0.7  - 0.9    % map RHO ~ RHO0
SLLTF          0.57       0.4  - 0.7    chi-squared + % map RHO > RHO0

SLRT1          0.60       0.4  - 0.8    % map RHO1 < RHO < RHO0
SLRT0          0.99       0.9  - 1.1    % map RHO ~ RHO0
SLRTF          0.00      <0.4           % map RHO > RHO0
```

Simplified model with spine and shear layer parameters set equal (V8 SIMPLE). This has the same power-law exponents and field structure coefficients (except for the increased radial component towards the edge) in the spine and shear layer, and SPINE_SL = 1 (=> matching emissivities).

The chi-squared value (8343 with 5369 DF) is not dramatically worse than the standard model (V8 OPT: 7880/5345). Looking at the maps, the differences are:

- simple model has too pronounced a spine at the ends of the modelled region;
- the field transition in the main jet is shifted too far towards the nucleus.

The differences are fairly subtle, as expected because of the small contribution to the total emission from a thin spine. We only have very weak constraints on the spine field structure parameters and, to a good approximation, the spine is defined only by the velocity profile.

The major remaining problem with this analysis is what to say about the spine parameters. To a large extent, the spine has no separate identity. It is quite narrow, and therefore its parameters are difficult to determine. What about using truncated Gaussian velocity and emissivity profiles instead?

From: Robert Laing <rl@ast.cam.ac.uk>
To: Alan Bridle <abridle@NRAO.EDU>
Subject: Models
Date: Mon, 10 Feb 1997 20:38:23 +0000 (GMT)

If you are happy with the latest model, I'd like to try to get this written up. What happened to my promises of doing this before looking at the A configuration, you ask? In retrospect, waiting for higher resolution was sensible: we would have dismissed the evidence for low jet/CJ ratios at the base, and missed something important.

The most important missing analysis, I think, is to do with allowed ranges of parameters. These appear to fall into 4 groups:

- determined by the outer isophotes (and not optimised);
- constrained by overall chi-squared;
- marginally constrained by chi-squared, but qualitatively affecting some feature of the model;
- hardly constrained at all.

The first lot are the outer geometry variables X1, X0, XF, JETANG0 and JETANG1, and are taken as given.

The second group either affects global properties or the brightness distribution for RHO > RHO0. The best determined parameters are THETA, BETA0, BETAF, ESL_OUT, SLLT0 and SLLTF. I think we can say something like:

Parameter	Best	Range (approx)	Main constraint
THETA	51.6	45 - 57	J/CJ ratio and main jet brightness profile for RHO1 < RHO < RHO0
BETA0	0.51	0.3 - 0.75	J/CJ ratio and brightness profiles for both jets, RHO > RHO0
BETAF	0.28	0.15 - 0.4	J/CJ ratio and CJ brightness profile for RHO > RHO0

I'm going through these systematically.

Some of the other parameters make obvious differences in specific areas. For instance, the region RHO < RHO1 has a fairly well specified shear layer, although the emission is too faint to make much difference to the overall chi-squared.

The parameters of the inner spine make very little difference to anything, since there is hardly any emission (none for RHO < RHO1). Some others are surprisingly poorly constrained (like VMIN1), and I'm trying to get something better for them.

I'm not sure that the chi-squared values tell us anything very quantitative about the fit, since we are unsure of the noise distribution and we are not, in any case, trying to fit a physical model. My rule of thumb for the model to look globally "different" from its optimum value is that chi-squared increases from 7890 (best) to about 10000 with the current 5345 DF. I'd hesitate to turn this into confidence limits!

A simplified model could be constructed by setting many of the spine parameters equal to those in the shear layer (although not all of the field ordering coefficients), and this might also be worth doing.

Does this sound like a sensible approach to you?

Cheers, Robert

P.S. Stuff about the MMA is interesting ... the UK angle on this is that there is some interest in participation in a mm array project, but no money to speak of (yet). We are in the middle of yet another extreme funding crisis (I won't weary you with the details), so discussion of future projects seems highly academic. However, one argument put forward by the mm types is that the UK won't have access to an array of this type if it doesn't buy in - hence my question. Meanwhile, there is a move to cut MERLIN operations severely and to leave it with essentially no development programme. So much for running complementary facilities.

From: Robert Laing <rl@ast.cam.ac.uk>
To: Alan Bridle <abridle@NRAO.EDU>
Subject: Latest version
Date: Mon, 3 Feb 1997 18:48:20 +0000 (GMT)

Dear Alan

Hope you have recovered from committees, snow, wood, proposal deadlines and other disasters, natural or otherwise.

I have put FITS files of the best subimages at 0.25 and 0.75 arcsec resolution, together with zipped tar file of the latest version of the code, misc files + instructions in the usual ftp directory. Be warned that the optimization runs specified in CONST1.DAT use smaller subimages (101 x 51) of the high-resolution map.

The model specified by VARS.DAT is near enough the best I have managed: I don't think there is much more to be done, although I want to run a few more optimizations to check that the field structure parameters are still tweaked up.

I have a bit of a problem with the physics of the transition at RHO1 (the fit is fairly good). It appears that there is a discontinuity at RHO1, in the sense that neither the velocity profile nor the emissivity can be continuous. For RHO < RHO1, the velocity profile of the dominant emission (assumed to be the shear layer) varies between BETAISL (= 0.6) and 0, to take account of the fact that, in 3C31, the inner jet/counterjet ratio is quite small. There is no evidence for any other material: either its emissivity is very low, or it is fast (e.g. a BETAISP = 0.95 spine with the same emissivity as in inferred for the shear layer would be effectively invisible). We cannot say much more, since the inner jet is faint and poorly resolved tranverse to its axis.

By contrast, all of the emission for RHO slightly larger than RHO1 is inferred to have velocities between BETA1 and BETA1*VMIN1 (0.9 - 0.6 for 3C31): faster than ANY of the emission seen in the inner jet.

It appears to be impossible to model the relative brightnesses of inner jet and transition region without a discontinuity in emissivity (variables JUMP1SL and JUMP1SP; the latter irrelevant for 3C31).

At present we insist on continuity of flow direction across RHO1. This implies that the flow in the spine decelerates whilst that in the shear layer accelerates, both remaining undeflected, which is silly.

In fact, I suspect that:
- the emission for RHO < RHO1 comes from a thin, low-velocity surface layer;
- the centre of the jet is very fast in this region, but widens and decelerates suddenly at RHO1;
- who knows what its velocity profile might be;
- what we describe as the shear layer for RHO < RHO1 (the very low velocity emission) may not be present at larger distances - if it is, it is too faint to see;
- what we describe as the shear layer at larger distances is higher-velocity emission.

The current model provides a good fit to the brightness distribution in 3C31, as would several other descriptions (there are too few constraints). Is there a model which is physically self-consistent, but which also fits the data? Advice appreciated.

I think the hard computing is done until the VLA upgrade, however.

Robert

From: Robert Laing <rl@ast.cam.ac.uk>
To: Alan Bridle <abridle@NRAO.EDU>
Subject: 3C31
Date: Tue, 28 Jan 1997 19:43:06 +0000 (GMT)

Dear Alan

Haven't heard for a while - hope things are OK with you.

I now have a pretty good combined model for the 0.25 and 0.75-arcsec images. I had to introduce two new features to the model. One is a direct result of the lower jet/counter-jet ratio in the innermost region. The data seem to want the majority of the emission to come from material with a mean beta of 0.4 or so (adequately modelled by a shear layer with velocities evenly distributed between 0 and 0.75). In contrast, the brightest part of the base wants a higher maximum velocity, with negligible low-velocity emission. This problem has been around for some time, as I think you pointed out, and the high-resolution data just confirm it. What may be happening is that we see only peripheral emission for RHO < RHO1 and that there is a major change at RHO1, which causes much of the previously-hidden material to become visible. The low-velocity edge may then too faint to show up.

The second change is to allow a step change in emissivity at RHO1. In previous models, the rising power law in emissivity for RHO < RHO1 was actually an attempt to interpolate between the inner (faint) jet and the first bright knots in both jets. The high-resolution map shows that this cannot be right. With this change, the emissivity increases by about a factor of 10 at RHO1 and the power law fall-off becomes r^{-1.7}. It is interesting that, with the various changes, the power law exponents in spine and shear layer have converged. It may well be that a good model will result with identical exponents of 1.7, 3.5 and 1.5 or so.

The fact that the emissivity and the shape of the velocity profile must both change at RHO1 is intriguing. My first guess would be a shock of some sort.

I'm now running what I hope will be a final set of optimizations using the two resolutions. I should be done in a day or two unless they run amok or our latest funding crisis diverts my attention.

Cheers, Robert

P.S. Could you pass on the RGO address to the NRAO Director's Office? I just got a copy of the Assistant Scientist/GBT job ad and a request for names via Herstmonceux (a mere 2 months after posting).
P.P.S. I'd probably have applied if I had any credentials in single-dish observing: PPARC are talking about closing telescopes.

From: Robert Laing <rl@ast.cam.ac.uk>
To: Alan Bridle <abridle@NRAO.EDU>
Subject: Progress
Date: Tue, 21 Jan 1997 17:47:00 +0000 (GMT)

I have now finished the ABCD maps of 3C31, and have a "standard" set at 0.25 and 0.75 arcsec resolution. The quality is pretty good: both have off-source noise at around 6 microJy/beam in all 3 Stokes parameters. I have made clean and maxent I maps, and the differences are very small: the clean stripes which afflicted the BCD clean maps at 0.75 arcsec went away with the extra coverage from the A-array baselines.

Slight reservations:

- very low-level wings near the core, transverse to the jet axis, on the 0.25-arcsec maps (possibly real);
- for some reason the peak flux comes out 0.6 mJy lower on the 0.75 arcsec image;
- integration over the images (for purposes of flux normalization) gives significantly different answers, despite the fact that the zero-levels are small (I'm not too bothered about this since the s/n at 0.25 arcsec is low except very close to the nucleus and the integration area is large: the maps agree much better close in).

On the modelling front, I have a version of the program (v8) which will make either 1 or 2 sets of maps, and which optimizes on the sum of chi-squares from specified areas of the 2 maps in the latter case. I'm experimenting with this at the moment. I have set it up to use the high-resolution map for the inner region (X0 < 0.29); otherwise the lower resolution, and have been playing around with the relative weights. At the moment, I'm trying the sum of the reduced chi-squares for each area, since the base is not contributing enough to the raw chi-squared. The latter worked quite well in the case of a single map, primarily because the flux was much higher in the base.

Testing the new code revealed a bug in the previous version. One of the expressions for QCHISQ_IN, QCHISQ_OUT, UCHISQ_IN or UCHISQ_OUT in makechisq.f had an obvious typo (I've deleted the offending version, so can't recall the exact details). I don't think that this has had any serious effects.

I'll put the new code + subimages in the usual place when I have a good model for the jet base at 0.25 arcsec.

Robert

From: Robert Laing <rl@ast.cam.ac.uk>
To: Alan Bridle <abridle@nrao.edu>
Subject: Re: Revised velocity law
Date: Wed, 30 Oct 1996 18:20:00 +0000 (GMT)

On Wed, 30 Oct 1996, Alan Bridle wrote:

>
> It seems reasonable that it should not make much difference to the fit,
> or am I missing something? Can we use the new chisquared to express
> the "goodness of fit" in a way that statisticians would recognise?
>

The only change to the fit would come from the slightly different weighting of jet and counter-jet. Not a big deal. I have been talking to statistically-minded colleagues here about the meaning of the fit. It is a bit tricky to assess this. I think that the key points are:
- our error model is a very crude approximation, so levels of significance don't mean very much;
- we have no real reason to suppose that the model should fit the data exactly: we are trying to extract conclusions about generic models, rather than to test a specific one;
- more practically, the chi-squared values are dominated by the outer jet, and the sum is insensitive to quite large errors close in (I think we have to assess the fit in these regions separately).

> I gave a 15-min talk on this at the Jansky symposium on Monday.
> Most people were quite astounded at the ability to fit at this
> level of detail at all, especially the VLBI contingent who were
> decidedly sheepish about some of their one-Lorentz-factor-fits-all
> analyses later. Got into a good discussion with Dave Hogg about
> the same boundary polarization problem that we discussed in
> Tuscaloosa -- how come we see high polarization near the edge
> if there's a turbulent entrainment layer there, or just beyond it?
> Is it enough just to hope that the relativistic particle density
> is small in the entrainment region, so we don't see it?

Another reason might be that the emission we see actually comes from filamentary structures with some preferential range of orientations (defined by large-scale eddies, which will certainly not be isotropic?) rather than from the general turbulent gunk.

>
> In fact one of the differences between the models and the data now is
> that the model predicts higher polarization on the edges of the jet
> than we see everywhere.

The models are a bit misleading, because of blanking of the real image. I found it useful to blank the model: intensity levels are very low where the highest polarization occurs.

> The error is quasi-periodic and also connected to the "arcs", of course.

Again, some of that may be due to enhancement of total intensity, rather than degree of polarization, although there must be something else going on because the vector directions are affected.

> It makes me wonder if the arcs

> are indeed something to do with a macroscopic (fluting) pattern in the
> entrainment.
>

Interesting. Are structures as large as the jet radius seen in supersonic, turbulent jets?

> I made the case that because we seem to see the deceleration starting
> at the edge and working its way in, this looks more like deceleration
> by entrainment than slowdown by mass-loading. Got some sage nods from
> the audience, but in odd moments of reverie during the rest of the
> symposium (I have a ferocious head cold at the moment, so have a
> decongestant-induced stupor on top of my usual one)

Just got rid of one of my own. Bad luck.

> I was
> second-guessing this. Stellar mass-loading might provide a more
> uniform "drag" on the jet and thus be more likely to keep the field
> configuration and velocity the same all across the spine as we have
> assumed.

That had certainly been my assumption. Stars ought to be distributed throughout the jet volume. I had a look at Bowman et al. (1996): they don't give a transverse velocity gradient, but looking at their emission models, I can't see much evidence of slower material near the edges. I'll ask Paddy Leahy.

> And perhaps some magnetic tension affects apply braking to
> the shear layer. Perhaps we have to reconnect fields out on the edge
> to prevent this and keep the jet flowing in the outer layers? I
> wonder if we can really hope to distinguish the two deceleration
> mechanisms at this point?

Not sure I understand your point here. I still think that the transverse velocity gradient is prima facie evidence for a boundary layer of some sort.

Robert

From: Robert Laing <rl@ast.cam.ac.uk>
To: Alan Bridle <abridle@NRAO.EDU>
Subject: Progress on 3C31
Date: Thu, 19 Dec 1996 17:56:07 +0000 (GMT)

I now have A, B and C arrays concatenated. The resulting map looks good, apart from residual weak positive "ears" on the core, which are proving rather resistant to treatment. I occasionally suspect that they might be real. The only reason to worry about them is that they are not much fainter than the first knot in the counter-jet. The sidedness ratio close to the nucleus has increased as the maps got better. I have done a fairly careful search for bad data, without much success (apart from antenna 2 in the A configuration, which was noisy enough to throw out).

Adding the D configuration will be slightly tricky, since the amount of flux in the high-resolution model is on the low side even for its longest baselines.

I am about to modify the model to deal with 2 sets of maps. I'll attach my notes to this message - any ideas (simplifications, especially) would be welcome.

Robert

Changes required to use two sets of maps

1. We need to decide whether to make two models on different grids, or to make one model and convolve it appropriately. The former option is almost certainly a lot faster in the case where we want to model a restricted region at high resolution and a larger one at low resolution, so we will adopt it.

2. Add new integer global variable NRES (currently = 1 or 2). This is the number of sets of maps to be used (model.inc, readconsts.f).

3. Environment variables:
```
 IMAPFILE -> IMAPFILE_HI, IMAPFILE_LO
 QMAPFILE -> QMAPFILE_HI, QMAPFILE_LO
 UMAPFILE -> UMAPFILE_HI, UMAPFILE_LO
(readmaps.f)
```

4. Arrays to contain maps and their associated parameters.
```
 Size parameters MAPX, MAPY -> 2-element arrays;
 similarly for PIXEL, FWHM

 IMAP, QMAP, UMAP dimensioned (-XMAX:XMAX,-YMAX:YMAX, 2)
```

5. For the moment, assume that RA, DEC, ROTN, FREQ, BW and EPOCH are the same for both sets of maps.

6. The model is specified almost entirely in grid coordinates, and this is now ambiguous, since the sizes of the 2 sets of maps may be different. We need to interpret the model parameters as referring to one of the maps, and to scale them to the other one. We need to put this scaling in the code that translates from the VALUE array to the model variables in COMMON, i.e. inside the big nested DO-loop in model and in the scaling from the VAR array in csfunc.

7. The arrays IARR, QARR and UARR used by model and csfunc need to be dimensioned (-XMAX:XMAX,-YMAX:YMAX, 2).

8. Both model and csfunc will have DO-loops (1 to NRES) to compute 1 or 2

model sets, but makemodel and makechisq should be unchanged, except in so far as they use global variables which are dimensioned differently.

9. Specification of chi-squared. The number of variables is getting excessive: can we cut it down? The default option is to use separate noise levels for:

- I and Q/U (have to keep this)
- different maps (obviously necessary)
- inner and outer regions
- vertices defining the area over which chi-squared is calculated.

This requires doubling the number of inputs and making the internal variables 2-element arrays. One possible simplification is that, for 3C 31, the transition between inner and outer regions is probably where we switch from high- to low-resolution maps. This won't always be the case, though, so I suppose all of the variables must remain.

From: Robert Laing <rl@ast.cam.ac.uk>
To: Alan Bridle <abridle@cv3.cv.nrao.edu>
Subject: Re: v7 code etc.
Date: Thu, 14 Nov 1996 15:40:55 +0000 (GMT)

Various things crossed in the post - this is intended as a reply to your last 2 messages.

> The sidedness pattern has gone very "conical" now. It's harder to
> evaluate because the north and south sides of the jet behave rather
> differently, but I think if we discount everything to do with the arc
> the new fit is a bit better for the outer jet. We still don't get
> down to as low a sidedness as in the data on the north edge, but it's
> a fair fit to the south edge, and the outer spine looks quite
> reasonable until the arc. Beyond the arc, we are still
> over-predicting the sidedness pretty much everywhere, and have been
> for some time. The overprediction seems to be concentrated more
> towards the center of the jet now than it was before, which seems a
> bit odd if the spine is not contributing much.

The trouble with the outermost sidedness is that we can't model abrupt changes. The on-axis sidedness changes by about a factor of 2 going through the arc. Whether this is a sudden deceleration or (more likely, in my view) that the arc has spuriously boosted the sidedness at about 25 arcsec from the core, we can't be sure. I'd be happier if the sidedness was 1 at the end of the jet, though. I made a symmetrized sidedness map by flipping about the x axis and averaging and this indeed confirms what you say about the N vs S differences. The average of the two is pretty close to the model result. For purposes of display, these artificially symmetrized images are quite useful - the model fit does the averaging for you, but the eye sometimes has problems.

Although the spine emissivity is quite small, the velocity of material at the spine/shear layer interface is still appreciable, hence the central concentration, I think. It's not really that the jet is spineless, more that it has a spine with a low emissivity.

Switching to a power-law velocity fall-off at large distances may be responsible for the central sidedness staying a little too high. We could revert to linear, at the cost of having to explain away some unphysical piling up of material where beta = 0.

The other thing that may have changed is the switch to a larger value of VMIN0. This is forced to continue to the end of the jet. If the velocity were a bit higher at RHOF, we might be forced to add a VMINF. As it is, I don't think it will make much difference.

>
> The thing we have never been able to fit well is the fact that the
> actual sidedness peaks in the transition zone, but the model sidedness
> peaks close to the core. This seems to me to be a rather basic trend
> in a decelerating jet, as the sidedness of everything along each line
> of sight has to decrease outwards if all the velocities are
> decreasing. The only way I can see to counteract this is to adjust
> the velocities to boost the faster (high-sidedness) emission from the
> spine relative to the shear layer in the transition region, but not
> closer in, which will again mess with the polarization (it might
> help the counterjet, but not the jet). It's hard to see how to
> do this while holding the peak sidedness up in the low 20's, unless
> we move the jet a bit closer to the line of sight. (The fitted angle

> has been trending down for some time in fact, so maybe this is trying
> to nudge us in that direction?)
>

We've sort of covered this point in the exchange of messages before this one. Various things could be done: another is to have a weak, low-velocity component which dominates close to the nucleus where the high velocity stuff is suppressed, but which is swamped further out. However, I'm not convinced that the observed sidedness peak is anything other than a very bright filament (+ the absence of a corresponding one in the counter-jet?). Your point about the field going oblique fits very well with this, and the observed sidedness profile is very bumpy. We will know much more from the A configuration data. The higher-resolution observations of M87 and 3C66B (both radio and optical) suggest that the emission in the innermost regions could well be dominated by filaments, and that the brightess distributions are far from smooth. I've also wondered whether the balance of toroidal and longitudinal B is really just caused by a load of filaments wraped around the jet at different pitch angles. If a single filament dominates the emission (because it is bright, or at high resolution), then we will see above-average surface brightness and a random field direction.

Fitting sidedness ratio or difference maps, or somehow telling the program to emphasise the differences would be interesting. I'll consult some of my statistically-minded colleagues. I had a wild thought yesterday about using MEM or clean techniques (cf. lens-clean, or whatever they call the program that deconvolves mass distributions from gravitational lens images), but thought better of it.

Anyway, I vote for calling a halt now, if only to avoid the temptation of peeking at the new data. I'll try to get something on paper a..s.a.p.

Robert

From: Robert Laing <rl@ast.cam.ac.uk>
To: Alan Bridle <abridle@cv3.cv.nrao.edu>
Subject: Might have cracked it this time
Date: Wed, 13 Nov 1996 15:53:53 +0000 (GMT)

I now have what looks like a pretty reasonable polarization compromise, which also gives a good chi-squared. The trick (I won't bother you with myriad false starts) turned out to be to allow the amount of radial field to increase from 0 at the spine/shear layer interface to a maximum at the jet boundary, roughly as the square root of the fractional distance into the layer. It's qualitatively the same idea I was talking about yesterday, but with a simpler (and physically more reasonable) implementation. I don't mean to imply that the functional form is important, but rather that the data insist on having little radial field in the centre but lots at the edge, over the range rho1 < rho < 2rho0 or so.

A little further tweaking would be possible (I've perturbed the results of an older optimization), but most of the parameters are now pretty stable.

I've put IQU in the usual place as V7.I, Q and U. See what you think. If you are happy with the results, I'll send you the revised code too. I really think that this might be it (just as well, as I'm starting to see polarization maps in my nightmares).

Robert

From: Robert Laing <rl@ast.cam.ac.uk>
To: Alan Bridle <abridle@cv3.cv.nrao.edu>
Subject: Re: How's this for an idea?
Date: Tue, 12 Nov 1996 18:21:03 +0000 (GMT)

On Mon, 11 Nov 1996, Alan Bridle wrote:

```
>
> I think one of the root problems is that we don't have a very physical
> way to tie the magnetic field details to the velocity field.  It seems
> reasonable to suppress the radial B-component wherever we have a large
> radial velocity gradient, but to preserve it (and the azimuthal
> component) wherever there is a significant longitudinal deceleration.
>
> I agree that a region of disordered field is very likely at the edges,
> at or immediately upstream of, anywhere we see strong deceleration.
>
```

Yes, it may well be that random motions are as important as systematic velocity gradients in some regions.

```
>  > I wonder whether we are seeing the mixing region around rho0?  You
>  > remarked some time ago that there ought to be a region with isotropic
>  > field round the outside of the jet - perhaps this is it?
>  >
>
> Probably so. One question is whether we should try to model this
> explicitly, or simply to go as far as we can with a "simple" model and
> note that a discrepancy occurs on the edges in the region where we
> should expect entrainment to be going on, broadly consistent with
> extra field disordering as a result of such entrainment.
>
```

I am having a go at a model with a weak, isotropic-field region at the edge of the shear layer, but with most of the rest of the field ordering parameters held at their standard values. The depth of this region is allowed to vary with position, and is specified at the usual fiducial points. First results suggest that the overall polarization pattern can be significantly better provided that the outer ~half of the shear layer at rho0 has an isotropic field, although the emissivity has reduced there (perhaps too much to fit the total intensity - I have to re-optimize the other parameters after these changes, but chi-squared doesn't look too bad so far).

```
>
> At this point I would distinguish between extra ingredients that we
> might add to the model in order to explore the discrepancies
> semi-quantitatively, from ones that we consider part of a "basic"
> model".
>
> At present, I'm inclined to stick with the idea that we would publish
> the 2-d model, its successes and failures, as the basic story.  The
> 3-d models should basically be in our back pocket just to test what
> we say about the "failures" in terms of extra complexity that may be
> present in the boundary layer.  From the 2-d (anisotropic) model we have:
>
```

I don't think that the full 3D field has added much, but I think that the

idea of an isotropic-field region at the boundary has some merit. It is
hard to avoid something like it without crippling the counter-jet
polarization.

> 1. The overall polarization pattern specifies a gross form of the jet velocity
> field that implies gradual deceleration of the spine and general dominance
> of the emission in 3C31 by the shear layer. The magnetic field picture
> that goes along with this is first-order what you might expect from the
> flow physics, but is obviously still a simplification.
>

At a very basic level, we have been forced to the idea that the field
is mostly toroidal+azimuthal.

> 2. Within the velocity constraints from the polarization, we can also
> fit the intensity and sidedness profiles reasonably well provided we
> specify the velocity field in the shear in a certain way (initially high
> on the edge, transitioning to low in the "flaring" regime).
>
> 3. While satisfying both of these constraints, we are required to stay near
> the orientation angle limit suggested by the VLBI data for a high-gamma
> flow on parsec scales.
>
> 4. The model "succeeds" in several important areas, relating a first-order
> plausible B-field and emissivity variation to transitions in the jet
> collimation via a first-rder plausible velocity field. This is more than
> enough to suggest that the model is "interesting".
>
> 5. The model "fails" in two main areas: (a) the arcs in the outer
> region and (b) the edge polarization in and near the transition
> region. Both of these suggest that there are important details in the
> boundary layer that we cannot address via so simple a 2-d model. We
> can however offer some hints (based on toying with 3-d models) about
> where they come from, such as turbulence on the edges of the
> rapid-slowdown region and (perhaps) large-scale departures from
> axisymmetry in the entrainment. But we do not think it is worthwhile
> trying to fit 3-d empirical models in detail to these sources before
> exploring the underlying dynamics.
>

Cf. above. Do you agree that the arcs have surprisingly little obvious
effect on the degree of polarization? They look to me as if they affect
mostly the total intensity and the direction of polarization (e.g. where
the strong feature crosses the outer CJ).

> Our main point is that 3C31 imaging and polarimetry strongly support
> the idea that the FRI/FRII transition comes from the deceleration of
> relativistic jets across well-defined, deep boundary layers. And that
> the "failures" of the simple model are also quite reasonable ones.
> The failures are also unlikely to be explained in detail until we know
> how boundary layers and their fields develop in entraining
> relativistic jets, probably from numerical 3-d relativistic MHD. Both
> the "successes" and the "failures" of our 2-d model should motivate
> such work.
>

Not sure whether it will get much beyond motivation! I talked to
Komissarov & Falle last week. Neither was offering much hope.

Robert

From: Robert Laing <rl@ast.cam.ac.uk>
To: Alan Bridle <abridle@nrao.edu>
Subject: Polarization minimum
Date: Fri, 1 Nov 1996 18:52:56 +0000 (GMT)

I tried the simple experiment of putting an isotropic field in the shear layer transition region (rho1 < rho < rho0). This didn't really work, because the resulting polarization minimum is too near the nucleus (it should actually extend from 0.2 to 0.4 or so) and the high-polarization ridge in the counter-jet was destroyed.

It turns out to be relatively straightforward to add the third component to the shear layer in the alpha = 1 approximation. I will have a look at this next. I wonder whether the way to look at it is that the inner region has a shear layer with toroidal and longitudinal components in rough balance; then the entrainment becomes violent, creating a significant radial field in the eddies (hence the low edge polarization). Further out, the flow becomes much smoother (but still with a velocity gradient) and the field adopts the toroidal + longitudinal mix with the former dominating.

I think that the best thing to do is to allow the 2 independent field ratios to vary, using the usual fiducial points. I'm not sure whether log (as at present) or linear variations are appropriate. I may be quite tricky to avoid losing the highish polarization at the base of the counter-jet.

Turns out I won't be going to La Palma this month, so I'll concentrate on trying to finish up the model.

Robert

From: Robert Laing <rl@ast.cam.ac.uk>
To: Alan Bridle <abridle@nrao.edu>
Subject: Re: Latest polarization model
Date: Thu, 31 Oct 1996 21:06:52 +0000 (GMT)

I now have the spine field with optional longitudinal field going. This can be used to make a more realistic polarization profile along the jet, but doesn't help at the edges, of course. However, I think the point you made in your last message is related to this: we probably need almost to destroy the field order in the transition region (probably more so in the shear layer), leaving just enough to provide the polarization on the ridge of the counter-jet. The point you made earlier about the bifurcation in the polarized intensity image close to the nucleus in the main jet must be related.

I guess that the machinery for the alpha = 1 case should be up to allowing 3 unequal field components. In fact, I think I did this some years ago when exploring a dead end. I'll see what can be done.

I'll tidy up the instructions (which have got a bit out of date) and send you the latest code tomorrow. I need to go home and check whether the trick-or-treat kids have done anything horrible to my house (they threw an egg at it last year).

Robert

From: Robert Laing <rl@ast.cam.ac.uk>
To: Alan Bridle <abridle@nrao.edu>
Subject: Re: Latest polarization model
Date: Thu, 31 Oct 1996 20:20:08 +0000 (GMT)

On Thu, 31 Oct 1996, Alan Bridle wrote:

>
> Hi Robert,
>
> I now have the program running with the power law outer velocity and
> the intrinsic.f from your last update; so apart from the chi-sq values
> we should be looking at exactly the same stuff again.
>

I have finished the modifications I mentioned and am trying to test them. Unfortunately, an evil spirit (something to do with Halloween probably) tempted me into deleting the data instead of some old models, so I'll have to restore from tape before proceeding much further. The latest attempt has a new parameter to specify the oversampling (so that the summed chi-squareds are worked out from every nth pixel) and therefore deals with the degrees of freedom sensibly. It also gives the chi-squareds for regions inside and outside X0, rather than for jet and counter-jet, which is quite instructive. I have also allowed a non-zero longitudinal component in the spine field, and this looks as if it can help with the field transition in the middle.

> Just looking at the (4-sigma blanked) %p in the data and the last
> model run side-by-side, I think one of the most striking differences
> is that the polarization predicted for the shear layer in and near the
> transitioni region is significantly higher than we observe, on both
> the jet and counterjet. On the counterjet side, we seem to see the
> predicted polarization only in the outer regime, while on the jet side
> we start out with the observed edge polarization more or less as
> predicted but then the observations drop below the prediction through
> the transition regime, and begin to approach the model values at about
> the same distance as they do on the counterjet side.
>

That's a VERY good point. I had assumed, without proof, that we just didn't have enough intensity to see the polarization, but this is clearly wrong.

> Perhaps this really does suggest that the field in the shear layer is
> more disordered than we think in just the region where the effects of
> entrainment on the jet's structure are largest. I.e. it may be
> consistent with more small-scale turbulence in the shear layer in that
> region? The regions where the predicted degree of polarization is
> much higher than the observed are quite well transverse- resolved, so
> the discrepancy really may be mainly in the shear layer.
>

The implication is that the shear layer field has a significant radial component in the transition region, and therefore a near-isotropic field. But that makes it difficult to model the high B perp polarization along the ridge-line of the counter-jet in this region, which depends on the shear-layer field sheets being observed edge-on in their rest frames.

Definitely needs thought.

Robert

From: Robert Laing <rl@ast.cam.ac.uk>
To: Alan Bridle <abridle@nrao.edu>
Subject: Re: Spines
Date: Wed, 30 Oct 1996 18:25:10 +0000 (GMT)

On Wed, 30 Oct 1996, Alan Bridle wrote:

> 3C31 is of course the classic example of the parallel-perp-parallel
> field configuration and I agree that this may make it a bit
> anomalous. Looking at what happens a bit further out, where the
> whole transverse profile is decidedly flat-topped (again like 3C353)
> I'm not terribly surprised by this.

That's a good point (although we may be missing some parallel-field edges in weaker sources). 3C31 and 66B certainly appear to be anomalous in their edge polarization. Martin Hardcastle showed me some results on 3C296, which appeared to have at most a very weak parallel-field edge, but clear evidence for transverse velocity gradients.

>
> I agree that it may require some caveats about just how
> representative of the whole FRI group 3C31 really is, however.
>
>

I wonder if there is any difference between FRI's with bridges and those with tails?

Robert

From: Robert Laing <rl@ast.cam.ac.uk>
To: Alan Bridle <abridle@polaris.cv.nrao.edu>
Subject: Forgot
Date: Tue, 29 Oct 1996 15:21:32 +0000 (GMT)

As a result of putting code and data files in separate directories, I
forgot to send you the parameters for the current model. These are:

```
MAPX 140
MAPY 75
RA  16.853979167
DEC 32.412513889
ROTN 70.3
PIXEL 0.2
FREQ 8439.9
BW 100.0
EPOCH 2000.0
FWHM 0.7
XCMIN 6
YCMIN 10
XCMAX 135
YCMAX 40
SIGMA_I 8.4E-6
SIGMA_P 8.4E-6
CALERR 0.0
SOBS  0.244
SCORE 0.0885
FTOL 0.01
ALPHAC 0.0

* V3_1.DAT - input file for jet model (v3 code; full area chi-squared,
* 0.7 arcsec comparison)
THETA 53.895
ALPHA 0.55
JETANG0 16.75
JETANG1 8.0
SPANG0 3.491
SPANG1 2.0
X0 0.2944
X1 0.108
XF 0.800
BETAI 0.99
BETA1 0.927
BETA0 0.929
BETAF 0.380
ESP_IN 0.084
ESP_MID 3.165
ESP_OUT 3.079
ESL_IN -2.006
ESL_MID 3.728
ESL_OUT 1.204
RHOTRUNC 0.0
SPINE_SL 0.942
SLMIN 0.271
VMIN0 0.106
VMIN1 0.498
LG_ANISI  0.001
LG_ANIS1 -0.037
LG_ANIS0 -0.118
LG_ANISF -0.198
```

```
# C-shell file to run jet modelling program
# 3C31: 0.7 arcsec resolution maps
setenv OPTIMIZE F
setenv COMPARE T
setenv PLOTMAP T
setenv PLOTCHISQ F
setenv FLUXNORM T
setenv DOPOL T
setenv BTYPE SU
setenv IMAPFILE /scratch/rgosc/FITS/3C31LOW.I
setenv QMAPFILE /scratch/rgosc/FITS/3C31LOW.Q
setenv UMAPFILE /scratch/rgosc/FITS/3C31LOW.U
setenv CFILE /scratch/rgosc/rl/doppler/CONST1.DAT
setenv VFILE /scratch/rgosc/rl/doppler/V3_1.DAT
setenv LOGFILE /scratch/rgosc/rl/doppler/NEWBETA.LOG
/scratch/rgosc/rl/doppler/v3/model
```

From: Robert Laing <rl@ast.cam.ac.uk>
To: Alan Bridle <abridle@polaris.cv.nrao.edu>
Subject: Code
Date: Mon, 28 Oct 1996 19:46:15 +0000 (GMT)

```
I have put the latest version of the code in the usual place.  I also
tried a velocity variation proportional to rho**-1 for rho > rho0: this
gives roughly the same velocity at rhof as the previous one, and looks
quite reasonable.   I guess that some obvious things to do are:
 - try power-law variations of beta at least in the outer region (there
   are obvious problems close in) in order to avoid the logical difficulty
   of a stopped jet and to make comparison with adiabatic models easier;
 - with this, try an adiabatic model for the spine (i.e. specify the
   velocity parameters and derive the emissivity - might work).
I'm not sure that any such exercise makes sense for the shear layer.

Robert
```

From: Robert Laing <rl@ast.cam.ac.uk>
To: Alan Bridle <abridle@polaris.cv.nrao.edu>
Subject: Adiabats
Date: Mon, 28 Oct 1996 17:56:08 +0000 (GMT)

Komissarov's note indeed agrees with my understanding of the relativistic modifications. In fitting the spine to an adiabatic model, we have 3 problems, I think:

- We would have to assume that the emissivity fall-off for rho < rho1 is quite steep (about r**-3, I think). This may be OK, since we can't see much spine emission in this region anyway. We can't allow much deceleration here.
- The deceleration in the transition region would have to be from about beta = 0.96 to 0.75 in order to flatten the emissivity fall-off. I think this is allowed by the data.
- The killer seems to me to be to get the outer region right. The adiabat actually wants the emissivity to increase with distance from the nucleus, if we keep our current velocity law. However, the velocity law is non-physical, in the sense that beta -> 0, in which case the adiabatic expression (like the jet!) explodes. We could get round this by using a more sensible functional form. It will still be quite hard to keep the emissivity fall-off from becoming too flat, or even rising with distance from the nucleus. I reckon we would need beta proportional to rho**-0.4 or so for rho > rho0 in order to match the current best fit, although I suspect that a somewhat flatter emissivity fall-off would still fit adequately.

I think that the use of the linear velocity law in the outer region is probably a mistake, since we should really avoid anything which is obviously unphysical. A power law would be easier to cope with in the context of adiabatic models, so perhaps we should try that?

Robert

From: Robert Laing <rl@ast.cam.ac.uk>
To: Alan Bridle <abridle@polaris.cv.nrao.edu>
Subject: Errors on parameters
Date: Mon, 28 Oct 1996 11:40:13 +0000 (GMT)

Dear Alan

Thanks for the messages, and for the Komissarov note, which looks useful. Sam Falle is giving a colloquium across the road either this week or next, and I'll find out what the Leeds people are up to then. I'd be interested in the 3C 264 paper when it's ready for public distribution (STScI preprints appear to come by slow sailing vessel).

I am afraid I misled you slightly about chi-squareds - the new geometry does, in fact, still have slightly worse chi-squared than the original one. I was running a test with CALERR set to 1% and forgot to unset it again. I hadn't put the new code in the ftp area, but will do so today. As you say, the polarization near the field transition in the main jet still isn't quite right. The only obvious thing to do now is to put a small component of B_long in the spine - not technically difficult, but I'm worried by the number of degrees of freedom.

Having settled on a model, I have been trying to decide what to say about the errors on parameters. The trouble is that effects which we believe to be significant can have a smaller influence on chi-squared than the non-axisymmetric features we cannot model. I tried to estimate the magnitude of this effect, by differencing the image and its reflection in the x-axis. This suggested that an error model with sigma set to some fraction of the flux would work better than the current constant, but I have never got very satisfactory results when I tried this - not sure why. In addition, a number of the parameters are closely coupled, so assessing their errors by varying them independently is a bit tiresome. My impression is that we complain about a model fit if its chi-squared is 5-10 larger than the optimum, for the current normalization (how do we justify this in a convincing way?)

This allows us to make statements like:

- the central velocity for rho < rho0 must be >0.8
- there must be some limit on the fraction of low-velocity emission at small distances from the nucleus
- theta = 54 +/- 5 degrees (more secure, because it affects the whole fit)
- the ratio of longitudinal to toroidal field component in the outer region is 0.6 +/- 0.2

and so on. We are (obviously) much better constrained where we have good transverse resolution, and the conclusions about the inner regions are much less firm. I think we need to be quite cautious here.

I think it would be valuable for both of us to go through the model in a sceptical way, asking which of the conclusions are really solid. A modification to the code which might help is to write out the chi-squareds for the 3 regimes separately, since some parameters only affect individual bits (although even there, the flux normalization causes unintuitive correlations).

I scribbled an outline for a short paper over the weekend - I'll put something on disk and see if it makes sense. May already be too long for Nature, though.

Cheers, Robert

P.S. I'm looking forward to the A-configuration stuff and to M84 (I'll try to use this as an excuse to get a larger disk on my machine).

From: Robert Laing <rl@ast.cam.ac.uk>
To: Alan Bridle <abridle@polaris.cv.nrao.edu>
Subject: New geometry
Date: Thu, 24 Oct 1996 17:28:32 +0100 (BST)

I have put the results from the optimized model with the new geometry in my anonymous ftp directory as V3.I, .Q and .U. The chi-squared is slightly worse than with the oldest (discontinuous streamline) flow, but slightly better than with the second attempt (continuous streamlines with kinks).

I think I would recommend sticking with the latest version, since it avoids obviously non-physical flows. The models look pretty good to me. The main deficiencies for those of a critical turn of mind are:

- doesn't match the peak sidedness (but this may just be because of small-scale bumps);
- doesn't quite get the main jet polarization minimum right (crossover at 5 arcsec rather than 8 arcsec) - no model has ever succeeded here;
- produces a minimum in the counter-jet emission at 10 arcsec or so, rather than a flat intensity profile;
- if you go out further (as I did when making a sequence of models at different angles to the l of s), the spine emissivity becomes so low compared with the shear layer than the jet apparently bifurcates (but we aren't really trying to model that far out).

The ridge-line polarization is pretty good now, as is the jet-side profile. Most of the other discrepancies are clearly due to non-axisymmetric structures.

Regards, Robert

From: Robert Laing <rl@ast.cam.ac.uk>
To: Alan Bridle <abridle@polaris.cv.nrao.edu>
Subject: Models
Date: Fri, 18 Oct 1996 17:43:56 +0100 (BST)

After adjusting the geometry parameters, I can now get very similar results to ANIS1 and 2, but with the new geometry. Probably not worth all of the effort, except in so far as it makes it somewhat easier to translate the fitting results into physics (all streamlines are now continuous). The current state of play is:

```
MAPX 140
MAPY 75
RA  16.853979167
DEC 32.412513889
ROTN 70.3
PIXEL 0.2
FREQ 8439.9
BW 100.0
EPOCH 2000.0
FWHM 0.7
XCMIN 6
YCMIN 10
XCMAX 135
YCMAX 40
SIGMA_I 8.4E-6
SIGMA_P 8.4E-6
CALERR 0.0
SOBS  0.244
SCORE 0.0885
FTOL 0.01
ALPHAC 0.0
```

```
Old geometry                New geometry

THETA 56.990                THETA 56.060
ALPHA 0.55                  ALPHA 0.55
JETANG0 16.75               JETANG0 16.75
JETANG1 8.0                 JETANG1 8.0
SPANG0 5.621                SPANG0 4.311
SPANG1 3.0                  SPANG1 2.0
X0 0.2944                   X0 0.2944
X1 0.108                    X1 0.108
XF 0.800                    XF 0.8
BETAI 0.99                  BETAI 0.990
BETA1 0.945                 BETA1 0.978
BETA0 0.859                 BETA0 0.834
BETAF 0.352                 BETAF 0.391
ESP_IN 0.000                ESP_IN -0.183
ESP_MID 4.405               ESP_MID 3.109
ESP_OUT 1.699               ESP_OUT 2.307
ESL_IN -1.906               ESL_IN  -1.032
ESL_MID 3.851               ESL_MID 3.364
ESL_OUT 1.309               ESL_OUT 1.159
RHOTRUNC 0.0                RHOTRUNC 0.0
SPINE_SL 0.682              SPINE_SL 0.963
SLMIN 0.237                 SLMIN 0.196
VMIN0 0.153                 VMIN0 0.095
VMIN1 0.623                 VMIN1 0.491
```

```
LG_ANISI -0.409         LG_ANISI  0.022
LG_ANIS1 -0.003         LG_ANIS1 -0.014
LG_ANIS0 -0.110         LG_ANIS0 -0.092
LG_ANISF -0.171         LG_ANISF -0.183

Npoints 12702                   12702
I    chisq 0.469E+02            0.670E+02
QU   chisq 0.323E+02            0.318E+02
IQU  chisq 0.372E+02            0.435E+02
```

so the old geometry is still a bit better. I suspect that the values of RHO1 and RHO0 are not quite right for the new one. As you can see, most of the parameters come out reassuringly similar: in fact, optimizing over a different area probably introduces bigger changes than changing the geometry. The major difference is in the values of ESL_MID and ESP_MID, as expected (path lengths have changed), and in SPINE_SL (which I don't really understand).

Anyway, I agree with you that further tweaking is now almost certainly a waste of effort. I would have liked to do the case of alpha not equal to 1 more rigorously, but apart from that I think things have converged. I'd like to settle on a standard model soon, and make a resolution not to change it!

I am not sure how to quote the allowed range of parameters, given that the model doesn't "fit" the data in a true sense (there is a very broad minimum in chi-squared, which is always much greater than 1). Obviously, we cannot fit small-scale variations and non-axisymmetric structure. We need some sort of recipe for this.

Cheers, Robert

From: Robert Laing <rl@ast.cam.ac.uk>
To: Alan Bridle <abridle@nrao.edu>
Subject: Re: Progress
Date: Fri, 4 Oct 1996 15:35:48 +0100 (BST)

On Thu, 3 Oct 1996, Alan Bridle wrote:

> Hi Robert,
>
> I think a previous message of mine might have got lost, so if
> this turns out to be a repeat please forgive the confusion.
>

I think I replied to your message and then got side-tracked and forgot to put the new code where you could find it.

>
> Great news that the extra degree of freedom gets the polarization
> looking much better. Especially so soon after you got the more
> complicated code going -- any chance you could put the code or
> the present images somewhere I can ftp them some time soon? I'm
> _very_ curious to see the goodies myself, of course!
>
> Cheers, A.

New version of the program is now in the anon ftp area here. As before, cd pub/rl and get model.tar.gz. The current data files CONSTL.DAT and VARLOW.DAT are set up to do the optimized model with unequal field components and chi-squared evaluated over the central region. modell.csh has the right steering parameters.

I have also put some images and colour postscript files in the same area. ANIS1.I,Q,U are the convolved models with chis-squared evaluated over the whole (jet+CJ) region. ANIS2.I,Q,U are the output of the model using the variables in VARLOW.DAT and a restricted area for chi-squared. These are all disk FITS files. ANIS*.PS are files generated by TVCPS for ANIS1. They are I, P, %P, data/model and jet/cj in fairly obvious notation. ANIS2*.PS are the corresponding files for the other model (the data files are much smaller!).

My current scheme is to improve the verisimilitude of the transition region. At present, flow lines start from nowhere, which is unphysical. Also, the observed field lines suggest that the flow expands and recollimates. As I think I said in a previous message, my original idea was to interpolate using a cubic function which matched values and directions of flow lines at the transition radii. It turned out to be straightforward, but messy, to write down the expression for the flow lines, but hideous to convert from position in the jet to flow-line parameters. I then decided on a simpler approach, which is to abandon matching of derivatives, and just have flow along straight lines in the transition region, enforcing continuity at each end. Solving for the velocity in the shear layer still wasn't trivial (and isn't exact - I used a small-angle approximation), but I think I now have the maths done. I'll code it and see what happens.

It also occurred to me that doing the field sheet with unequal components using numerical integration might not be as hard as I thought, for 2 reasons:

- we probably don't need such a large range of anisotropy parameter and
- the way I set the problem up at the moment may have an unnecessary degree of freedom (I calculate I, Q and U directly, rather than calculating I and P as integrals and evaluating the PA separately). I'm not sure whether this can be done for the new field configuration, but if it can, only a 2D array is needed for the values of I and P.

I'll see whether the numerical approach is sensible.

I tried to put some of the maths into Latex the other day .... nearly drove myself insane and started to suspect that the worst difficulty in proving Fermat's last theorem was typesetting the paper.

Have fun.

Robert

From: Robert Laing <rl@ast.cam.ac.uk>
To: Alan Bridle <abridle@nrao.edu>
Subject: Re: Progress
Date: Fri, 11 Oct 1996 11:46:12 +0100 (BST)

On Thu, 10 Oct 1996, Alan Bridle wrote:

>
> I didn't try to grab the .PS files as they were so big, but I remade
> the model and some others here - I take it that the VARLOW.DAT in the
> ftp area did in fact correspond to ANIS2.
>

That's right. I've had to clear the disk space now, so I;m relieved you got the important bits.

> Do you happen to have a note of the VARLOW.DAT that produced your
> ANIS1, by any chance? It does do noticeably better on the large-scale
> sidedness, but I agree they the models are pretty good across the
> board now that the extra field freedom is there, so we can probably
> declare victory by the usual standards quite soon.
>

Yes, here it is:

```
THETA 56.990
SPANG0 5.621
BETA1 0.945
BETA0 0.859
BETAF 0.352
ESP_MID 4.405
ESP_OUT 1.699
ESL_IN -1.906
ESL_MID 3.851
ESL_OUT 1.309
SPINE_SL 0.682
SLMIN 0.237
VMIN0 0.153
VMIN1 0.623
LG_ANISI -0.409
LG_ANIS1 -0.003
LG_ANIS0 -0.110
LG_ANISF -0.171
```

Rest as in ANIS2.

> I notice that even in the center-weighted optimization the sidedness
> peak in the model is fighting hard to be closer to the base of the
> jet, and the modeled polarized intensity is much more obviously
> bifurcated as we start to resolving the shear layer than is the
> observed polarized intensity. Looks like the field in the actual
> shear layer is a little less axial in the transition regime than we
> are making it at present. Both of these seem to point to things still
> not being quite right in the first transition zone, and presumably
> this connects to your misgivings about the nonphysical velocity field
> there.
>

I agree. I have made a fair amount of progress with the alternative velocity configuration, although I seem to have made more than the usual ration of what Martin Ryle would have described as "clot errors" in the first attempt. The spine is right, as is the 1D shear layer: the rest is still a bit broken. I should finish this over the weekend, with luck, and rerun the optimization.

> Even so, my feeling from the responses I get showing any of this to
> people here is that the general populace is quite ready to agree that
> we are headed in the right direction. The local VLBI'ers (Tony Z.,
> Ken K.) are in fact quite astounded by the idea that so much image
> detail can be represented with just a few analytic forms!
>

Perhaps it will encourage them to dig out some VLBI counter-jets for us to practice on!

> There's an internal symposium going on here at the end of the month
> (immediately after I get back from DC for the NSF reviews, so I'll
> have to prepare everything for it this week). Will it be o.k. by
> you if I show the current state of these models there?
>

Of course. Let me know if you need more on any of the details.

> Cheers, A.
>

Regards, Robert

From: Robert Laing <rl@ast.cam.ac.uk>
To: Alan Bridle <abridle@nrao.edu>
Subject: Re: More models
Date: Mon, 19 Aug 1996 19:04:12 +0100 (BST)

On Mon, 19 Aug 1996, Alan Bridle wrote:

>
> Does this imply we should still be adjusting the zero level on the
> high-resolution image? It seems strange if we have higher
> sidedness ratios on the lower-resolution images, I don't immediately
> see how that can happen physically (i.e. I agree there is a whiff
> of rodent in this).
>
> I suppose this increases the premium on getting the higher resolution
> "for real" to check out what is going on?
>
> A.
>

I am afraid that the rodent is a large and odorous one. The discrepancy caused me to do some experiments, with disturbing results. Firstly, I tried adjusting the zero-level on the high-resolution map to equalize the on-source flux densities at the 2 resolutions. This produced a very negative off-source level and made little difference to the sidedness map.

I then convolved the 0.3" map to 0.7" and regridded it. There is a very large difference between the 2 images, which is neither a simple multiplication nor an additive constant. What looks to have happened is that the high-resolution image has additional flux not present at low resolution, distributed fairly uniformly over the source region (much more uniformly, in fact, than the real structure - perhaps like a low-pass filtered version?). This has proportionately more effect at low intensity, and is therefore diluting the J/CJ ratio, as well as making both jets apparently less centrally peaked. In fact, the opening angle for the outer isophote is probably a little less for the original 0.7 arcsec map. Unless there is a processing foul-up somewhere, I suspect that the problem is in the high-resolution image at low S/N. I think that it is probably underconstrained, and that, although the basics of the structure are correct, we are pushing the data too far in the quantitative analysis. It is a bit unfair to expect MEM to do a perfect job in the more diffuse regions, after all.

It would probably be a good idea if you also had a look at the problem, in case I've made a blunder - in any case, the effects are a bit difficult to describe in words.

As a fall-back, suppose we adopt the position that we model the 0.7-arcsec data, using the super-resolved image to set the form of the model, especially at small distances from the nucleus? I don't think that our basic conclusions will be affected much, if at all.

Robert

P.S. I have now added the code to do different field configurations in the shear layer. I'll send you the revised code as soon as I have tested it.

From: Robert Laing <rl@ast.cam.ac.uk>
To: Alan Bridle <abridle@polaris.cv.nrao.edu>
Subject: More models
Date: Mon, 19 Aug 1996 13:18:10 +0100 (BST)

Dear Alan

I have now debugged the optimization program for different grids, resolutions etc. The results of using the 0.3 and 0.7 arcsec resolution maps are significantly different in one important respect. The high-resolution data require theta to be close to 60 degrees; the 0.7-arcsec maps want something nearer 50 deg. This is almost all a consequence of the higher sidedness ratio at the lower resolution. This is, in itself, a bit of a concern. The other area where there is a smell of rat is in the amount of flux on the map. Integration over the high-resolution I-map (with the assumption that the beam actually is a 0.3-arcsec FWHM Gaussian) gives a lot more flux than we see on the 0.7-arcsec map. I get 450 and 330 mJy, respectively (including the core, which is almost the same in both). The reason must be that there is a small, positive bias in surface-brightness in the high-resolution map, which is then integrated over lots of pixels (there are also problems in determining the zero-level).

The fixed parameters in the optimization are:

ALPHA	0.55
XI0	16.75
XI1	8.0
ZETA1	3.0
RHO0	0.2944
RHO1	0.1083
RHOF	0.8660
RHOTRUNC	0.0
VMIN0	0.0

I have included the results for 3 optimization runs. The first 2 are for 0.3 arcsec resolution. Run 1 has equal rms for I and Q/U; run 2 uses the off-source rms's, which differ by a factor of 2 (Q/U lower, of course). The third run uses the 0.7 arcsec data, and equal rms's.

The best values of the varying parameters are:

	1	2	Low-res	
THETA	58.335	61.444	51.886	(driven by larger J/CJ at 0.7 arcsec)
ZETA0	4.694	6.379	5.398	
BETAI	0.999	0.999	0.999	(truncation value: program wants >1!)
BETA1	0.949	0.946	0.985	
BETA0	0.691	0.774	0.801	
BETAF	0.270	0.128	0.269	
ESP_IN	-1.719	0.293	0.345	(very poorly constrained; no spine emission)
ESP_MID	4.925	4.855	5.241	
ESP_OUT	0.980	0.958	0.813	
ESL_IN	-1.199	-1.413	-0.887	
ESL_MID	3.580	3.846	4.050	
ESL_OUT	1.327	1.231	1.458	
SPINE_SL	1.211	0.973	0.926	
SLMIN	0.726	0.553	0.387	
VMIN1	0.648	0.514	0.737	

You will see that a noticeable change from our earlier models is the lower value of zeta0: this gives a spinier look to the source and matches the data significantly better. The initial velocity has to be extremely high (the optimization wants it to be >1, but the modelling code clips this, of course). As a consequence, the value of esp_in is very badly determined. With the exception of theta, I think that the differences in parameter estimates are within the errors (we could assess this systematically).

The fits to total intensity at both resolutions are now extremely good. The one area where the model comes unstuck is still its underestimation of B-perp polarized intensity in the centre of the counter-jet and, to a lesser extent, its difficulty with the field transition in the main jet (this isn't too bad at 0.7 arcsec).

Regards,

Robert

From: Robert Laing <rl@ast.cam.ac.uk>
To: Alan Bridle <abridle@polaris.cv.nrao.edu>
Subject: Driving instructions
Date: Wed, 19 Jun 1996 19:06:10 +0100 (BST)

Jet model optimization program

The program is called optimize, and the main program is in optimize.f. The two shell files optcomp and optlink compile and link the program (and therefore contain a list of all of the modules). Note that the main program contains a heavily nested DO-loop and has to be compiled (under Solaris) with the -Nc30 switch. I do not know what might happen with other implementations of f77.

Input files

1. The program expects the VLA data to be in three files called 3C31.I, 3C31.Q and 3C31.U in the same directory as the executable. These files are the output from IMTXT with an E10.3 format descriptor.

2. The set of parameters to be used is defined in a file called PARAMETERS.DAT, which should also be in the same directory as optimize. Default values are defined in the code, and will be used in the absence of an entry in PARAMETERS.DAT. The names of the parameters and their default values are as follows:

```
60.0,     ! THETA
0.55,     ! ALPHA
16.75,    ! XI0
8.0,      ! XI1
8.375,    ! ZETA0
3.0,      ! ZETA1
0.2944,   ! RHO0
0.1083,   ! RHO1
0.95,     ! BETAI
0.95,     ! BETA1
0.75,     ! BETA0
0.2,      ! BETAF
0.8660,   ! RHOF
0.0,      ! ESP_IN
4.75,     ! ESP_MID
1.0,      ! ESP_OUT
0.0,      ! ESL_IN
3.75,     ! ESL_MID
1.65,     ! ESL_OUT
0.0,      ! RHOTRUNC
0.8,      ! SPINE_SL
0.7,      ! SLMIN
0.0,      ! VMIN0
0.8,      ! VMIN1
```

These are as used in 2D TRIPLE 58, with a change of convention for RHO0, RHO1 and RHOF. These ar now defined in the plane of the sky, rather than in the frame of the jet (and their values have therefore been multiplied by sin 60 = 0.866).

The format of PARAMETERS.DAT is as follows:

A set of parameters is specified by giving the name of the variable (in full, and in upper case, starting in column 1) followed by up to 10 values, separated by spaces. For example:

```
THETA 55.0 57.5 60.0 62.5 65.0
```

Anything after a ! or * is treated as a comment. Lines starting with ! or *, or entirely blank lines, are ignored.

To run the program, type

```
optimize
```

You will be asked whether you want the model and chi-squared maps to be written (answer y or n).

The program:
- reads in the VLA images
- rotates Q and U (incidentally, the headers still have CROTA2 = -70.3 deg, so PCNTR will malfunction on the rotated images)
- reads the PARAMETERS.DAT file and tells you what default values are being used
- opens any output files and writes their headers
- sets up the configurations to be modelled
- makes the models, writing out maps and chi-squared images
- writes a log file.

The format of the log file is:

```
Configuration    1
 50.000  0.550 16.750  8.000  8.375  3.000  0.294  0.108  0.950  0.950
  0.750  0.200  0.866  0.000  4.750  1.000  0.000  3.750  1.650  0.000
  0.800  0.700  0.000  0.800
 0.195E+07 0.187E+07 0.861E+05 0.243E+07 0.227E+07 0.160E+06
 0.632E+06 0.518E+06 0.115E+06   57544
Configuration    2
 60.000  0.550 16.750  8.000  8.375  3.000  0.294  0.108  0.950  0.950
  0.750  0.200  0.866  0.000  4.750  1.000  0.000  3.750  1.650  0.000
  0.800  0.700  0.000  0.800
 0.587E+06 0.482E+06 0.105E+06 0.173E+07 0.153E+07 0.207E+06
 0.610E+06 0.481E+06 0.129E+06   57544
```

and so on. The configuration number is included in the output filenames, for example configuration 1 corresponds to

IMAP01.TXT, QMAP01.TXT, UMAP01.TXT, ICHISQ01.TXT, QCHISQ01.TXT and UCHISQ01.TXT.

The first 3 lines are the input parameters, as entered, in the order given above. The remaining 2 lines have chi-squared values in the order I (whole source, jet, counter-jet), Q (ditto), U (ditto), number of points. Chi-squared is evaluated over a pair of quadrilaterals chosen just to include the jet and counter-jet, but not the core. The vertices are at

(6,6), (274, 102), (274, -102), (-6, -6) if the core is at (0,0) - add (275, 103) to get AIPS coordinates. I have assumed that the off-source noise levels are appropriate for evaluating chi-squared, but this just scales everyting, of course).

The models are as we had previously and the chi-squared images can be duplicated to within digitization accuracy, using COMB. I'm therefore fairly sure that there are no major bugs. I am going to let the program roam through parameter space tonight and see what happens - more intelligence can be introduced tomorrow.