From root Mon Oct 5 16:43:24 1992
From: dclarke@chandra.harvard.edu (David Clarke)
To: abridle@polaris.cv.nrao.edu, rperley@aoc.nrao.edu
Cc: dclarke@chandra.harvard.edu
Subject: Re: Bow shocks
Date: Mon, 5 Oct 92 16:41:13 EDT

OK, here it comes. I am beginning to come around to dismiss the so-called compression model too. Despite everyone's valient attempts, I just hadn't appreciated the "power" of Doppler favouritism. At any rate, Alan brought up again - this time I heard it - the notion that there really is something still (apparently) feeding the southern hotspot. I look back in the 1991 paper that Jack and I put out which discusses the restarting jet scenario, and it seems to me we may be seeing part of that in 219. In this model, the old jet is *still* feeding the hotspot - we just gotta look! It's right there in that "extension" from S9 to the core that both CBBPN and BPH pointed out. When the old jet got cut off, as it were, a rarefaction wave travelled down the pipe at the jet speed plus the sound speed, which for highly supersonic velocities, is just v_jet. That takes a non-zero time to happen, during which time the hotspot is unaware that the jet has been turned off and thus remains bright and compact. Could this extension back to the core be that vestigal jet? And the edge-brightened features could be the rim of the now hollow cavity which once housed the jet. In time, this cavity will collapse onto itself, but in the meantime, it is filled with cold (the rarefaction wave acts like a rapid decompression, sapping the stuff of its energy), non-emitting stuff which should yield a centre-darkened region which once was the jet. Before the vistigal jet has completely emptied into the southern hotspot, the new jet is launched. As in Jack and my paper, this jet is launched into a rarefied, hot medium, with a high sound speed. The new jet may even be ballistic (denser than its immediate surroundings). Remember, its ambient is the old jet stuff - hotter and more rarefied for having passed thru the working surface. The new ballstic jet is not slowed (much) by the ambient, rendering a weak jet shock and a very bright (Doppler boosted) jet. Observationally, a weak jet shock may be supported by the fact that the tip of the jet isn't all *that* much brighter than the rest of the jet - at least not orders of magnitude (or is it? I forget what the new data say). Presumably the Mach disc is strong enough that on the CJ side, the tip slows enough to become visible.

Allow me the occasional "yes but..." if I feel the compression model deserves another gasp of breath here and there, but at this point, I see the above scenario as being quite inviting.

To answer Alan's other question directly, yes, I see X-shocks as a ubiquitous feature to be in trouble. I should point out, though, the same simulations show that terminal Mach discs are often not seen either. Instead, 3D jets seem to end in a series of oblique shocks. This may be telling us that we are not in the correct Mach number regime, and the Mach number which restores the integrity of Mach discs (if that is desirable) may also restore X-shocks. The jury is still out on that one.

You guys got a good sense of humour? Hope so, cause if my "revelations" are right, I could have been leading us all on a wild goose chase!

Cheers, David.

From root Wed Jul 15 17:34:02 1992
From: rperley@sechelt.AOC.NRAO.EDU (Rick Perley)
To: dclarke@ncsa.uiuc.edu
Cc: abridle@sechelt.aoc.nrao.edu, ccarilli@sechelt.aoc.nrao.edu
Subject: Re: Some More Ideas...
Date: Wed, 15 Jul 92 15:39:32 MDT

About Cyg A, I meant that if the thermal matter in the lobes was at the same density as the ambient, or anywhere close to it, the depolarization would be complete. (Remember, the ambient density around Cyg A is higher than 10**-2 /cc.) The best estimates of the upper limit is about 10**-4 or 10**-5. If it were that high, the thermal gas would easily dominate the dynamics. But of course it could be MUCH lower, so far as the observations tell us. Perhaps there are good theoretical reasons it can't be, but if so, I don't know them.

Yup, the bright tips on both jets is a challenge to the BA++ model. (Excuse the bad pun). It's tough to arrange the jets to be relativistically boosted, and have the hot tips leading the way but NOT be boosted.

Gotta Pack!

Rick

From root Wed Jul 15 16:12:32 1992
From: dclarke@ncsa.uiuc.edu (David Clarke)
To: rperley@sechelt.AOC.NRAO.EDU
Cc: abridle@polaris.cv.nrao.edu, ccarilli@sechelt.aoc.nrao.edu,
dclarke@ncsa.uiuc.edu
Subject: Re: $0.1 worth
Date: Wed, 15 Jul 92 15:17:46 CDT

Hey, this is getting fun!! Welcome aboard Chris!!

I'd like to clarify one of Alan's interpretations of what I said about bow shocks and how bright they should appear. I'm not suggesting that the bow shock apex and the Mach disc should be equally bright, even if the two stand-off shocks are of equal strength. Given two stand off shocks of equal strength (defined by the pressure jump across them), this should cause, roughly speaking, similar emissivity *enhancements*. So, suppose the average brightness of the jet is 10 mJy per beam, but the tip is at 30 mJy per beam. Next suppose the average cocoon emission is at 1 mJy per beam. Then if we are to interpret the bright tippy-tip (I *dare* you guys to use that phrase in the next paper!!) as a terminal Mach disc, then it is responsible for a factor of three enhancement of the unshocked jet emission. Thus, I would expect to find a bow-shock feature leading the jet with a factor of three enhancement over its local unshocked emission, thus 3 mJy per beam. Does that jibe with what you understood of our discussions last November Alan?

Chris: I actually, more or less whimsically, suggested to Rick and Alan several e-mails ago that the rim of what we used to call the 219 jet could be the bow shock of the new jet deeply embedded and still way unresolved. Such a narrow opening angle (essentially parallel) would imply a whopping Mach number - ie hundreds, perhaps thousands. That may or may not be a problem - we know from simulations that the ram pressure of such hypersonic jets generate enormous overpressures in the jet. As Alan stated, there is no reason yet to believe that the jet is *that* over-pressured, if at all.

Rick's suggestion to watch for motions at the tip is intriguing, and naturally I would give a thunderous endorsement to the notion. I would caution though that a forward motion could be interpreted as the advancing jet tip, *or* the natural fluctuation of the position of where the jet intensity falls off. These internal shocks are not static - they wobble about going forward, then backward. On the other hand, *backward* motion may be hard to explain with a BA model, but is quite consistent with the jostling of the criss-cross shocks presumably responsible for the emissivity fall off in the passive field model.

Cheers, David.

From abridle Wed Jul 15 13:35:59 1992
From: abridle (Alan Bridle)
To: rperley, dclarke@fermi.ncsa.uiuc.edu
Subject: $0.1 worth
Date: Wed, 15 Jul 92 13:35:52 -0400

I've been massively distracted (new student getting started on huge data reduction) since you guys started your discussion re bow shocks, hot knots and jet models, so have just been scanning your comments to get some sense of where you're aiming at rather than adding my own on a daily basis. But just for the record:

1) I bought David's basic arguments about equality of the shock strengths a long time ago. As he says, they come from some very fundamental physics. It seems to me that the issue of whether or not we *see* the bow shock depends not on whether it's present but on the content of the medium it's traveling in. Specifically, are there any ways in which it can be pushing through a medium with fewer relativistic particles, or weaker perpendicular fields, and thus provide a much smaller emissivity? Or (I suppose) are there any directional effects that beam the in-jet shock emission at us but the bow shock away from us? My thoughts have mostly been about the first alternative, i.e. that the medium ahead of the BA jet is now depleted in relativistic particles relative to the BA jet. This is why I was asking about channel closing and whether David thought we could simply have fewer relativistic particles ahead of the jet than within it by waiting long enough. If not, then maybe we have more relativistic particles in the BA jet intrinsically. This would be ad hoc, unless we could find a reason why a freshened-up jet, freshly shocked by reopening its cavity, should be hotter than its predecssor).

2) Confinement. I didn't think the interknot parts of 3C219 jet were too bright to be confined by a plausible external medium so long as it was circumgalactic rather than galactic. That was the conclusion from the previous high-resolution image, and I'm supposing we've now stuffed more of the flux density into more compact features, leaving the truly extended residue even easier to confine. My own interpretation of the cylindrical sides would have been that they confirm that (*if* the jet *is* externally confined), then the confining scale must indeed be a long one. This again favors it being an ICM rather than the ISM of 3C219.

3) The "cocoon". David's got a good point here, we have yet another quasi-cylinder surrounding the jet, and the relativistic particles can't possibly be in pressure balance alone in the cylindrical jet, the cocoon and the lobe. We have not, of course, done the crucial observational test, which is to see if we can detect the cocoon by slice integrations *past the end of the jet*. I.e., we don't know if the cocoon also appears to stop where the jet does, or whether it continues.

4) When you guys get time, I think we should all look at the summed 22cm and 18cm images. The "L Band sum" actually gives us our most sensitive look at the lobe structures, and on that image some of the "filamentary things" we've glimpsed before look a little more like they may add up to an edge-brightened extension of the jet beyond the vanishing point. I'm not completely convinced either way, but

I do feel that there's some extra "signal" in there that we haven't fully used in our attempt to decide between the models.

5) I basically agree with David that both models still have their problems. I saw part of the purpose of the "Charlottesville" draft of the JB paper to be to provoke David into some quantitative statements about a) how fast a dropoff the decompression model can stand, b) what ratios govern the ratio of emissivities (not schok strengths) of the jet shock and bow shock and now (c) how tiny a hot knot can be before we need to talk about a subjet to feed it. We're started on all three of these, but I think we're nowehere near finished, so the presentation at Jodrell *has* to talk about pros and cons of both types of model.

I'd still like to emphasize the observational result that the tippy-tip of the counterjet now looks remarkably like the tippy-tip of the main jet, in both compactness and flux density, as this is the really new ingredient that came from high resolution. This identity was *required* by the BA model, and helps keep it well strapped together, but there may be a way to get it out of the other model too when we've thought longer about it. But it's a strong new observational fact about 3C219 that won't go away, and these are always the best things to emphasize at observation-oriented conferences.

I look forward to straightening all this out later in the year. But I agree with David that the talk cannot say we have killed off one model, it should simply itemize the things that agree and disagree with each of the models. I think the observations are, as usual, running a little ahead of the ability of *any* model to account for them fully!

A.

From root Wed Jul 15 14:01:32 1992
From: rperley@sechelt.AOC.NRAO.EDU (Rick Perley)
To: abridle@polaris.cv.nrao.edu
Cc: ccarilli@sechelt.aoc.nrao.edu, dclarke@ncsa.uiuc.edu
Subject: Re: $0.1 worth
Date: Wed, 15 Jul 92 12:06:58 MDT

There's one more thing to think about. Another outgrowth of the internal talk I gave was a suggestion by Joan about using the VLBA (or VLA, even) to track the growth of the main jet. This is not such an outlandish idea!

suppose the jet really is propagating into a underdense medium. With a relativistic jet, we can expect the leading edge to be pushing forward very quickly, probably relativistically. since our tippy-tip is very small and bright, we might well be able to follow its motion, probably best with the VLBA at 20cm. I haven't put any numbers in here, but the idea is worth throwing around.

It also occurs to me that motion of the tip would strongly favor the BA model, and provide even more difficulties to the 'expanding jet' model.

Rick

From root Tue Jul 14 16:41:35 1992
From: rperley@sechelt.AOC.NRAO.EDU (Rick Perley)
To: dclarke@ncsa.uiuc.edu
Cc: abridle@sechelt.aoc.nrao.edu
Subject: Hey, No Sweat!
Date: Tue, 14 Jul 92 14:47:03 MDT

Just a quick reply for now, I'm juggling a number of balls at once.

1) You haven't told me why I should truly believe that jets are internally supersonic. It seems to me, on general grounds, that they must be, since this is easily the best way to deliver lots of energy and momentum with minimal associated problems, such as stability and confinement. (We needn't debate this one, I'm not about to argue that jets are composed of slow-moving rocks).

2) As for overpressures -- I'm not so sure about hiding behind the usual defence that these are due to local, unresolved enhancements. Certainly, knot 'A' in Virgo A is a local enhancement, and advocates of overpressure are clearly out on a limb in using this as 'proof' of the need for currents, or unseen pressures. However, in Virgo A, the entire inner jet, which has much smoother emission is 'overpressured' as well. As for 3C219, the entire jet is too bright for the confining medium. Suppose, as I advocate, that the lobes contain *no* thermal gas (or no dynamically important thermal gas). The processes which create the lobes have effectively evacuated the entire region. Then, unless the lobe material surrounding the jet is *VASTLY* out of equipartition, we have a serious problem with jet confinement. The entire 3C219 jet is too bright! This is not a 'local' problem, but one which extends for the entire length of the jet. It is not just a 'local hot spot' problem, but one which exists for every place we can see jet emission. I'll gladly grant you that hotspots and bumps are local phenomena. My worry is that all regions of the visible jet share the problem. This makes the problem global, in my thinking.

I thought not?

3) I didn't claim (or didn't think I claimed) that the straight sides implied out-of-pressure equilibrium conditions. It says to me what it apparently says to you -- that there is something quite strong keeping the jet in pressure equilibrium. I'm not sure I know what the 'something' is.

4) My 'cylinder' idea is not a wonderful one -- if the interior of radio lobes are empty, what is left to confine the jet? Another problem...

5) I'm not avoiding the 'bow shock' problem. In fact, I intend to highlight it! At the Monday afternoon 'Science Break' (where I droned on for an hour), I repeated my worries about this, over, and over, and over, and... I even begged the audience for ideas! (I didn't get any, by the way).

So we are in good agreement on the content of the Jodrell talk. I'm quite happy to modify the outline in accordance with your two points:

1) I'll remove 'kill', and substitute 'challenge' (or words to that effect).

2) I'll worry out loud, and in public, about the lack of a bow shock.

How's that?

Rick

From root Tue Jul 14 17:53:16 1992
From: dclarke@ncsa.uiuc.edu (David Clarke)
To: rperley@sechelt.AOC.NRAO.EDU
Cc: abridle@polaris.cv.nrao.edu, dclarke@ncsa.uiuc.edu
Subject: Re: Hey, No Sweat!
Date: Tue, 14 Jul 92 16:58:44 CDT

Sounds great Rick ol' bean.

By the way, have you ever worked out the numbers (ie the depol and RM) that would result if there were enough thermal matter mixed in with the radio plasma in the lobe (of 219 for example) to confine the jet? Up to now, I have thought of your outright rejection of a thermal component as being one of your biases. But your continued assult on the idea is making me think that maybe you have some quantitative reasons for not believing in thermal gas. Have you? Larry Rudnick, for example, doesn't understand your objections to a thermal component, and in facts advocates that all radio sources are made up *primarily* of thermal matter, with the relativistic component representing a loud ('cause we see it) minority of the stuff that's actually there. If you have worked out the numbers, could you reproduce them for me, either by e-mail or by US-mail? I would like to try to understand the two sides of the controversy better.

Also, it seems to me that the "too bright" radio jet in 219 *cries out* for a dynamically important thermal gas component, dudn' it? As you say, otherwise we have a problem.

Have a blast in jolly ol' England - a chance to escape the heat!!

Cheers, David.

From root Tue Jul 14 18:40:43 1992
From: rperley@sechelt.AOC.NRAO.EDU (Rick Perley)
To: dclarke@ncsa.uiuc.edu
Cc: abridle@sechelt.aoc.nrao.edu
Subject: Re: Hey, No Sweat!
Date: Tue, 14 Jul 92 16:46:09 MDT

```
        My continued attacks on thermal matter are based on pure prejudice!  The
only limits that can be cited are the standard ones of 10**-5 (cgs) which come
from depolarization studies.  Obviously, if the thermal matter is even present
at the 10**-9 level, it will still provide most of the pressure.  But there is
no reason not to suppose, or at least conjecture, that it's not there at all!
Is there?  Another line of argument might indicate that the thermal pressures ar
e
low -- in Cygnus A, the 'equipartition pressure' of the lobes is about the same
as the thermal pressure of the cloaking gas.  Here, for sure, we can state that
the action of  the jet and lobes has been to evacuate the region that the lobes
currently occupy.  For if this were not so, the depolarization of the lobes
would be total and complete.  Yet, there is no measureable depolarization of
these lobes.  The density of the lobes is no more than 1/1000 of the outside
gas.  The 'cleansing' action of athe jet/hotspots, etc. is pretty darn good!
So I merely extend this argument in two ways:  If the ratio is < 10**-3 for
Cygnus A, why can't it be 10**-6?  Or less?  And, if this can be admitted for
Cygnus A, why not for all luminous extragalactic radio sources?
        Now, for FR1s, and head-tails, the situation is probably different.  Her
e
I will grant you that entrainment is probably important.  I note that for these
object, the 'equipartition' pressures are too low, meaning that either
equipartition doesn't hold (which is the cheap way out), or that another source
of pressure is present -- an entrained thermal component is quite likely, given
that H-T sources are in clusters.  I think this is a prime problem for observati
ons
-- nobody has done a careful depolarization study with the VLA of these objects,

so far as I know.
        Now, for the 3C219 jet's confinement problem -- there may well be a prob
lem!
But some more information should help -- Leahy has gotten (I think) extensive RO
SAT
time on 3C219.  PErhaps when that's in, we can better define the problem, if the
re
is one.

        Thanks for the debate.  This has helped a whole lot.  BAsed on the
enthusiastic response to my 'preliminary talk' given here yesterday, the real
talk in England should go over pretty well.

        Rick
```

From root Mon Jul 13 15:10:52 1992
From: dclarke@ncsa.uiuc.edu (David Clarke)
To: rperley@sechelt.AOC.NRAO.EDU
Cc: abridle@polaris.cv.nrao.edu, dclarke@ncsa.uiuc.edu
Subject: Re: My long–promised response –– somewhat muted.
Date: Mon, 13 Jul 92 14:16:12 CDT

Rick.

I spent the whole morning going over the jump conditions and what they look like in various reference frames just to be sure I wasn't feeding you a line of bull. I think I have things straight enough in my head to comment correctly on all your concerns. So here goes.

```
>       O.K.  I admit it.  Under the assumptions your have made, your
>conclusion appears inescapable:  A propagating jet which is internally
>supersonic must drive a bowshock into the surrounding medium.  I put
>'internally' in to distinguish between a jet which is subsonic w.r.t. its
>own sound speed, but supersonic w.r.t. the outside medium (or vice versa).
>It's easy to get a jet which is externally supersonic to advance subsonically
>-- but if I understand this correctly, this jet cannot be internally
>supersonic, and thus cannot have a 'Mach Disk'.  Right?
```

Yes, that sounds right.

```
>      Now, let's go through the assumptions, as I see them.  They are:
>        1) That the jet is internally supersonic.
>        2) That the jet is underdense, (epsilon < 1).
>        3) That the jet is in pressure balance.
>        If any one of these is not true, I think we can lose the advance
>bow shock.  Let's now discuss these assumptions.
```

I disagree with this. The first one yes. The second one in as much as the 1/(1+sqrt(epsilon)) factor is less than one - yes. But as you point out below, for "realistic" jets, this factor is very nearly one and doesn't buy you much. As I mentioned in my previous e-mail, my assertions are rather independent of whether the jet is in pressure balance. I assumed pressure balance only to make my e-mail equations readable.

```
>
>        1) Internally supersonic.  How do we know this to be true?  By the
>hot tip?  but perhaps this is merely a mild compression, enhancing the
>emissivity.  Perhaps it's a solid rock, emi% of
thtting particles and fields.
>(I don't advocate this, but put it in merely to point out how little we
>REALLY know about the nature of the tip emission).  If the jet is comprised
>of super-hot, relativistic material (which I DO believe), the jet velocities
>could easily be relativistic, but still subsonic.
>        2) The jet is underdense.  This I do truly believe.  I think we get
>into difficult energetics if we were to advocate overdense jets.  Besides,
>simulations of overdense jets look manifestly unlike real radio sources.  So
>I don't think we can escape our bow shock problem by advocating jets
>comprised of ball bearings.
>        3) Pressure equilibrium.  This one is interesting.  Unlike you, I
>don't necessarily believe the jet is in simple pressure equilibrium with
>the outside.  (This linearity of the edges in 3C219, and especially in
>Virgo A, despite internal changes in emissivity, leads be to believe (hope?)
>that pressure balance is not made).  Suppose the jet pressure is a factor
>kappa different than the lobe pressure.  Kappa = Pjet/Plobe.  Then, the
>ratio of sound speeds becomes:  c(jet)/c(lobe) = sqrt(kappa/epsilon).
```

```
>Putting this into the general advance speed formula:
>        M(hs) = M(jet)*c(jet)/c(lobe)*sqrt(epsilon)/(1+sqrt(epsilon))
>gives:
>        M(hs) = M(jet)*sqrt(kappa)/(1+sqrt(epsilon)).
>
>        Unfortunately, getting a subsonic hotspot this way requires kappa
>to be much less than one -- i.e., a greatly underpressured jet.  This
>doesn't square too well with the enhanced emissivity, or the linear walls.
>Perhaps I've got something inverted.  Indeed, if we take the conventional
>view that the jet is overpressured, it then appears that the advance speed,
>in units of the lobe sound speed, will be increased, making my problem even
>worse.
```

I think this reasoning is flawed on two grounds. The first problem is a general one. I don't think anyone believes that local pressure imbalances are important in the "confinement" issue. You show me a feature with a minimum pressure much greater than the inferred ambient (from X-rays), and I'll show you a very compact feature, whose dimensions are << the resolution of the X-ray observations. So, I couldn't care less about minimum pressure arguments of *compact* features - these could well be transients, and say nothing of the big picture. Folks like Mitch Begelman have been pointing this out for years.

The second problem is mathematical. Your "general advance speed formula" is not at all general. You've merely used the formula derived *assuming* pressure balance in the first place, then gone on to "generalise" by plugging in a non-unity kappa. Instead, start with Newton's third law, and take it from there. In the frame of reference of the advancing working surface, force balance requires that the *sum* of thermal pressure and ram pressure be the same (for no acceleration).

```
      P_jet + rho_jet * v_jet**2 = P_lobe + rho_lobe * v_lobe**2
```

Transforming to the lab frame (quiescent ambient):

```
      P_jet + rho_jet * (v_jet - v_ws)**2 = P_lobe + rho_lobe * v_ws**2     (1)
```

where v_ws is the working surface velocity. To get your expressions above, you need to equate P_jet and P_lobe. Can't do that. Instead, plug and chug (1) to get:

```
          eps * v_j - sqrt [ eps * v_j**2 - (kappa-1) * (eps-1) * P_l / rho_l ]
   v_ws = ---------------------------------------------------------------------
                                   eps - 1
```

where I have abbreviated "epsilon" to "eps", "jet" to "j", and "lob" to "l". Now, the sound speed in lobe = sqrt [gamma * P_l / rho_l]. Thus,

```
          eps * M_j - sqrt [ eps * M_j**2 - (kappa-1) * (eps-1) / gamma ]
   M_ws = ---------------------------------------------------------------
                               eps - 1
```

where M_ws and M_j are respectively the Mach numbers of the working surface and jet wrt the lobe sound speed. This is the correct (under simple 1-D assumptions

mind you!) expression for the advance speed given lack of pressure balance.

But my claim is that this is all irrelavant. The advance speed must be determined from a free body diagram, if you will, of the goings-on at the working surface. This will include the thermal pressure, ram pressure, microphysical processes, other pressures such as radiation and magnetic, and even the fact that in 3-D, the effective working surface that the ambient works on is different from that which the jet works on. The truly general problem is very complicated. These factors will all have effects of order unity (a few to one over a few) to this total balance. Experimentally and numerically, if there are shocks in the jet, there will be shocks in the ambient, and vice versa. You may be able to tweak the parameters and teeter the experiment on some precarious line on which a weak shock may exist in one medium and not in the other. But to invoke a strong shock in the jet and ignore outright a bow shock in the ambient is unphysical.

By the way, why do you think that the *parallel* straight edges of the 219 jet implies out-of-pressurre balance? To me this is the strongest observational evidence that the 219 jet is *overall* in pressure balance with its surroundings.

Surely it means we have pressure balance with a slowly varying pressure as in a cluster IGM rather than an ISM?

>
> So, unless one of you can find an error in my analysis, I think we
>have to retreat to one of these alternatives, if we are to sustain the idea
>that the jet is opening up a new channel:
> 1) The jet is not internally supersonic, so the 'knots' within them,
>or at the tip, are not shocks at all. I don't like this idea too much, but
>would be interested in hearing your comments.

How do you maintain collimation and definition over such enormous distances (beating entrainment tendancies) without invoking supersonic flow? Next, how do you stop a supersonic, relativistic jet (so you see the counter-jet tip, for example), without a shock?

> 2) That there is no emitting material in front of our putative
>restarting jet. Either the channel stays open (somehow) and is empty (somehow)
>or the interior of this source is relatively empty. (How then is the jet
>bounded, you ask? By some magical means, I reply). Note that the lobes
>don;t need to be thin shells. David has rejected this hypothesis on the
>grounds that the lobes are not center brightened. Indeed, they aren't, but
>they don't have to be, either. If the lobes are thick shells, say, 50% of
>the full width, then there is no appreciable edge brightening. There will
>be some center dimming, but I'm sure I can find a way to make this small.

True. A thick cylinder may work. But then one has the problem with likelihood. I mean a truly thin shell implies some extreme - one effect is clearly winning out over the others. A completely filled shell implies the other extreme - that the effects which would maintain a thin shell are ignorable. To get a shell that is not completely hollow asks for a possibly precarious (and I would claim unstable) balance of the competing thin vs thick forces - something I find unsavoury to say the least. Free fluids *always* find some way to exploit instabilities - Clarke's first law. At any rate, I find that the fact that Rick is having to devise "retreats" from the bow shock problem argues strongly that the lack of a bow shock be at least mentioned at the Jodrell meeting - unless one of you can come up with an iron-clad anti bow shock argument. This includes any future publications we make. So, to get back to my original complaints with Rick's Jodrell outline:

1. I am *not* willing to state that the present data "kill" the "passive field" model. I *am* willing to state that they discourage it.

2. I should think that in scientific fairness, we should state that the lack of a bow shock discourages the restarting jet model.

Other than that - if you want to pursue your jet-in-a-bubble model, I yield to those who have thought more about it!!

Cheers, David.

From root Thu Jul 9 18:48:14 1992
From: rperley@sechelt.AOC.NRAO.EDU (Rick Perley)
To: abridle@sechelt.aoc.nrao.edu
Subject: Here it is...
Date: Thu, 9 Jul 92 16:53:44 MDT

>From dclarke@ncsa.uiuc.edu Wed Jul 1 12:09:55 1992
Return-Path: <dclarke@ncsa.uiuc.edu>
Date: Wed, 1 Jul 92 13:09:54 CDT
From: dclarke@ncsa.uiuc.edu (David Clarke)
To: rperley@sechelt.AOC.NRAO.EDU
Subject: Eagerly awaited comments.
Cc: dclarke@ncsa.uiuc.edu
Status: RO

Hi Rick;

Thanks for your thoughts.

First, I need to explain more fully what my "bow-shock thing" is. Let me try to put some numbers on it. For the moment, let us consider the BA model in which what we thought was a jet is a jet, rather than a lobe-in-the-making. There is *no* evidence that this jet is expanding (other than inside the "gap"). In the absence of magnetic confinement, all our simulations tell us that this requires that the jet and ambient be in thermal pressure balance. OK, suppose the ambient density is 1, the ambient sound speed (C_amb) is 1, and the density ratio between the jet and ambient (eta) is 0.01. Whatever units you like. Suppose further that the jet is travelling at Mach 10 relative to its own sound speed. The question is, what are the strengths of the terminal Mach disc and of the Bow shock excited in the ambient, and can we get a Mach disc without forming a bow shock?

The working surface of the jet will advance at a speed governed by the balance of ram-pressures. Thus,

    V_ws = V_jet * sqrt(eta) / (1.0 + sqrt(eta))

Now the jet speed is simply 10 * C_jet = 10 * C_amb / sqrt(eta) (assuming pressure balance between the ambient and the jet as required by the observations). In our units, C_amb = 1.0. Thus V_jet = 10 * 1 / sqrt(0.01) = 100. Thus, V_ws = 100 * sqrt(0.01) / (1.0 + sqrt(0.01)) = 9.1, which is greater than unity - the sound speed in the ambient. Thus, the working surface will advance into the ambient medium supoersonically relative to the ambient sound speed, thereby exciting a bow shock. This is a specific example of a general theorem:

> A supersonic jet pushing into an ambient medium will drive two shocks, one in the jet and one in the ambient medium, of equal "strength", where strength is defined by the ratio of post-shock and pre-shock pressures.

This is inescapable. It is basically the manifestation of Newton's third law to jet-ambient dynamics. You can't have a supersonic jet ending in a Mach disc without it exciting a bow shock in the ambient at the same time any more than you can push on something without it pushing back. This is even true for jets out of pressure balance, but the situation is sufficiently complicated that it is harder to demonstrate on the back of an e-mail envelope! Further, it is true for relativistic flows - again adding complexity to the numbers.

So whether the sound speed in the ambient is 1 cm/sec or light speed, it won't matter - if the jet ends in a Mach disc, there will be a bow shock of comparable strength in the ambient. Turning this around, if the ambient sound speed is c/2 as you suggested, and the working surface advances less than this, then the jet is necessarily subsonic to its own sound speed, and no Mach disc will exist.

So no, I don't like this argument on the grounds that it violates Newton's third law. As for the empty lobe hypothesis, I don't like that any better, but not for anything so fundamental as Newton's Third Law. The lobe doesn't look empty to me!! The 219 lobes are *not* edge brightened as one would expect from an empty lobe model. Even the hot spots, especially the southern one, are more interior than many FR II's. I don't think this is safe ground at all to retreat to.

I regard the lack of a bow shock to be something which flaws the BA model as much if not more than the sharp cutoff flaws the passive field model. So while I am all for harping on the sharpness of the cutoff - it can't be ignored - I think we should pay as much attention to the bow shock problem. Hell, if I'm willing to supply you with the coffin nails for the passive field model (my report that criss-cross shocks may be passe for example), then I would hope you guys at least would acknowledge problems with your BA baby as well.

As far as our inability to get criss-cross shocks in 3D, certainly you may mention this at Jodrell, but please emphasise that these are preliminary results. We have only begun 3-D in earnest, and so this is only a possibility at this point. Realise too that the lack of criss-cross shocks has implications far beyond the passive field model. Up to now, most folks have been quite happy to regard these as the origins of jet knots. Without criss-cross shocks, we're back to square one. So I don't want to scare a bunch a people or cause them to go off running half-cocked with a preliminary result. Please, if you are going to mention this (to help kill the passive field model, I would presume), emphasise that this is an early trend, and that it is quite possible that there may be a parameter regime, other than the magnetically confined one, in which 3D criss-cross shocks are stable. There may still be numerical reasons why we have been unable to generate a stable series of criss-cross shocks in 3-D. We just don't know.

Re your inner/outer jet model, I still find it hard to swallow that these limited data buy you enough to advocate a whole different picture of jet dynamics. Nevertheless, were it the case, then the scales are much smaller, and one might interpret the entire outer periphery of what we once called the jet as the bow shock in the ambient, with the apex of the bow shock and the Mach disc still unresolved as one bright feature. Still, I have a problem with the scale. Even with the BA jet model, the distance between the core and the jet tip is about 80 jet radii (using r_jet = 0.25"). Between the core and the southern hot spot is about 280 jet radii. I don't know anyone who has demonstrated jet stability over such distances without invoking ultra-sonic velocities. Then, to ask for an even smaller diameter jet inside your forming lobe would push this point beyond credibility, in my opinion. I mean, you're asking for core-hotspot distances in excess of 1000 jet radii. This represents an enormous theoretical problem.

Now, as for what Larry is doing. When you plot the various spectra of separate points over Cyg A and compare these to various emission laws (like I seem to recall Chris did), then yes, the fit to the J-P model looks convincing. But that is not all that the J-P model must do. You could go one step further and ask what type of colour-colour plot would a J-P model generate. Basically, all you need to do is draw an ideal JP model, extract from it adjacent spectral slopes, and plot one slope vs the other. These give you a locus of points which make a well-defined curve on an alpha^6_20 vs alpha^20_90 plot, for example. The Cyg A data do *not* track this locus of points. I think Larry would disagree with you that a JP model could be a subset of what is going on. But I shouldn't put words into his mouth - you might want to touch bases with him on his latest results. And yes, I think your attitude that the spectral gradient away from the jet tip is indicative that the tip is special, without necessarily attributing it all to particle re-aceleration, is reasonable. I would agree that the spectral data do inidcate something is different at the tip of the jet than in the upstream (downstream) plasma for the jet (forming lobe) model.

I think this addresses all your comments to my comments. Let me know how you and Alan decide to resolve this bow-shock thing. As far as the forming-lobe model is concerned, you guys have looked at the data far longer than I. If you think it's a goer, then go for it.

Best wishes, David

From root Wed Jul 1 11:43:46 1992
From: rperley@sechelt.AOC.NRAO.EDU (Rick Perley)
To: dclarke@ncsa.uiuc.edu
Cc: abridle@sechelt.aoc.nrao.edu, jburns@nmsu.edu
Subject: Comments on your comments.
Date: Wed, 1 Jul 92 09:48:53 MDT

Hi David. I have emerged briefly from administrative-land, to get some air. Here are my comments.

I agree that 'killing' the decompression model may be a little too strong. No telling what you simulators might come up with next. With regards to your interesting new information about the lack of criss-cross shocks in 3-D simulations, are you comfortable with the idea that I mention this during the talk? Or would you prefer I keep mum on the subject, pending further work by you and Mike?

Now, for the bow-shock problem. Suppose, for sake of argument, that the lobes of these radio sources is filled with nothing but relativistic particles and magnetic fields. A fully relativistic gas. Nothing in the observations precludes this , for FRII objects. (I would agree that for FRIs, thermal material very likely has leaked, or been dragged, in). Then, the sound speed is of order c/2, so that provided the jet tip is advancing at a rate less than this, there won't be any bow shock at all!

If you don't like this argument, I'll retreat to another defensive position: Suppose the interiors of our radio sources are essentially empty, and that the emission we do see is dominated by surface emission. There is a school of thought around here (headed by FNO) that truly believes this is the case for jets, and although it is probably much to dangerous a leap to extend this to lobes, I'll do it anyway. Note that I don't actually require the interiors to be truly empty, only that the emissivity be very low, for whatever reason. We could probably dream up a few semi-plausible ones.

On to our inner/outer jet. First, I wince at your comparison of what Alan and I suggested to a 'stellar-type' model. It is very likely that we are in a totally different regime of physics, so I don't want to have my thinking (such as it is) influenced by the comparatively mild processes that go on in those flows. (Besides, I am very envious of all the real numbers they have to narrow their range of models with). Perhaps what influenced us the most, in advocating the inner jet model, is how the jet looks like a miniature lobe! Although there are differences (the linearity, and the straight sides being the most obvious), the narrow tip and wider base of the jet really is reminiscent of what FRII lobes look like. So if we all believe the hotspots of these lobes is due to a collimated, efficent flow, why not the same for the jet that we are resolving? Is this a lobe-in-the-making? Note that the NE lobe has the appearance of a superposition of three separate semi-spherical lobes. Could each represent a separate 'event'? Perhaps the SW lobe is different because the events were all more colinear, or that the environment better superposed the events.

Finally, on the spectral gradients. I have yet to see what Larry is doing, although I certainly have heard about it. I can't agree with your statement of Larry's work -- that his two-color plots don't support Chris' conclusions. Chris fitted true synchrotron evolution models to multi-frequency data, and came with a definitive conclusion. This, however, doesn't mean that his conclusions are the only ones possible, as he left out other physical processes. Including the host of missing processes will certainly alter the conclusions. Chris' conclusions aren't wrong, until one can show that the missing processes are truly present in the real source. I doubt Larry can show this, but I'm ready to be convinced otherwise. Now, as for our application of old, basic ideas to 3C219, I believe the following: Although we can't prove particle acceleration is going on at the tip of the jet, we can certainly say that particle compression, and field enhancement is going on there, relative to what is going on throughout the rest of the jet. The steepening that is seen in the jets (and lobes) certainly represents expansion, both in the standard synchrotron models, and in Larry's (presumed) new approach. So I think our interpretation of what's going on in the jet can still be defended.

I eagerly await your comments.

Regards, Rick

From root Sun Jun 28 13:54:28 1992
From: dclarke@ncsa.uiuc.edu (David Clarke)
To: rperley@sechelt.AOC.NRAO.EDU
Cc: abridle@sechelt.AOC.NRAO.EDU, dclarke@ncsa.uiuc.edu, jburns@nmsu.edu
Subject: Re: New Outline of SARA Talk. Please comment
Date: Sun, 28 Jun 92 12:59:16 CDT

Rick:

Thanks for your draft. I, of course, have a few comments.

Overall, I think it is good, but I think a few points need to be reconsid
First off, I am delighted that you have reduced the X-band data and are preparing this presentation for the Jodrell meeting. Thanks. Now onto my specific comments, most of which are in the spirit of what I perceive as be "fair" to the two models.

1. In segment I, I would add before the last line:

   -Models differ in their predictions regarding a bow shock.

2. In segment III, I would add after point 1:

   1a) No detectable bow shock leading either jet tip.

3. - Super-abrupt emission drop kills (?) the field decompression model. It is inconceivable that the expansion could quench the emission as abruptly as is observed.

OK, you got my attention. Of course, we don't *know* what drop should exist with all the micro-physics or with the proper combination of geometry, Mach number, beaming effects, etc. We only know that in one low-resolution simulation which lives in one point in a limited parameter space, such a trend, though not as severe, has been reproduced. Let's not expect too much out of a rather simple model. I would agree that such a drop has not been reproduced, but certainly no one can say that it is *impossible*. "Kills" is too strong. I would concede that these data discourage this model, but the death-blow has yet to be wielded.

4. In our rush to bolster up the BA model, let us not forget that there are still no signs, not even a smidgen, of a bow-shock. Alan asked me what about the thermal material closing in on the cavity? My response to that is: What then is doing the emitting in the extensive cocoon? My argument has always been that there should be a jump in emissivity across the apex of the bow shock in the cocoon comparable to the jump in emissivity at the leading tip of the jet, and that the two features ought to have similar compactness. For a propagating jet in a synchrotron-emitting plasma, I don't see how you can get around this. This point has not even been paid lip-service in your outline, and I think this is an over-sight. Unless you have a way around it I haven't thought of??
In the event that you do not, I would add after your "Super-abrupt emission drop kills (?) ..." point the following:

   - lack of bow-shock is consistent with decompression model, inconsistent with BA model.

5. I suppose I would need to see the data again, so perhaps this question is naive, but do we really need to invoke a stellar-type jet model ("second wind" as Bo Riepurth calls it) for extragalactic jets? Is this actually called for by the new data, or is this just speculation on your and Alan's part? No one that I know of has demonstrated that there is a lower limit to the ratio between

the hot spot diameter and jet length, or even the hot spot and jet diameters. Is there a problem with a hot spot diameter that is smaller by a factor of a few than the overall jet diameter? I am not aware of one. Particularly when one realises that the synchrotron emissivity is a *very* sensitive function of the expansion of the fluid, perhaps the hotspot represents only the most compressed portion of the shocked meterial.

Now, let me mention two things that I think are new, that you may not be aware of.

1. In the few 3-D models that I, Mike, Dinshaw Balsara, and Jim Stone have done, we have not yet demonstrated that the criss-cross shock pattern carries over to 3-D. I should caveat this by saying that when the jet is magnetically confined, the criss-cross shocks are stable. But hydrodynamically, they seem to be an artefact of 2-D axisymmetry. I wouldn't have said this as little as two weeks ago bacause none of our simulations had reporduced the numerical resolution of the 2-D computations that Mike and company did 10 years ago. But two weeks ago, Mike showed me a 3-D PPM simulation that he and Jim Stone did with comparable resolution across the jet as the 2-D simulations had (40 Million zones in all) and still no criss-cross shocks. The death-blow to the decompression model, as you call it, may not come from the observations, but from the same mother that bore it in the first place! This result is subject to revisal. We may find that there is a well defined parameter space besides the magnetically confined regime in which the bi-conical shocks persist. But at the moment, criss-cross shocks *may* be passe.

!

2. Larry Rudnick was up here two weeks ago, and he has been re-educating me in what we may interpret from spectral index data. In particular, as you probably know, he is working intensively with your and Chris' Cyg A database with the initial intent to determine if there is any evidence of particle reacceleration. It has turned into a project of asides. The latest and most significant is his discovery that a colour-colour diagram (20-90 cm spectral indices plotted against 6-20 cm spectral indices) does not support at all Chris' conclusion that a simple J-P model fits the data. The immediate upshot of this study is the realisation that spectral steepening is by no means a signature of particle aging. For a curved emissivity law (as opposed to a power law for simple synchrotron emission), a (de)compression of the B-field, sheared B-field, and adiabatic expansion to name a few can all put you on a steeper or flatter part of the spectrum without necessitating particle aging or reacceleration. Certainly all of these processes must go on. The point is, it is difficult, if not impossible, to sort out which process is causing what fraction of the observed spectral flattening or steepening. Therefore, to use spectral flattening/steepening as evidence of a strong shock in which in situ re-acceleration is going on may be naive. I am mentioning all this partially to caution against over-interpreting the spectral gradients you are observing along the jet, and also to suggest a possible re-analysis of the 3C219 lobe data (with some additional observations at C-configuration, 6 cm) in light of Larry's discoveries in the Cyg A data.

OK, phew!! That about covers what my thoughts are at this point. Please let me know what your thoughts are on any of these issues. I shan't hold fast to anything here other than to play a more honest broker with what the data tell us about the two models. Good luck - I hope administrivialand hasn't got you too bogged down!!

Best wishes, David.

From abridle Wed Feb 12 14:08:10 1992
From: abridle (Alan Bridle)
To: rperley
Subject: Abstract
Date: Wed, 12 Feb 92 14:08:06 -0500

Here's the tack I would have taken:

===

Recent 8-GHz observations of the partial jet and counterjet in the radio galaxy 3C219 at resolutions of 0.19" and better show much fine structure. The jet consists of a "cylinder" of faint emission in which are embedded a number of undulating filaments and bright knots, or "globules". Both the main jet and counterjet terminate in such bright "globules". These terminal knots are remarkably well-aligned across the radio core, and are very similar in brightness despite the aparently very different lengths and total intensities of the two jets. These relationships are, perhaps surprisingly, consistent with the simplest model of a "born-again" twin relativistic jet with strong deceleration or disordering of the velocity field near the tips of the jets. There is, however, no clear evidence for the "bow shocks" that could corroborate such a model. The magnetic field in the jet appears to be well aligned with the local ridge lines except near the tips of the jets.

===

I have not started typing in my comments on Ilias' draft yet, (got heavily distracted by watching the superb U.Va-U.Md women's basketball game last night) but I see from yours that we are in very substantial agreement. We shall indeed take a stand together!

From root Wed Dec 4 13:10:08 1991
From: dclarke@ncsa.uiuc.edu (David Clarke)
To: abridle@polaris.cv.nrao.edu
Subject: Re: Polarimetry
Date: Wed, 4 Dec 91 12:12:32 CST

Alan.

Thanks for the update. My FAX number is 217 244 2909. Can't think at the moment of any mechanism that would allow for a brighter bow shock than jet shock. A shock of a given strength should generate a feature with a certain *contrast* rather than a specific brightness. And given that the jet overall is brighter than the cocoon, this should yield brighter jet shocks that bow shocks (assuming that the jet shock and bow shock are of comparable strength, as they should be at least to first order). Even in the case of a nose cone, an external bow shock should still be there. Although, let me think about that one.

So I've come across as a restarting jet advocate, have I? That either means I'm fickle, or open minded. Hopefully more of the latter!! Actually, what I got out of working on the 219 epistle is that the two models look very good, but each has a major failing. The restarting jet scenerio seems to require a visible bow shock, and it ain't there. The passive magnetic field model can't seem to be able to kill the jet anywhere nearly as quickly as observed. At the moment, I honestly cannot say which model has the more lethal problem and so I can say that both models are on relatively similar footing for me. That is clearly a change from my position two years ago.

This is all extremely interesting, I look forward to the Faxes and to be able to get at the U band data.

Cheers, David.

From abridle Tue Dec 3 16:03:27 1991
From: abridle (Alan Bridle)
To: dclarke@ncsa.uiuc.edu
Subject: Re: More on 219 X Band
Date: Tue, 3 Dec 91 16:03:21 -0500

Well, I said I'd started on the polarimetry, not that I'd done it! The Q MX has been running all day (since 9.30 am). I'll take a look at it before teeing up the U overnight. If all goes well, I'll have some news for you tomorrow.

Yes, we could have caught 219 at a "convenient" time for the restarting-jet model, especially if it's not really on-off but "loud-soft". But what to say about the lack of a bow shock, though? The end of both jets looks highly "scrunched" now. (I was tempted to FAX you the slice I plotted just to scare the daylights out of you re the comparison with the drop in the passive-field model, but I guess I'll have to hold off now you're becoming the defender among us of the restarting-jet picture!) This gets more and more awkward for both pictures at the same rate, I think - if it's a strong compression we're seeing.

The limb-brightening is definitely not symmetric in the middle of the jet. I.e. it does not look like a double helix all the way. In fact the jet takes a bit of a bend in the middle, and the south limb seems to fade while it does this, leaving the north limb on its own. This is consistent with the lower-resolution data too. We'll need to look at profiles very carefully, though, as there are long-wavelength ripples under the jet that might introduce some bias. I have not yet tried to flat-field the image by excluding the <60 klambda baselines. I wanted to see how far I could get with calibration and CLEANing before using that filter.

I'll basically be able to stay on this until the weekend, then Colin Lonsdale is coming for a week to work on the quasar project paper. I may keep some big sums rolling in the Convex while he's here but won't do anything that requires much interaction until closer to Christmas.

Good luck with the Convex+AIPS. At least it has been done before (they run in a C2 at CSIRO and at UNC) so you know the problem has a solution. As this thing takes a whole working day per MX on the C1 I'd like to have a C2 on it here right now!

Cheers, A.

From abridle Fri Dec 6 15:28:57 1991
From: abridle (Alan Bridle)
To: dclarke@fermi.ncsa.uiuc.edu
Subject: Maximum entropy image
Date: Fri, 6 Dec 91 15:28:49 -0500

I'm watching the VTESS image deconvolving on the screen in front of me now. I decided not to be too greedy and to go for 0.08" resolution with a 0.025" cell size. It's looking good. The major picture seems to be that we will have four very compact spots - the extreme tip of the jet, the extreme tip of the counterjet (standing out bold a streetlight on this image, I'm truly amazed by this), the knot on the north side near the begiining of the bright segment of the jet and (surprise!) the knot at the north-west side of the terminal "hook" on the main jet. I.e. there are now *two* bright spots at the end of the jet, the one right at the tip and the one just above it on the other side of the "flat face" of the hook.

Just shows to go you what a little more resolution can do to really convince you that Nature is much smarter than we are at making radio sources.

I'm enjoying this ... now I *really* don't understand what's going on!

Cheers, A.

David, here at last is my collection of comments and suggestions for the paper, plus some answers to your questions. There are some plots associated with all of this coming in the regular mail. Sorry I took so long, but between non-219 distractions and doing some reanalysis of the old high resolution data this took longer than I first thought.

Did you get my message yesterday re making a poster paper for the AAS in June?

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Comments on the paper, in order:

Overall: It's reading much better, and I like almost all the changes you made since last version.

Throughout: polarization, not polarisation (unless this goes to M.N.!)

could we eliminate the World War II "L Band, C Band" terminology everywhere from the text of the paper and replace with frequencies or wavelengths (I don't much care which, but these band designations don't mean a thing to anyone outside the radio astronomy user community!)

1 kpc (FWHM) could we give the linear resolution (FWHM) in h_100 text as appropriate?

"flux" is almost never correct in radio astronomy, because we are almost always talking about a flux *density*, per steradian, per Hz. Flux is Watts. Anything that's in Watts/ster, or W/Hz or W/ster/Hz, is a *density*. I've noted a number of places where "flux" should be "flux density" but I've probably not caught them all. Could you scan the input file with your text editor for "flux" and make sure it's followed by "density" everywhere (unless there is somewhere that it's in Watts!). Also "fluxes" should be "flux densities".

p.2, l. 4 "is" for "exists"
l. 5 "partial jets", i.e. they seem to disappear
l. 6 is accompanied by a transition from an axial to a transverse
l. 2 from end: break into two sentences (present one far too long)
... the radio core. This model accounts for
p.3, l. 2 between the two sides to different orientations of the passive magnetic field transported by the two jets.
l. 6 delete "finally", insert "alternative" between "these" & "models".
p.4, l. 6 replace "First" with "In the first"
l. 8 replace "Second" with "In the second"
p.5, l.11 "Section VII summarizes our major results"
p.6, l. 3 "could be traced for only about a third of the distance" (it's the third that's "only", not the tracing!)
l. 7 "generic properties", instead of "the properties" (jets have many properties, but not all of them are generic)
l. 6 from end:
"on the path where a counter-jet would be expected. This knot is resolved, and is brightest ...."
p.7 l. 10 "... confining toroidal field component, because data on the apparent field direction alone can neither confirm nor refute ..."
l. 15 delete "proper" (we're not correcting to the rest frame of 3C219;
~~~~~~

if trying to emphasize how wonderful the data are, substitute "reliable" for "proper", but I'd prefer not to qualify it anyway).

p.8 l. 7 from end:
major whoops! (See my earlier E-mail).
Delete the sentence about "These effects were partially removed from the data ..." They weren't, and they can't be.

l. 3 from end:
same thing: "This distortion effectively reduces the radial resolution" (delete the phrase about correction, it's wrong).

p.8 l.11 "absolute flux density"

p.9 l. 2 from end:
"lack of shorter spacings in the L Band data" (delete "that were available" - they weren't available!)

p.10 l. 1 Add sentence. "All final images were corrected for the primary beam attenuation using the standard NRAO model of the VLA antenna pattern." (Actually, everybody will expect that this correction was made, as it's totally routine, but I suppose it won't hurt to say we did it.)

p.10 There is nothing said about adding zero spacings to the L Band data. Wasn't this done? (Should always add them, in fact).
Could mention that the 2.2 Jy used for the zero spacing in the 6cm data also fits the single dish spectrum (KPW got 2.2 Jy at 5 GHz). Curiously, this spectrum calls for 7.8 Jy at 1.4 GHz, more than was CLEANed from the VLA images; I suspect this means that the VLA 22/18 images could have been CLEANed deeper to get the bowl out. Never mind!

p.10 l. 9 and 10, and 4 from end: "flux density" not "flux"

p.11 l. 1, 11, 12, 2 from end: "flux density" not "flux"

p.12 l.12 I'd prefer to say "depolarization over the source" than "depolarization from the source". The depolarization outside the instrument need not be in, or from, the source.

p.12 last line:
I don't like the term "FWHM CLEAN beam of x.x". Can we put FWHM where it would go in words, e.g. "CLEAN beam of FWHM x.x" ?

p.13 I'm afraid I still don't quite understand the point of this test for "type 1" depolarization. It shows that the polarization would be under-estimated at a resolution less than the one we are working with. It doesn't say much about the effect at 1.4" resolution. A comparison with the degrees of polarization in the 0.4" resolution 6-cm data would perhaps be more informative. Could we drop this segment altogether? (I remember Rick raising this point last time round - it struck me again rereading this version).

p.14 l.7 from end:
delete "as well", add "also" before "seen" on line above.

p.14 ref for CLEAN instability:
Cornwell,T.J., Astron. Astrophys. 121, 281 (1983).

p.14 last line:
It isn't "astonishingly circular". It's clearly elliptical, see enclosures and comments below for p.18

p.15 l. 1 "claim indicates" is ungrammatical. "consider to be part of" ?

p.15 l. 8 delete "that may be in the same Zwicky cluster as 3C219". It also may not, we don't have a red shift.

p.15 I still find the steep spectrum for the core of "Baby 219" hard to believe (Table 3). Are you sure the background subtraction was done correctly for this? If not, I'd like to drop all the Baby 219 data from this table. They are peripheral to this paper and I wouldn't want to have to explain away unreliable values later ...

p.15 It occurs to me that someone may ask whether "Baby 219" actually *is* 3C219, gravitationally re-imaged. I don't think it can be, as there is no sign of the jet or hot spots in Baby 219, and gravitational imaging preserves the surface brightness of features while it changes their apparent scale. It's also not clear what the "lensing" object could be, of course. Perhaps we should say no more, but the absence of obvious hot spots and jet in Baby 219 do militate against it being such an image.

p.15 l. 7 from end:
I think Figure 3 is at 1.4" resolution. Shouldn't it say so, either in the text or in the caption? Similarly for other figures that show derived quantities (people don't necessarily read the paper in order, and may just look at one or two figures)

p.17 l. 7 I don't like the term "sudden break". Synchrotron spectra can't have sudden breaks, and we can't say there is one from data at only three frequencies. How about "steepening" instead?

p.17 I worry that nobody will follow the argument that this spectral steepening could indicate a receding relativistic jet, stated as baldly as it is here. The rationale is given on p.30. Could we postpone comment on it until then, i.e. delete from "Since most knots ..." to the end of the para. ?

p.17 last para:
This discussion doesn't mention the possible effects of evolution of magnetic field strength over the lobe, which tend to accentuate the spectral gradients if the field decreases away from the hot spots. Should we mention this effect? It makes the "fans" all the more unusual, as any field decrease would steepen the apparent spectrum away from the hot spots, just the opposite of what we see in these fans!

p.18 l. 7 "it is edge brightened and contains a central peak"

p.18 l. 7 The hot spot rim is clearly a *distorted ellipse* on the 6cm high resolution images - so the "nearly perfect circle" is misleading. Perhaps we should also comment on fact that the surface brightness is very non-uniform around the ridge of the hot spot, the north and east sides being much brighter than south and west. Note that the west rim being so faint makes only about half of the hot spot show up clearly on a contour plot. Perhaps this contributes to the perception that it was "circular" at low resolution. There are also some filaments crossing its interior that may contribute to this but I think the true outer boundary is made quite clear by the 6cm polarimetry, which shows that the highly polarized emission follows the elliptical *outer* ridge of the I image. I'm sending you some plots that may make this clearer.
I also saw a strong spectral index gradient on the hot spot in our old data, with the north rim having a markedly flatter spectrum than the rest. Did this stand up in the revised images, and if so could we quantify it here?

p.19 l. 3 "prominent" (typo)

p.19 l. 7 "flux densities", not "fluxes"

p.21 Fig.6 Errors should be plotted so readers can judge "significance" for themselves, if we keep this format.

Fig.6 PCNTR plots of (%p, chi) in hot spots would be more informative. I did suggest this before, but given the possible debate about the shape of the northern hot spot I'm now more convinced than I was then; I'm sending plots of the high resolution data that could be a Figure in the paper. As shown by PBWF with rather crummier data, the degree of polarization of the north spot systematically increases toward the outer edge of the ellipse. The chi distribution at this resolution is also much clearer than that in Fig.9 and would help make the orientation point on p.42 much more forcefully.

p.21 l.10 "has a local maximum", not "is"

p.21 Figure 7 doesn't add much; you said you were debating whether to keep it. I vote to drop it

p.22 l. 1 delete "from"
p.22 l. 2 I would not emphasize the comparison with Garrington et al., given that we see little depolarization anywhere; we may simply not be at a low enough frequency to see where 219 fits in to their correlation.
p.24 l. 8 What does "significantly less" mean quantitatively here? Compare with filament filling factors in 219, other radio galaxies?
p.25 Figure 9 will get people confused if they read it as a B vector display and it got me confused because it's a Faraday-rotated E vector display, but I guess it will have to be that way for comparison with 13 from CNB unless you can turn that into a B display. To be honest, I think plotting Faraday-rotation-corrected E vectors is totally perverse. If there's enough information to do that, there's enough to produce a %p, B display as has been the norm in radio astronomy for about 20 years. The "standard" displays are %p and uncorrected E, or a high-freq %p and derived B, and most readers will expect one or the other of these. But I guess you're trying to avoid making a new diagram here. To help reduce this confusion, could we label the figure itself "%p, intrinsic E vector orientation" in an easily-readable font size, so it will be properly understood even if people don't scan the small-print caption fully? Incidentally, wouldn't the 6cm vectors be better here, they should be less depolarized and the rotation correction would be smaller. It would not make a noticeable difference in many places, however.
p.25 l. 5 "geometrical edge effect" needs elaboration. Sure it isn't field shearing?
p.26 l. 7 "The next section rediscusses the class of 'restarting jet' models described by BPH in the light of our new observations ...."
p.27 l.10 from end:
Wills (1975), not Willis (also in reference list).
p.27 l. 6 from end:
"up to" 100% variable
p.27 l. 4 from end:
Perhaps we need a reference for variability spectra, I'll try to find one.
p.29 l. 4 (J.P.Leahy, unpublished) -- initials identify the Leahy!
(W.J.M. van Breugel, unpublished) -- similarly to above
p.28 l.14 "contradicted" for "refuted" ?
p.29 l. 1 substitute "constant" for "even"
p.29 l. 5 "If the outflow velocities vary with distance from the nucleus in the same way on both sides, and are non-relativistic, the positions (relative to the core) ... "
p.30 l.14 "also" closer to the core
p.32 l.14 substitute "the discontinuity" for "it"
p.33 l. 7 from end:
"this simulation does not necessarily predict a prominent feature resembling a bow shock ..."
p.35 l. 6 "The simulation of a restarted (born-again) jet therefore does not conclusively support or rule out this class of model."
p.36 l. 3 from end:
"Note that the magnetic field ...."
p.36 last line:
"It is incorporated only to compute the synchrotron ..."
p.42 I find the comparison of Fig. 16 with the North hot spot parameters a bit confusing. Isn't the point that in the actual hot spot, the degree of polarization increases radially outwards, coming to a maximum near the outer rim, where the intensity is also locally a maximum? In both the models and the actual hot spot, we have a central feature with a low polarization, and the degree of polarization increasing toward the intensity maximum on the outer

rim. So why do you say the maximum intensity is a minimum of polarization? This isn't true either for the data or for the models, if I am understanding it correctly.

p.43 l. 3 Cowan, not Cohen
p.44 l. 8 Is It K\"ossl as here or K\"osl as in references?
p.45 l. 7 from end
"single elongated knot which may be ..."
p.45 l. 4 from end
"edge-brightened elliptical hot spot"
p.46 l. 3 "main jet, and becomes transverse as the jet disappears."
p.46 l. 6 delete "circular"
p.46 l. 8 "a small transverse gradient (16 rad/m 2)
p.46 l. 3 from end:
"extrapolation to 10-yr time scales of the evidence ..."
p.48 l.11 "... intermittency, so the polarimetry ..."
p.48 l. 2 from end:
"would favor the 'born-again relativistic' jet model"
p.48 end
In Clarke and Burns you point out that although the shock in a restarting jet and the bow shock should be of the same strength, they need not have the same emissivity, depending on the history of the particles and fields in the cocoon and in the new jet. So I'm not sure how much to emphasize the "prediction" that they should be of comparable brightness. I'm a bit confused about the range of possibilities now.
p.49 l.15 "Both types of model make strong ... "

Caption for Figure 16: define delta and gamma. The figure may not end up being on a page of the journal near the definitions on p.36 of the manuscript, so caption should be fully self- explanatory. If readers don't quickly get the point, they may skip it!

Reference Christiansen, Rolison and Scott (not Rolinson)
Willis,A.G., Wilson,A.S. and Strom,R.G. (1978), Astron.Astrophys. 66, L1.

~~~~~~

Some new data analysis and answers to your questions:

The "inner V" of the jet. Yes, let's say more about it. It would be nice to measure the FWHMs from slices across it if possible, and add them to a collimation plot (see also new collimation stuff below). My guess is that because we see it, it isn't adiabatically expanding though!

I've written a procedure that passes data from AIPS slices into the NRAO single-dish analysis program, "drawspec". drawspec has nice facilities for baseline fitting, multiple gaussian component fitting and error analysis etc., all much more competent than SLFIT in AIPS. Also, it lets you stack (average) slices together, a nice way to get average properties along a jet. While I was making the new VTESS images of the jet I remade the images at the old resolution on a slightly finer grid (I can get away with that in the CONVEX) and then constructed 30 slices uniformly spaced across it, rather than just slicing selected parts as in PBH. The collimation plot from these slices, spaced every 0.45" with a 0.35" beam is quite interesting - it shows a narrowing of the jet at its tip (beyond the last slice in the PBH collimation plot). This narrowing is also consistent with the "superresolved" MEM reduction, of course. It might be worth showing the improved collimation analysis and commenting on the apparent narrowing of the jet near its tip, as well
~~~~~~

of the flow velocity in the reborn jet than in the original one. We wanted beta = 0.57 sin i for 3C219, so a quasi-ballistic case is probably necessary. That was not pointed out in PBH.

Re jet velocities for FRI's. Frazer was claiming a measurement now in M87, not just a limit, but it's a topic that we can drop without damage to the main flow of the paper.

From root Thu Aug 8 12:01:12 1991
From: dclarke@ncsa.uiuc.edu (David Clarke)
To: abridle@polaris.cv.nrao.edu
Cc: dclarke@ncsa.uiuc.edu
Subject: Re: the denouement
Date: Thu, 8 Aug 91 11:00:31 CDT

Alan:

2/5 is exactly what I had anticipated from NRAO. I had read something from NRAO about what they would pay: 33% or pro-rated by authors, whichever was the greater. So I wasn't completely in the vacuum on that one.

Having the preprint single spaced is a perfect idea. I will repaginate the Figures so that Fig 1 is on page 33, and have those sent to you on Monday. Was 150 copies correct?

The 50 reprints I assume is just for the Library. Do you want any in addition for yourself?

I didn't know you were an "oscuro" man. I am always on the look out for a good dark beer, the darker and chunkier, the better. I'm looking forward to Germany, because, with all due respect to your roots, I think Germans make the best beer on the planet!!

Cheers. David.

From abridle Thu Aug 8 09:46:53 1991
From: abridle (Alan Bridle)
To: dclarke@ncsa.uiuc.edu (David Clarke)
Subject: Re: the denouement
Date: Thu, 8 Aug 91 09:44:11 -0400

The NRAO will pay 2/5 of the page charges, based on the 2/5 authorship. The library will need 50 reprints.

Re the preprint: you get a very readable single-spaced version of the paper by halving your baselineskip parameter to 12pt. That way the Figures start on p.33 for Figure 1. It would be nice to do the preprint distribution in that format, as it achieves the same reduction as the two-pages-per-page but is much easier to reproduce and read. If that's too much of a pain, why not just run the Figure pages un-numbered so we could put them at the back of either format?

Nice to have it accepted at last, will raise a glass of good dark stout to it in celebration!!

From root Thu Aug 8 06:08:54 1991
From: dclarke@ncsa.uiuc.edu (David Clarke)
To: abridle@polaris.cv.nrao.edu, jburns@NMSU.Edu, norman@ncsa.uiuc.edu, rperley@zia.AOC.NRAO.EDU
Cc: dclarke@ncsa.uiuc.edu
Subject: the denouement
Date: Thu, 8 Aug 91 00:51:47 CDT

Well, folks, the official letter from Dr. Abt came in today. The version with the corrected units is slated for January 20, 1992. Let's see. I first wrote the section to my thesis in Feb 1988. Hmmm. At any rate, this one is in like Flynn.

I presume no one had any problem with the charging algorithm that I outlined in my cover letter to Dr. Abt. In addition to that, everyone can pay for the number of off-prints that they may want, plus a 1/5 share of the offprints we agree to send out on a mailing list. So, could everyone let me know how many extra offprints they would like above and beyond what will be mailed out right away to those on our mutually inclusive mailing list? Thanks.

Alan: I will send you 150 copies of the Figure pages early next week for the preprint series.

Again, thanks to all. It is indeed satisfying to see our chick finally hatched.

Cheers, David.

From root Sat Jul 27 14:51:19 1991
From: dclarke@ncsa.uiuc.edu (David Clarke)
To: abridle@polaris.cv.nrao.edu
Cc: dclarke@ncsa.uiuc.edu
Subject: Re: misunderstandings
Date: Sat, 27 Jul 91 13:56:23 CDT

Alan.

I answered your long and much appreciated response to my earlier message, but my cC'ed copy never got to me. So, in case you didn't get your copy either, I just wanted to thank you for your thoughtful response. It was very much appreciated. I also wanted to tell you that in retrospect, I realise I acted hastily, and should not have ignored Rick's comments. I am getting the strong impression that his invitation to me to ignore his comments was made in jest, and it probably never occurred to him that I might take him up on it!! Anyway, live and learn. I sent an e-mail to Rick apologising for the recent exchange, and I hope he takes it in the spirit in which it was meant.

Onto more practical details: The revised draft of the 219 paper (what other paper could I be talking about!!??) is now in my directory on Fermi. You might want to make sure I didn't miss any flux densities which should have been brightnesses. I think I made all the correct changes, some that Rick did not find. I don't think I changed any flux densities that should have remained flux densities either. If any further errors are found, we can change them at copy-editting time.

I will take care of getting the figures done. The pagination remains unchanged, (Thank God) and so I will send you 150 copies of pages 55 through 68. They will be double-sided, full size images. Am I correct in assuming that NRAO typically puts two pages on side, double-sided (ie, four pages to a sheet)? This would cut down on the humongous amount of paper generated. Speaking of which, I am loath to duplicate circulation of preprints because of the blatent waste of paper. So I resist doing an independent NCSA preprint circulation. Can you therefore, send me a copy of the mailing list used to send out the NRAO circulation? Then I can make a request for enough copies to send out to those on my list not included on yours.

I will await the final word from Dr. Abt that the paper is to be published as submitted before I get the Figures reproduced.

Best regards, and thanks again for your response.

David.

From root Thu Jul 25 14:53:10 1991
From: dclarke@ncsa.uiuc.edu (David Clarke)
To: abridle@polaris.cv.nrao.edu
Cc: dclarke@ncsa.uiuc.edu
Subject: Re: Rick's comments
Date: Thu, 25 Jul 91 13:58:13 CDT

Dear Alan:

Well, when I first got in this morning, Jack had already responded to my response to Rick simply by saying:

"I'm with you on intensive/extensive & flux density. Let's just do it & move on!!"

That vote, with mine and Mike's gave inertia the majority, and so I went ahead and sent everything off to Apt this morning before I got your vote. Sorry, but I think this is not the first paper to get the technical definitions of flux density muddled, nor will it be the last. Also, intensive/extensive has remained, as has "thermalized, "minimum energy magnetic field", and "interesting".

So, there it is. About preprints. You mentioned that you would like to go through the NRAO route. That is fine with me, and you should also know that I would be willing to take care of it via our less formal preprint mechanism here if you have no time for it. Either way.

Assuming that we go the NRAO preprint series route: I have sent you via regular mail a hard copy of the paper. In this mailing, I have returned your 3 1/2 inch floppy disc. I have not updated the paper onto this floppy. It contains the version of paper sent to the referee. So, if you want an electronic copy of the paper, you can ftp to my machine:

fermi.ncsa.uiuc.edu (141.142.221.15)
dclarke
tto742

and get the files:

papers/3c219.pap
tex/setup.tex

As for figures, we have a machine here that will produce superb photocopies of the glossies (I've sent you a sample with the hard copy that is in the mail). If you think it is important to have such reproductions in the preprints rather than the usual crude copies that most copiers make, *and* you do not have access to such a copier, then I could handle the figures here, and you could have the text copied there. Also, I have all the figures pasted on pages with the correct annotation (page number and Figure number). If you do want to do the figures there, and you do not want to have to cut and paste your copies of the glossies onto pages with the correct annotation, I could send you my originals. *But*, I would want these back, as they are my only copy of the original glossies. Again, it might be easier if I just got the figures reproduced here.

If I do the figures and you do the text, how many copies do you want to make? I have a mailing list (of which Jack's and Mike's are subsets) of about 50.

I hope my semi-draconian measures of not waiting to hear from you regarding Rick's comments hasn't pissed you off too much! I was just anxious to get this thing out once and for all!!

From root Wed Jul 24 19:11:29 1991
From: dclarke@ncsa.uiuc.edu (David Clarke)
To: rperley@zia.AOC.NRAO.EDU
Cc: abridle@polaris.cv.nrao.edu, dclarke@ncsa.uiuc.edu, jburns@NMSU.Edu
Subject: Rick's Comments
Date: Wed, 24 Jul 91 18:16:12 CDT

My responses to Rick's comments:

1. Yes, the use of extensive and intensive are ''borrowed'' from thermodynamics, but do have general meaning. In thermodynamics, the ''definor'' of extensive is the volume (or sometimes the number of particles) in question. Here, I have used the ''definor'' to be the total intensity. Dividing P by I does not remove the dependency of total intensity, since the total intensity and polarised intensity are fundamentally different quantities. Hence, I include fpol and dpol as extensive quantities. On the other hand, chi is derived from a ratio of Stokes Q and Stokes U which are fundamentally identical quantities. Thus, the division removes any dependence on intensity, polarised or total, and is therefore an intrinsic quantity.

The definition is formal, and does not make *that* much of a difference to the entries in the Tables. Perhaps it is a red herring. I will delete these terms if folks want.

2. Well then, I *still* don't understand the difference between fluxes, flux densities, and brightnesses. To me, the word FLUX implies an amount of energy crossing a unit area in a unit time (units WATTS/M**2). To be a FLUX DENSITY, you need to add another "per". So, if you want to talk about FLUX per unit wavelength, then that already becomes a FLUX DENSITY. If you also stipulate per beam, then that is still a FLUX DENSITY. Units of FLUX DENSITY can be WATTS/M**2/HZ (proportioanl to a JANSKY) *or* WATTS/M**2/HZ/BEAM, depending on what you are talking about. Because both these types of FLUX DENSITIES are used in radio astronomy, I think of BRIGHTNESS as being the former, ie WATTS/M**2/HZ, which is determined by integrating the FLUX DENSITY over the feature in question.

Now have I got these backwards??? Are the units of BRIGHTNESS JY/STERADIAN (ie JY/beam) and the units of FLUX DENSITY just JY?? I thought Alan and I had this all straightened out!!

3. No, the NCB model says nothing about depolarisation. It seems to me that depolarisation from a foreground screen is not caused by the screen per se, but by the fact that we are not resolving it sufficiently. Thus, the Cygnus A screen should not depolarise the signal if the screen is sufficiently resolved. I thought you guys tested this in Dreher, Carilli, and Perley, and found that you had amply resolved the Faraday screen, ergo no depol. Now, 219 is not as well resolved as Cyg A. We do not know that the screen is amply resolved. In particular, we are saying that it is very likely *not* well resolved in the vicinity of the depol filaments.

4. See 2.

5. I think "high" is fine given that we define \nu on page 7, second par, second line.

6. see 2.

7. This very argument, as I recall, was shot down by you and/or Alan during our meeting in ABQ two years ago! The thinking was that if the jet was continuous, and yet not visible for most of its length, then the toroidal field

should continue down to the hot spot. If the jet was not continuous, but actually ended wher it appears to end, then a toroidal component to the magnetic field should still survive in the lobe from previous jet activity (an expanding smoke ring still retains a toroidal component for some time). These are not compelling ideas, but they are suggestive that any RM pattern indicative of a toroidal magnetic field ought to be prevasive all the way down the lobe. This idea has been in the paper now for two years. I would suggest this is not the time to re-open this debate.

8. Noted.

9. Note the "and/or" rather than "or" in the sentence. Actually, one does not need compression, and I doubt very much that one could support any large scale compression given that the boundary layer between the radio lobe and the IGM is probably a contact discontinuity rather than a shock. At any rate, take a wire, bend it in any shape you like and hold it up at any orientation you like. Nine times out of ten, the projection in the plane of the sky of the piece of wire nearest the "bottom" of the curves in the wire would be described as "tangential" rather than "perpendicular" to anything that might contain it. I would need to show this to you if you can't gleen what I'm trying to convey from what I wrote. At any rate, I think projection effects by themselves are sufficient to give circumferential pol vectors in the majority of cases (look at Laing, 1981 - there's no compression there).

10. If regions of low fractional polarisation are caused by a mixing layer between two regions in the source (or a boundary layer, or whatever you want to call it), then the orientation of the local magnetic field is probably doing back flips there. Thus, a beam would not resolve the B-field sufficiently there, ergo the low fpol. Boundary layers are 2-D curved surfaces, not 1-D filaments. Talk to Alan about this, he was the one who pointed this out to me.

11. ...

12. Thermailised here means nothing more than changing the form of energy from (organised) kinetic energy to (pseudo-random) internal energy. For a non-LTE fluid or plasma, I agree that calling it a "thermal" gas is inappropriate. But I believe the expression "thermalised" still conveys the physics.

13. ...

14. got it.

15. got it.

16. I think this was admitted honestly on top of page 33.

17. Actually, we mean in the jet *and* the lobe. While it is true that an adiabatically, gamma=5/3 gas will eventually expand enough so that an initially passive magnetic field becomes active (pressure goes as r**(-5), B**2 goes as r**(-4), thus beta goes as r**(-1)), this is not the case for a gamma=4/3 gas. In any event, the simulations all start off with a very passive magnetic field (beta = 10**10), and so the field is still passive in the lobes. You see, you need a passive magnetic field in the lobes to produce filamentation, at least according to NCB.

Thanks Rick. Could everyone please cast their ballots regarding the phrases "flux density" vs. "brightness" and regarding the use of "extensive" and "intensive" by Friday please? I shall then resubmit (with old Fig 16 reinstalled).

From root Wed Jul 24 17:52:11 1991
From: rperley@zia.AOC.NRAO.EDU (Rick Perley)
To: abridle@zia.aoc.nrao.edu, clarke@ncsa.uiuc.edu, jburns@nmsu.edu
Subject: My Comments on 3C219 Paper
Date: Wed, 24 Jul 91 15:57:17 MDT

Sorry to be a day late -- more administrivia, etc.

I'm very happy with the revised paper. I have no major suggestions, only a series of rather minor comments. I suggest that if the referee (or editor) bitches about the length, we shout back good and loud.

My comments, organized by page and paragraph:

1) page 11, on 'extensive' and 'intensive' quantities. My memory recalls only that these terms arise from thermodynamics, where extensive refers to quantities dependent on the volume (like total energy, and mass), while intensive refers to properties such as temperature, which are independt of volumen. If this is correct, it seems we are extending somewhat the definitions. In any event, even if we accept these new definitions, doesnt fractional polarization (paragraph 2, 4th line) NOT depend on flux density (here used as the definor of extensive)?

2) page 12, paragraph 2. We speak of flux density of the filaments, then use mJy/clean beam to describe them. mJy/clean beam is a unit of brightness, not flux density. I trust that here we are truly speakinf of brightness, not flux density (which would then require some statement of the filaments' size).

3) page 13, top. Does the NCB model make any prediction about the depolarization which accompanies the proposed entrainment, and are our data consistent with that prediction? Even if you (we) take refuge in the low density medium which surrounds 3C219, what of Cygnus A, where we have lots of filaments, lots of heavy-duty gas, and NO depolarization?

4) page 13, paragraph 2. Line 3 has 'flux density' where, I think, we want brightness.

5) page 13, par 2, line 6. The word 'high' for spectral index is always a little difficult. What's high? large positive? large negative? (What's the definition, anyway..) Perhaps we should use 'steep'.

6) page 19, par1, last line. Again, flux density vs. brightness (or, in this case, energy density). This case is a little different, in that flux density can be used -- but is an incomplete description, since the proper physical quantity is the emissivity.

7) page 19, par 2. We say that the RM gradient should not disappear if it is caused by a global magnetic field structure. The implication is that we think such a global structure should be there. Why should it be so? The jet could 'somehow' be self-supporting via currents which circle about its end, such that no circumferential fields would exist in advance of the tip.

8) pag 20, par 2. Other example of localized, intriguing RM features near hotspots are found in Ed Zukowski's thesis (U. of T., 1990).

9) page 20, bottom. A line of sight grazing the edge of a lobe won't give a circumferential appearing field without accompanying compression (I think). i.e., it could be shear, or compression. (Or have I forgotten the truth?)

10) page 22, middle. We say 'probably 'sheets', not 'filaments'. On what basis? I can see no justification. Have I forgotten something, or missed something?

11) page 25, middle. Yes, it will be interesting to investigate these things, but I think it is better to say 'important', rather than 'interesting'. But perhaps that reveals my bias.

12) page 26, top. The word 'thermalized' bothers me a little. It

implies there is thermal material in the lobes, and in the jet. There is no evidence for this (that I know of and believe). [Patrick Leahy violently disputes this statement, by the way]. Unfortunately, you modelers use, implicitly, Maxwellian plasmas, implying the existing of thermal material. (Am I right or wrong here?) I admit that the hot spots *probably* represent strong shocks, resulting in a redistribution of energy, from bulk to 'thermal', thus justifying the term 'thermalized'. But the lobe material may not be (probably is not) thermal at all.

O.K., O.K., I'm a nit picker. I probably have something better to do...

13) page 30, par 2. On the preceding bow shock in the restarting jet. The predicted presence of this shock is based on there being both fields and particles in the 'pipe'. I guess this is reasonable, as the 'wake' of the preceding blob should have contained both entities.

14) p 33, top line. 'Extend' shoudl be 'Extent'.

15) p 34, second-last line. Another typo, 'enhamcement'.

16) p 35, par 2. We say the passive-field model accounts for major features in total intensity and polarization simultaneously. Sort of -- the rapid (precipitous) fall-off in brightness upstream of the jet is not explained. But is this too nit-picky?

17) p 36, line 3. ...magnetic field in 3C219 in passive. We mean in the jet of 3C219, presumably.

That's it, guys! You may ignore, if you wish, any and all of these comments.

Rick

From abridle Thu Jul 25 11:46:21 1991
From: abridle (Alan Bridle)
To: dclarke@fermi.ncsa.uiuc.edu, jburns@nmsu.bitnet, rperley
Subject: Rick's comments
Date: Thu, 25 Jul 91 11:44:37 -0400

Here are my reaction to Rick's comments and David's reply:

1. "Intensive" vs "extensive". I didn't like this originally because I thought a "new" terminology would distract the readers and we weren't using it for anything substantial. Given the debate it has started among ourselves, I suggest we follow David's suggestion and drop the language (into what I think Robert Laing once described as another paper's "kettle of red herrings"?).

2. Rick is definitely right, re the standard radio terminology. David, we have indeed been over some of this before, but if you recall it was in the context of standardizing our use of flux _density_ rather than just _flux_, and of mJy per _CLEAN_ beam rather than the ill-defined mJy per beam. In radio astronomy the term "flux density" is really an abbreviation for "spectral flux density", and its units are watts.m^-2.Hz^-1. Anything in Jy or mJy is a flux density because of the Hz^-1. Once you also normalize by an area in steradians you have something that is formally equivalent to a brightness temperature in Kelvins and so astronomers at most wavelengths will call it a "surface brightness" or just "brightness". I agree that it is in a sense a further category of density, per area as well as per bandwidth, but Rick is absolutely correct in saying that we should call it a brightness. I've just read the paper too often to see these things any more. I suspect that lots of people get confused by reading sloppy usage in the literature, but Rick is perfectly correct in asling us not to contribute to that sloppiness ourselves. The rules are quite simple - if it's in Jy or mJy, it's *always* a FLUX DENSITY, if it's in Jy or mJy per area of image it's *always* a SURFACE BRIGHTNESS, or brightness for short.

3. I think David is right. We are quite explicitly associating the depolarization with unresolved RM fluctuations, on the grounds that we see it best near partially resolved RM fluctuations. To connect to the NCB model you would need predictions of scale lengths and the IGM field strength, inter alia.

4. Rick's right.

5. David's right (he means we defined \alpha, not \nu, on p.7 !) But maybe Rick's point means that the reader will have forgotten this already, so we might as well define it again at the top of this paragraph, which is where we describe all the spectral index _results_. Some people won't read the "how we did it" in Section II anyway (do you think theorists _ever_ read sections titled DATA REDUCTION?). They'll skip to Section III for "what we found" or even more likely Section VI, for "what we believe".

6. Rick's closer to being right than our present text is. Field strength is governed more by surface brightness than by flux density, though in fact we introduce a further depth parameter in the B calculation, and this depth parameter is a key one for this context. While you're fixing this to read "surface brightness", David, why not also do something about the "energy-as-adjective" construction in "minimum energy magnetic field"? - e.g. "between the observed surface

brightness and the magnetic field strength at minimum energy" ?

7. I agree with David, for _his_ model. If the jet continues all the way through the lobe, and the RM gradient is a sign of an active field, then the RM gradient should still be there even if the jet has become hard to see. Of course, if it's a "born-again" jet, Rick's point may be fair enough, depending on how the currents return to mother in an active-field born-again jet. Did we ever look into that possibility, in fact? Should we leave it open, if we didn't?

8. OK.

9. I think David's right. The Laing model does this (though this model precompressed everything into sheets and then wrapped them -- there's just no _extra_ compression at the edge in Robert's picture).

10. I was bothered by unequivocally describing these things as "a third form of filament" in an intermediate draft. I believe that they are most likely to be places where our line of sight goes across a boundary layer of some kind that's small compared to our beam, so the effect is a mixture of beam depolarization and edge geometry as in #9. In this sense, the "sheets" would be curved (conical around a jet). The problem is that this paragraph is still a bit terse for its content. The main point is that we can't assume that long thin features in depolarization are _filaments_, and _sheets_ are also quite likely to be present, but will look like filaments in the data.

11. Do it.

12. Do we need the loaded word "thermalized" at all here? Can't we say "randomized", or "disordered" and convey the same picture?

13. You got it, Rick. (Nothing to do here.)

14. I suggested dropping the phrase anyway, as "tempered" already implies "to some extent".

15. ...

16. Horse already flogged to my satisfaction.

17. We mean _everywhere_, to make the big filamentary cocoons.

Hey, guys, we seem to have converged without colliding! I agree with Rick, if the ref shouts again after reading *this* version, we start shouting back. This is now much better reading than most Ap.J. papers already! May not be perfect, but it will certainly do.

Mail just in from Barry proposes Sep 8 and 9 for our two 8-hour runs on "Return of 3C219, Part III: Judgement Day".

Cheers, A.

July 16/91

Dear 3C219ers,

Jack has gone through the paper with a surgical machete and has done what I think is a fair and good job of cutting back the size of the paper. It is fair because both models are still treated equally and because the paper still has the observations as its main thrust. I enclose a copy of Jack's letter to me which explains his criteria for cutting the paper.

Please go over the new version of the paper. If we can all agree that none of the baby has been lost with the bath water, and if you can find no typos or gaps in the continuity we can get this thing off our backs for good. If I dont hear anything from anybody by July 26 (a Friday), I'll send the paper off as is to Dr. Abt.

I also enclose what I propose to be the cover letter to Dr. Abt explaining the changes made

Thanks guys.

David

Sorry for all the paper — our miserable copier refuses to duplex without jamming.

David A. Clarke
The Beckman Institute
NCSA, Drawer 25
University of Illinois at Urbana-Champaign
Urbana, IL 61801
July 15, 1991

Dr. Helmut A. Abt
Managing Editor
The Astrophysical Journal
Kitt Peak National Observatory
P. O. Box 26732
Tucson, AZ 85726-6732

Dear Dr. Abt;

Please find enclosed three copies of the revised manuscript entitled "Origin of the Structures and Polarization in the Classical Double 3C 219" by D. Clarke, A. Bridle, J. Burns, R. Perley, and M. Norman which was received by your office in its original form on March 22. We have carefully considered the referee's comments and have attempted to address them as explained below.

The referee had only general comments regarding the manuscript in its original form. Briefly, these comments were:

1. the paper was too long, and repetitive of published results;

2. the paper lacked focus.

Accordingly, we have made the following changes:

1. The abstract has been cut by 50%. It now just states the results with no attempt to give the reader a feel for the reasoning behind the conclusions.

2. Sections I and II have been combined and their composite is only 2/3 the length of the original sections. This has been accomplished by eliminating most of the observational history of 3C 219, relying on the interested reader to refer to BPH and PBWF.

3. Section III is now Section II and has been reduced by 15% or so, mostly at the expense of the discussion of the "false" 3C 219, as suggested by the referee.

4. Section IV is now Section III, and has been left more or less intact. Two figures have been eliminated since we never actually referred to those figures after they were described in the text.

5. Although Sections V and VI (now Sections IV and V) have been left as separate sections, we have reduced their length considerably. In particular, all discussion not directly relevant to 3C 219 has been removed, and all previously published figures (or facsimiles) have been eliminated. While we feel that this may rely too heavily on the reader to be intimately familiar with the previous papers describing the numerical simulations and

thus detract from the continuity of the paper, we agree with the referee that the original manuscript discussed the previous results too much. We hope that we have reached a suitable compromise.

6. Finally, Section VII (now Section VI) has been cut by about 25%.

Overall, the text in the paper has been reduced by about 20% and eight figures have been eliminated. We hope that these changes address the referee's comments to everyone's satisfaction, and that the reduced length of the manuscript has improved its focus, as the referee suggested it might. We would like to thank the referee through you for a thorough and timely reading of the paper.

Yours very truly;

David Clarke

THE ASTROPHYSICAL JOURNAL

HELMUT A. ABT, *Managing Editor*
Kitt Peak National Observatory
Box 26732, Tucson, Arizona 85726-6732
Telephone: *602-325-9215*
Express: 950 N. Cherry Av., Tucson, AZ 85719
Facsimile: *602-323-4183*
INTERNET: apj@noao.edu

June 7, 1991

Dr. David A. Clarke
The Beckman Institute
NCSA
Drawer 25
University of Illinois at Urbana-Champaign
Urbana, IL 61801

Dear Dr. Clarke:

Your paper entitled "Origin of the Structures and Polarization in the Classical Double 3C219" by David A. Clarke et al. was sent to a competent referee, and a copy of the report is enclosed for your consideration.

When you send a revised version, please describe the changes made.

When you retype the manuscript you might try using the new style announced in the last July 1st editorial because all parts of the Journal (and several other journals) are now printed that way. The footnotes to the tables should be typed double spaced for copyediting.

Sincerely,

Helmut A. Abt jo

Helmut A. Abt
Managing Editor

HAA:jo

Enclosures:
original manuscript
& 13 figures and 12 plates
Report of Referee

Published by The University of Chicago Press, 5801 Ellis Avenue, Chicago, Illinois 60637
for THE AMERICAN ASTRONOMICAL SOCIETY

REFEREE'S REPORT

The radio source 3C219 has many qualities and problems that are associated with a number of other radio sources, and as such it is an ideal object for detailed study. The present paper adds to the body of observational data available about this source, and in addition it includes a detailed comparison with previous numerical simulations.

In its present form the paper has two fundamental and probably related difficulties. First, the paper is much too lengthy, and second, the paper lacks focus. Some examples are in order. Every section of the paper could be shortened, some slightly, some drastically. An emphasis on brevity and concise statements will also serve to focus the discussion and encourage reader interest, which is a problem with the paper in its present form. The abstract, which is closer to a short three paragraph essay, contains too much background material. Section II repeats some of Section I and is not really needed. In Section III, the discussion of the "false" 3C219 is much longer than needed. Section IV includes detailed discussion of many features with little or no motivation and no indication of which features may be more astrophysically important than others. This is particularly true of Section IVc.

Although Section IV is overly long, the new observations in it are a useful contribution to our knowledge of 3C219. The length of Sections V and VI, however, cannot be justified. A lengthy and rather undirected discussion of previously published results, complete with figures, is inappropriate for articles in this journal. Moreover, at the end of all this, the authors conclude that neither model can be definitively tested or applied. One gets the impression that both models must be discussed in detail simply because a subset of the authors have previously done the calculations. No basic astrophysical problems associated with these objects are addressed in this section. Instead one finds a discursive discussion of many different features which may or may not be reproduced by the simulations. (I would plead with the authors to delete the inelegant and incorrect phase "numerical observations"). Which of these features are "weather" and which, if any, are indicators of fundamental processes is never discussed. A brief reference to the simulation papers, together with a discussion of only those aspects which are of fundamental importance should be all that is included in Sections V and VI. In fact, they could easily be combined into one section. If comparison to particular features of the simulation is made, one also needs an indicator of how robust (i.e.-real) these features are.

The summary section (VII) is also overly long as it is a restatement of what has gone before. The salient points the authors wish to emphasize should be highlighted here.

These concerns must be addressed before the paper is acceptable for publication in the Astrophysical Journal. However, they are largely stylistic and editorial, although the relevant astrophysics must be identified. Finally, the authors are to be commended for their candor in describing the shortcomings of the numerical simulations; such refreshing realism is all too uncommon.

From abridle Mon Jul 22 15:53:04 1991
From: abridle (Alan Bridle)
To: dclarke@fermi.ncsa.uiuc.edu, jburns@nmsu.bitnet, rperley
Subject: 219 paper
Date: Mon, 22 Jul 91 15:52:10 -0400

I got my copy of the revised manuscript today. It's in good shape, and Jack has done an excellent job of pruning. I have only one worry about part of the baby going down the drain. That part is the old Figure 16 (the "anatomical sketch" of the main features of the simulations). I'm not sure that all the readers will be immediately familiar with the terminology of criss-cross shocks and cocoons. When I give talks on this topic I suaually get asked questions that are helped a lot by havinga Figure like this to refer to. So I think it would still be useful to have this Figure and its caption appear shortly before what is now Figure 12, and before the text in Section V that uses the "anatomical language" extensively. I know the figure has been published before. But in this one case that's not the point - it's a visual that will help some readers quickly comprehend the relationships beteeen features of the radio source, the model and the language we are using to describe both. So I'm slightly in favor of putting it back in. It would now be Figure 11 and would be reinserted about midway down the new page 31. I do not wish to insist on it, however.

I do have a small number of very minor points.

On p.12, second para. we should say "In contrast to _those_ in M87, the total intensity filaments ..."

On p.16, the last sentence of the third para has become clumsy and acquire the typo "absense". We can fix some of the clumsiness and the type by saying:

"Because the lobes of Baby 219 resemble those of 3C219 while the jet and hot spots are absent, we can therefore rule out the possibility that Baby 219 is a gravitational image of 3C219".

On p.33, a typo "extend" from the previous version has survived. Let's fix it by shortening the sentence anyway, to read: "The qualitative agreement between the model and the observations is tempered by two possible inconsistencies."

Also on p.33, the recasting has produced too many sentences starting with "In addition". How about changing the first sentence of the second para to read: "These simulations have several limitations".

I haven't checked whether all the references that were deleted from the discussion were also deleted from the reference list. Has Jack or David done this, or should a final refereence check be done again?

Overall, I like the revised version a lot better than the one we submitted, and I believe the referee's comments and the work Jack did on shortening it have done us a favor.

Best wishes, Alan

TO: DAVID
FROM: THE OLD CHIEF
SUBJECT: 3C 219 CUTS

I am enclosing a marked up copy of the 3C 219 paper for your consideration. As I mentioned in my E-mail, I have made a large number of cuts but tried hard to maintain the strong observational sections & the overall thrust of the paper. You will have to decide if I succeeded.

I generally tried to follow the suggestions of referee. Most of the time, I agreed with his comments for cuts but not always. If you accept my deletions, we can certainly state firmly that we responded well to the referee's report. The text is shortened by at least 15% and I've suggested that we delete 7 figures, most of which have already appeared in our other refereed publications. Please don't be horrified by the large number of suggested deletions. I think that they are all justifiable if you accept my primary criterion for deletions: appearance in another major publication.

Let me be more specific now on my reasoning behind the suggested cuts. First, the abstract was shortened by 50%. I agree with the referee that it should be a brief synopsis of what's in the paper. I think it serves that purpose well now in its shortened form.

Second, I suggest that we combine sections I and II together into one section. Your goals of both reviewing previous publications and setting the motivation for the new work can still be accomplished. There was a fair amount of repetition and some non-essential background material in these sections which can be easily cut. Sorry about the mess, but I changed my mind a bit on exactly how the cuts could be made after a 2nd iteration.

Section III was cut by about a page. This was almost entirely in the area involving the "false 219" maps discussion. I cut down some possibly unnecessary details & just kept the essential summary of the results. I think that's enough to convince the reader that you explored the uv limitations on the spectral index. All the details may be overkill. The spirit of this important check remains alive, however.

Section IV (The Data) is the heart of the paper & I could not justify cutting very much. Here I disagree strongly with the referee. This section is pure observations. We are simply explaining the features that we see, noting peculiarities, & attempting not to make (many) interpretations. I like this section the way it stands. My only cut was concerning your paragraph disputing the Hines _et al._ overpressured filaments argument. I believe this rebuttal to their work is not appropriate in this paper, especially since the 3C 219 filaments do not appear to be strongly overpressured. I, too, disagree with Hines _et al._ but suggest we leave this fight for another day & a better forum. Also, I suggest that we delete Fig. 10. Fractional polarization maps at both 6 and 20 cm plus a depolarization map are redundant. The 20-cm fractional polarization map really isn't needed in my opinion since it doesn't add a lot to the later discussion & quantitative analysis.

Sections V and VI are where the big cuts occur. I've probably cut 40% or more of these sections combined plus most of the figures that have appeared in our other modelling papers. I've taken the approach that the interested reader should go to these referenced papers, read them, & look at the figures. For the only mildly interested reader, he should be able to get enough from the summary to understand the models & their applications to 3C 219. In fact, I've tried to keep descriptions of the model results only as they apply to 219. I do like Figures 19 & 20 since they are new and directly compare 219 & the MHD model. You'll just have to go thru my suggestions in detail to see if you agree.

Finally, none of my suggestions are cast in concrete. I expect that you will disagree with some or many of the deletions -- afterall, this is your baby & its hard to let him go into the arms of another. But, I've tried to take the position of an outside reader & asked how the paper could be restructured so that I would likely want to read it all. When you've reviewed my comments, give me a call & we can go over them if you like. I hope that this is helpful to you.

NATIONAL RADIO ASTRONOMY OBSERVATORY
EDGEMONT ROAD, CHARLOTTESVILLE, VIRGINIA 22903-2475

Dr. ALAN H. BRIDLE

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BITNET abridle@nrao SPAN 6654::abridle
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March 14, 1991

Dr. Helmut A. Abt,
Managing Editor,
The Astrophysical Journal,
Kitt Peak National Observatory,
P.O. Box 26732,
Tucson, AZ 85726-6732

Dear Dr. Abt,

We enclose two copies of the manuscript of an article entitled "Origin of the Structures and Polarization in the Classical Double 3C219" by D.Clarke, A.Bridle, J.Burns, R.Perley and M.Norman. We hope that this will be suitable for publication in the main journal of the Astrophysical Journal. Xerox copies of the figures are included with each manuscript but some do not do justice to the originals. We therefore enclose, in a separate plastic envelope, the originals for all of the glossy plates, which you could send to the referee if necessary for clarity. The Figure numbers are labeled on the back of each glossy. Although we have a second set of prints should these be lost or damaged, we hope that those enclosed will suffice both for the referee and to produce the paper.

Please send all correspondence in connection with this article to:

Dr. D. A. Clarke,
The Beckman Institute,
NCSA, Drawer 25,
University of Illinois at Urbana-Champaign,
Urbana, IL 61801

Yours sincerely,

David A. Clarke

Alan H. Bridle

From: CVAX::GATEWAY::"DCLARKE@NCSA.UIUC.EDU" " (DAVID CLARKE)" 24-OCT-1990 1'
To: ABRIDLE AT NRAO
Subj: One more time...

Date sent: Wed, 24 Oct 90 15:56:58 CDT
Received: from VTVM2 by vtvm2.cc.vt.edu (Mailer R2.07) with BSMTP id 6475; Wed
 24 Oct 90 17:01:29 EDT
Received: from uxl.cso.uiuc.edu by VTVM2.CC.VT.EDU (IBM VM SMTP R1.2.1MX) with
 TCP; Wed, 24 Oct 90 17:01:25 EDT
Received: from bardeen.ncsa.uiuc.edu by uxl.cso.uiuc.edu with SMTP id AA04848
  (5.65+/IDA-1.3.5 for ABRIDLE%NRAO.BITNET@VTVM2.CC.VT.EDU); Wed, 24 Oct 90
 16:02:03 -0500
Received: from fermi.ncsa.uiuc.edu by bardeen.ncsa.uiuc.edu (4.1/NCSA-4.1)
 id AA07663; Wed, 24 Oct 90 16:00:25 CDT
Return-Path: ‹dclarke@ncsa.uiuc.edu›
Received: by fermi.ncsa.uiuc.edu (4.1/NCSA-1.2)
 id AA01657; Wed, 24 Oct 90 15:56:58 CDT
Message-Id: ‹9010242056.AA01657@fermi.ncsa.uiuc.edu›
To: ABRIDLE@nrao.bitnet
Cc: dclarke@fermi.ncsa.uiuc.edu

Hi Alan.

Just so I have this spectral index thing clear, you mentioned three sources of calib error:

1. error in normalisation (whatever that is)

2. spectral shape (I think I know what you mean by that - the interpolation done by SETJY or CALJY or whatever it is called at the beginning of the old DEC10 calibration procedure)

3. error in absolute scale of all the 3C286 spectrum (I take it that means th: we never know precisely how bright 286 is at any one time).

Now, the latter two will depend mostly on time. Therefore, since we observed 18 and 22 cm simultaneously, these doesn't contribute very much. So far am I understanding you correctly? As for the first type, what is it, and why would it NOT be the same (or at least very nearly the same) for two frequencies observed in the same band at the same time?

The reason why I seem to be so pedantic about this is that while you were en route back to Ch'ville, it ocurred to me that the calib error at 18 and 22 cm should be nearly the same, and so not contribute to the spix error between 18 and 22 cm. So I fired off a quick e-mail to Jack to make sure that I wasn't completely out to lunch, and he sent me back a message telling me that I was, in fact, completely out to lunch! Diconcerting, since I thought I understood calib errors, at least a bit. So if you could confirm for me that my re-hash of what I thought you said is correct, then I would be more settled in my mind about what to do with the calib error in the paper vis a vis the low freq spix

As far as your explanation of the depol filaments and how they are related to the RM, I'm fine with leaving out the mass calculation, but I still don't understand where the numbers came from. Are you saying that you were complete: wrong in what you did, or that what you did was so hopelessly model-dependent that the numbers don't end up meaning that much? In the case of the latter, I would still like to know:

- How can an RM gradient of 26 rad/m**2 per beam give you any significant depo:

(when it takes 150 rad/m**2 per beam to give you a depol of a lousy 5% at 6 cm)?

- What does a "simple slab model" have to do with the calculation?

- Why are large-scale RM gradients relevant? I thought it was large local gradients in the RM that gives you depol.

- How can we mix RM's (which measure the n_e B_los of the screen) with depols (which measure the n_e B_los of the emitting plasma)? The only way I thought they could be mixed is in the event of type 2 beam depolarisation, as in the discussion in section III in the paper.

Thanks for answering all these questions.

Cheers, David

6cm 1".4 slices every 1".2

	I	Φ
1=Core	0.0505 ± 0.0000	1.4205 ± 0.00018
2	0.0058	1.366 ± 0.0016
3	0.00023	1.548 0.0406
4	0.00018	1.280 0.0534
5	0.00054	1.417 0.0238
6	0.00326	1.503 0.0041
7	0.00573	1.306 0.0022
8	0.00493	1.529 0.0021
9	0.00546	1.477 0.0023
10	0.00287	1.551 0.0039

22cm

1	0.03815	1.4167 ± 0.00078
2	0.00747	1.345 ± 0.0030
3	0.00041	1.055 ± 0.0391
4	0.00086	1.533 ± 0.0233
5	0.00164	1.585 ± 0.0071
6	0.00751	1.531 ± 0.0036
7	0.01388	1.512 ± 0.0022
8	0.01309	1.486 ± 0.0016
9	0.01531	1.546 ± 0.0021
10	0.00871	1.536 ± 0.0018

:

Jer Colin 219

			#2		#1
SL#	ΦJ	PJ	ΦC	PC	
1	0.710 ± .038	-0.109 ± .012	3.00 ± 0.10	0.070 ± 0.035	3.44 ± .07
2	0.796 ± .031	-0.076 .008	2.44 ± 0.16	0.019 ± 0.047	2.68 ± .33
3	0.680	-0.007 .028	2.82 ± 0.005	-0.222 ± .0016	3.38 ± .01
4	0.834 .121	0.009 .040	2.96 ± 0.017	0.226 .005	3.57 ± .03
5	0.767 .011		2.74 ± .055		2.75 ± .13
6	0.696 .063		2.83 ± .020		3.03 ± .14
7	0.642 .017		2.70 ± .103		2.86 ± .18
8	0.550 .012		2.36 ± .05		2.39 ± .07
9	0.688 .016		2.40 ± .19		2.40 ± .20
10	0.628 .007		3.07 ± .05		3.31 ± .11
11	0.701 .020		3.46 ± .07		4.30 ± .29
12	0.715 .023		3.50 ± .11		4.23 ± .37
13	0.459 .017		3.24 ± .08		3.29 ± .14
14	0.473 .135		2.11 ± .02		2.14 ± .02
15	0.378 .091		2.66 ± .02		2.73 ± .03
16	0.516 .098		3.30 ± .02		3.80 ± .03
17	0.504 .072		2.92 ± .016		3.13 ± .03
18	0.519 .118		2.62 ± .018		2.71 ± .03
19	0.603 .016		2.98 ± .017		3.23 ± .06
20	0.687 .009		2.78 ± .079		3.08 ± ?
21	0.711 .007		3.08 ± .059		3.27 ± .10
22	0.823 .070		2.91 ± .008		3.10 ± .17
23	0.765 .009		3.20 ± .067		3.10 ± .19
24	0.589 .005		2.57 ± .046		2.58 ± .03
25	0.607 .039		2.668 ± .004		2.75 ± .00
26	0.789 .036		2.552 ± .007		2.63 ± .0

27	0.653 ± .033	2.220 ± .003	2.25 ± .005
28	0.604 ± .030	2.748 ± .003	2.97 ± .005
29	0.586 ± .070	2.619 ± .004	2.66 ± .005
30	0.386 ± .014	3.030 ± .079	3.34 ± .24
31	0.751 ± .043	2.959 ± .006	average

```
From:   CVAX::GATEWAY::"DCLARKE@NCSA.UIUC.EDU" " (DAVID CLARKE)" 13-MAR-1990 18:00
To:     ABRIDLE AT NRAO
Subj:   Re:  Shocks again

Date sent:  Tue, 13 Mar 90 17:00:50 CST
Received: from VTVM1 by vtvm1.cc.vt.edu (Mailer R2.05) with BSMTP id 2805; Tue,
 13 Mar 90 18:00:30 EST
Received: from uxl.cso.uiuc.edu by VTVM1.CC.VT.EDU (IBM VM SMTP R1.2.1MX) with
 TCP; Tue, 13 Mar 90 18:00:24 EST
Received: from bardeen.ncsa.uiuc.edu by uxl.cso.uiuc.edu with SMTP
        (5.61+/IDA-1.2.8) id AA01125; Tue, 13 Mar 90 17:02:00 -0600
Received: from fermi.ncsa.uiuc.edu by bardeen.ncsa.uiuc.edu (4.1/NCSA-1.2)
        id AA01243; Tue, 13 Mar 90 17:01:07 CST
Return-Path: <dclarke@ncsa.uiuc.edu>
Received: by fermi.ncsa.uiuc.edu (4.1/NCSA-1.2)
        id AA01048; Tue, 13 Mar 90 17:00:50 CST
Message-Id: <9003132300.AA01048@fermi.ncsa.uiuc.edu>
To: ABRIDLE@nrao.bitnet
```

Hi Alan.

thanks for the message and for the vtess image of the 219 jet which arrived today.

First, re: BS
The toroidal component of the magnetic field points out of the computational plane. The shock normal lies in the computational plane (otherwise the imposed phi-symmetry is broken). Thus, all shock normals, regardless of their orientation in the computational plane are necessarily perpendicular to the toroidal field, and so will necessarily compress it. Thus, what I said previously about a highly oblique shock compressing the toroidal field less than a less oblique shock is nonsense. As Jack would say 'You can't believe those upity post-docs!'.

I don't think what you say about the fact that b-axial being pushed up regardless of the initial field configuration is at all inconsistent with anything that I've said. THAT is not part of the BS. The BS was only that I was stating that B-phi would not get boosted by an oblique shock but would by a more transverse shock. Drivel.

Second, re: B-phi going to zero on axis:
The toroidal component of any vector must go to zero on axis - otherwise its curl (the current density in the case of the B-field) will go to infinity and that's not good. In an axisymmetric calculation, one simply imposes a profile of the toroidal component that is proportional to the radial distance from axis. This has the physical interpretation of a uniform current density across the jet width, which is as good an initial condition as assuming a uniform matter density profile across the jet. Once the jet is launched, it can redistribute the curent density and the matter density in any way it deems fit - and it does with little memory of the initial profile. But the geometric fact remains that b-phi (and v-phi as well) must go to zero on axis. By the way, a radially varying v-phi corresponds to a uniform angular velocity. If v-phi itself is uniform across the jet, then its curl (angular frequency) will become infinite on axis, which is nonsense. Of course in nature which is intrinsically three dimensional, the toroidal field goes to zero on axis by twisting itself about into the poloidal plane, which it can do when the axisymmetry is broken. The bottom line becomes: Where there is significant compression along the axis

of the jet, there aint any b-phi. or at least its down by an order of magnitude or two compared to the amount of b-phi at the jet radius.

Note that although we start the calculation off as a CH field, it does not remain so. The radial dependence of the pitch angle becomes as compex as that of the density or any of the other parameters.

Third, re: vtess image of 219 jet.
Why are there -ve contours? I thought maximum entropy was positive definite. I did a crude estimate of the e-folding distance of the fall-off of the jet emission at the tip of the jet. I get about 0".3, or about two beam-widths, half a jet radius. Does this concur with any slices you've done down the jet axis? If so, then maybe this fall off is finally resolved and looks like it occurs across a good fraction of a jet radius, which is what I mentioned in one of my previous mail messages.

No, I did not see the photo of the SR-71. Do you have the newspaper clipping and/or copies? I would love to see it.

Cheers! David

From: CVAX::GATEWAY::"DCLARKE@NCSA.UIUC.EDU" " (DAVID CLARKE)" 13-MAR-1990 18:08
To: ABRIDLE AT NRAO
Subj: Re: Shocks again

Date sent: Tue, 13 Mar 90 17:08:07 CST
Received: from VTVM1 by vtvm1.cc.vt.edu (Mailer R2.05) with BSMTP id 2848; Tue, 13 Mar 90 18:07:33 EST
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Return-Path: <dclarke@ncsa.uiuc.edu>
Received: by fermi.ncsa.uiuc.edu (4.1/NCSA-1.2) id AA01052; Tue, 13 Mar 90 17:08:07 CST
Message-Id: <9003132308.AA01052@fermi.ncsa.uiuc.edu>
To: ABRIDLE@nrao.bitnet

Hi again

Something that struck me that I forgot to mention in my last blurb. The hook in the ridge line at the end of the vtess jet is really intriguing. It almost looks like the onset of a firehose instability which we see at the END of the slab-symmetric simulated jets. Maybe food for though, especially if there are wiggles in the ridge line before this obvious one whose amplitude grows like one of Phil Hardee's unstable jets.

David

From: CVAX::GATEWAY::"DCLARKE@NCSA.UIUC.EDU" " (DAVID CLARKE)" 12-MAR-1990 17:33
To: ABRIDLE AT NRAO
Subj: Re: Two questions

Date sent: Mon, 12 Mar 90 16:33:00 CST
Received: from VTVM1 by vtvm1.cc.vt.edu (Mailer R2.05) with BSMTP id 4050; Mon,
 12 Mar 90 17:32:26 EST
Received: from uxl.cso.uiuc.edu by VTVM1.CC.VT.EDU (IBM VM SMTP R1.2.1MX) with
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Received: from bardeen.ncsa.uiuc.edu by uxl.cso.uiuc.edu with SMTP
        (5.61+/IDA-1.2.8) id AA16225; Mon, 12 Mar 90 16:34:01 -0600
Received: from fermi.ncsa.uiuc.edu by bardeen.ncsa.uiuc.edu (4.1/NCSA-1.2)
        id AA10141; Mon, 12 Mar 90 16:33:09 CST
Return-Path: <dclarke@ncsa.uiuc.edu>
Received: by fermi.ncsa.uiuc.edu (4.0/NCSA-1.2)
        id AA00283; Mon, 12 Mar 90 16:33:00 CST
Message-Id: <9003122233.AA00283@fermi.ncsa.uiuc.edu>
To: ABRIDLE@nrao.bitnet

Thanks for resending the message. I did get that one, though the other day. The incomplete message that I got today seems to have been sent today. I guess it's just the mysteries of e-mail!

Answers to your questions:

1. Although we attempt to initiate the jet with thermal pressure balance with the ambient, we cannot achieve it perfectly for the simple reason that the ram pressure the jet encounters as soon as it slams into the ambient throws everything off. This is what I mean by "force balance" as opposed to "pressure balance" The jet is initially in (thermal) pressure balance with the ambient because we set it that way. But it is not in force balance because the ram pressure of the ambient is a new unpredictable ingredient to the equation. This imbalance trips the internal shocks at the jet-cocoon interface setting up the criss-cross shock pattern.

2. The velocity of the jet-shock (terminal Mach disc) goes as

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     v_{Mach disc} = v_{jet} * \sqrt{\eta} / ( 1 + \sqrt{\eta} )
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(I presume you read TeX?) where \eta is the ratio of jet to immediate ambient densities. This can be derived from simple ram pressure balance arguments. So yes, the Mach disc of the restarted jet would be expected to travel faster than that of the original jet. In our simulation, \eta of the restarted jet is about 4, so the ratio of v_{Mach disc} and v_{jet} is 2/3. This will reduce the relativistic effects, but probably won't kill them. On the other hand, the criss-cross shocks are stationary things relative to the jet nozzel. Oh, they fluctuate a bit back and forth, but in viewing an animation of a jet simulation, one is struck by just how still they are. So would the emission from these puppies be Doppler enfeebled or enhanced relative to the emission from the Mach disc, and mightn't we also expect to see emission from the (presumed) criss -cross shocks in the counter-jet? Would time-of-flight effects be observed for these features?

I would like to emphasise one point though. Whether the Mach disc is slowed or not, I still think there is a significant problem in using shocks to light up your reborn jet when the contrast between the emission from the Mach disc and the bow shock in the surroundings is so high. Have you any thoughts on this? This is a problem that only occurred to me upon rewriting the paper, and

I have only bounced it off Mike and yourself, so there may still be something glaringly obvious that I am missing.

Tally-ho!! David.

From: CVAX::GATEWAY::"DCLARKE@NCSA.UIUC.EDU" " (DAVID CLARKE)" 18-FEB-1990 18:35
To: ABRIDLE AT NRAO
Subj: 3c219

Date sent: Sun, 18 Feb 90 03:16:28 CST
Received: from UIUCVMD by VMD.CSO.UIUC.EDU (Mailer X1.25) with BSMTP id 5731;
Sun, 18 Feb 90 17:33:48 CST
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id AA17631; Sun, 18 Feb 90 17:33:43 CST
Return-Path: <dclarke@ncsa.uiuc.edu>
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Message-Id: <9002180916.AA01714@fermi.ncsa.uiuc.edu>
To: abridle@nrao.bitnet
Cc: dclarke@fermi

Dear Alan;

Well, at long last, the much talked about, elusive 219 paper, in its (I hope) next-to-last draft is on its way to you by US mail, sans updated figures. You will notice that:

1. Virtually every word of text has been changed - this is a complete re-write. ✓

2. The abstract and intro are almost verbatim as you suggested. I have made only a few modest changes to them. They were, in my opinion, much better than the ones I originally wrote. ✓

3. The intro to section V is a hybrid of what you wrote and what I originally had. I hope it is an equitable compromise. Although a substantial amount of the text you submitted was used, I rearranged the order a fair bit to eliminate bouncing back and forth between ``flip-flop'' and ``born-again'' models. You'll see what I mean. I was first floored by all the new references, but having digested what you wrote, I ended up using virtually all of them.

4. The conclusions are again a hybrid of what you submitted and what I originally wrote, with perhaps more of what you suggested.

5. I have tried to incorporate all of the changes suggested by yourself, Jack, and Rick in the margins of the respective copies of the last draft. The tables have been partially changed. They now include the columns requested by you and Rick (except for the sizes of the hotspots, which I will do for the final draft) but some of the numbers are awaiting revision when I get the data on the IIS at the Astro dept here (eg, background subtraction, reporting integrated fluxes over resolved features).

As for the figures:

1. I am suggesting we throw away the fractional polarisation E-vector images at 6 and 22 cm without Faraday rotation corrections, since I feel they are redundant with the Faraday corrected image. In addition, I suggest we keep the fractional polarisation grey scale images, since these are directly comparable

to the numerical observations of CNB. Note that this is exactly the reverse of how we left things after our meeting last May(?). In particular, the ridges of low fractional polarisation in the southern lobe are of interest and indicate, I think, evidence of some degree of 2-D symmetry. I discuss this in the new draft.

2. There will be glossies for
   - total intensity, 6 cm (Figure 1)
   - total intensity, 22 cm (Figure 2)
   - spectral index, 6 and 18 cm (Figure 3)
   - fractional polarisation, 6 cm (Figure 4)
   - fractional polarisation, 22 cm (Figure 5)
   - rotation measure (Figure 8)
   - restarting jet (Figure 11)
   - CNB figures (Figures 13 and 14)

Some of these may not need to be made into glossies appearing at the end of the journal. We'll see how they reproduce.

3. I am still debating on whether the depolarisation image (Figure 7) will be included in the final draft.

4. Do we REALLY need the position angle on Figure 6 as requested by either you or Rick? Since Figure 6 is only used as a comparison to Figure 16, I'm not convinced that chi is needed, and only clutters things up. I suggest Figure 6 is in its final form.

5. Since there is inertia (the way I think, CNB, the literature) in using E-vectors, I would suggest we use E-vectors in Figure 9. This is not to say that I think E-vectors are better or worse than B-vectors, just that the direction of my present momentum is to use E-vectors. Would that be OK with you? If so, then Figure 9 is pretty well in its final form.

6. Figure 10 has yet to be prepared.

7. I suggest we use the photos for Figure 11. They show the salient features well.

8. Figure 12 will be redone to reflect the more oblique nature of the criss-cross shocks.

9. I will make Figures 13 and 14 into glossies from the IIS.

10. Figure 15 will be redone with the AIPS slicing routines. In present form, they look too "hand-drawn" (for good reason - they are!)

11. I propose that Figure 16 is in its final form.

Points of science:

1. Do we want to make more of the V shaped feature joining the core to S1 (ie, the adiabatically expanding portion of the jet as suggested by BPH)? I have mentioned it in two places, but not quantitatively.

2. I do, as always, confuse fluxes, brightnesses, flux densities, etc, and

erroneously use these phrases interchangeably. Please feel free to choose the units you prefer and make the necessary changes for consistency throughout the paper.

3. Q: Why does the jet disappear in CNB?

A: In a flux frozen field, an expanding toroidal field component will produce more cocoon emission along the line of sight than the unexpanded axial field in the jet for initial pitch angles as low as 30 degrees, and a comparable amount of emission for pitch angles as low as 10 degrees. Thus, without field compression in the jet, the jet will generally be difficult to see against the background emission of the cocoon. THIS IS INDEPENDENT OF RESOLUTION. With field compression, as accomplished by oblique criss-cross shocks, the filling factor goes down, and the jet emissivity goes up making it readily visible against the background. In this case, as the resolution goes up, the flux density from the cocoon goes down, while that of the jet remains more or less constant. If the criss-cross shocks should disappear before the terminal Mach disc, the jet will seem to disappear as the axial field expands to fill the volume of the jet. The distance over which the jet disappears will be comparable to the dimension of the high pressure region behind the last shock-cell. Note that the point where the shock-cells end depends on whether the jet can reach force-balance with the surroundings before the Mach disc. This varies from jet to jet and time to time. Thus, a partial jet is a possible effect of a passive axial field, not a necessary one. I think all of this is in accord with what I've said to you before.

What I haven't said clearly is why the toroidal field is not similarly compressed by the criss-cross shocks. I made some mumblings about the normal of the criss-cross shocks being parallel to the toroidal field when the shocks were highly oblique and thus compression of the toroidal field by the criss-cross shocks is not expected. This, of course, is patently BS. The normal of all shocks in a 2-D calculation is in the poloidal plane, and therefore by definition is perpendicular to the toroidal field component no matter how oblique. What is true is that the location of greatest compression by the criss-cross shocks is in a small region (small compared to the jet radius) directly downwind of the point where the criss-cross shocks converge on the axis. So, while the toroidal field will, in principle, be compressed by the criss-cross shocks, there is no toroidal field to compress where the compression is truly significant since the toroidal field necessarily reaches zero on the axis. I make this point in the paper.

4. Q: Should we see a bow shock as an emission feature?

A: Not necessarily. But beware: the Mach disc and the bow shock bring their respective media into pressure balance and so must be of comparable strength (defined as downwind to upwind pressure ratio). Thus, it may be reasonable to expect that the two shocks result in two features of comparable brightness, especially if the surrounding plasma is old, magnetised jet material populated with free electrons. I don't think it is enough to simply state that we don't see a bow shock at this sensitivity and resolution, but perhaps with still higher sensitivity and resolution (the tired, age-old request of all astronomers!) we will. We must also address why a bow shock is not seen DESPITE THE BRIGHTNESS OF THE PARTIAL JET.

to compare with jet turn off in simulation.

5. On the subject of shocks. If we are interpreting the emission knots in the jets of 219 as shock features (at the tips, for example), then we must be careful to use the relativistic nature of the born-again model self-consistently. The flow of the jet material may be relativistic BUT, the oblique criss-cross shocks are relatively stationary, and the advance speed of the jet tip (the Mach disc) is substantially reduced for an underdense jet [v_{advance} / v_{jet} = sqrt{\eta} / (1 + sqrt{\eta})]. For \eta (the density ratio of the jet to ambient media) of 0.1, the ratio is 0.24. For \eta = 0.0001, the ratio is 0.01. Thus, the desired relativistic effects of the emission features (time-of-flight, spectral steepening of the counter-jet tip, Doppler favouritism) whose velocity may be substantially less than the flow velocity of the jet may not exist at all.

6. Do we want to make more of the filaments, such as what Hines, Owen, and Eilek did for M87? I resist going into the depths that they did, but perhaps some rudimentary analysis for completeness.

7. I have omitted all mention of the jet velocity experiments for Class I sources. I originally used these NULL results (that is, the data for M87 and Cen A are consistent with no motion over the time baseline of observations) as indicators that the velocities were at most mildly relativistic. In my opinion, v < 0.5 c is at best mildly relativistic, because even at 0.5 c it is difficult to explain a 10:1 emission contrast between the two jets. On the other hand, you used these results to indicate that even in weak Class I sources, there is evidence of (perhaps) mild relativistic flows, so what must be the velocities in the even more powerful Class II sources?. In retrospect, the fact that these results are null doesn't help either of our positions. Further, these velocities are very likely unrelated to the actual flow speed of the jet plasma (probably much lower, I admit, but it is the velocity of the features, not the flow speed, responsible for relativistic nature of the observations). So, I have removed these references altogether.

Well, this has gone on long enough. You should receive the paper shortly, and these comments will make more sense. I encourage comments and criticisms on this draft, while at the same time I hope that we will be able to get this submitted soon. You might also bear in mind that this paper has almost doubled in length since the original version. Jack is concerned that it may be in danger of being relegated to the supplement series, particularly if it gets any longer. As we mentioned before, I think we can spare the rest of the authors this version of the paper. Once you and I come to an agreement on what we want in the final draft, the others will almost certainly be satisfied.

Thanks for your patience on this Alan. I hope you find this draft at least partially worth the wait.

Best wishes and happy reading!

David.

PS, I have included a copy of CNB and a copy of the version of the restarting jet paper as submitted to Ap. J.

VII. SUMMARY

We have imaged the radio galaxy 3C219 with the VLA at four frequencies. The major features of the source include: an abbreviated jet that appears to extend only about a third of the way from the core to the terminal hot spot; a knot near the core on the side opposite the jet, which may be the brightest part of a counterjet; extended filamentary lobe emission (a "cocoon") that surrounds the jets but also extends away from the major axis of the source on both sides; an extended confusing source that blends with this cocoon to the north and west of the core; an extended L-shaped hot spot in the jetted lobe; and an edge-brightened circular hot spot in the counterjet lobe. We have derived the distributions of spectral index, fractional polarization, rotation measure, depolarization and apparent magnetic field configuration over these features. The spectral index distribution of the extended emission contains evidence for secondary outflow from the hotspots toward the outermost edges of the lobes. The magnetic field of the jet is apparently axial, but is replaced by an apparently transverse field once the jet "disappears". The apparent field runs parallel to the boundaries of both lobes over most of the extended structure, except for a disturbed region near the jetted lobe's hotspot and a circumferential-field region around the circular hotspot in the counterjet lobe. The large-scale rotation measure and depolarization gradients across the source are both small, but there are small transverse rotation measure gradients in the jetted (south) lobe, consistent with a large scale toroidal component of the field in this lobe.

We have considered two alternative explanations for the abbreviated appearance of the main jet in 3C219 - a "born-again" relativistic jet model and a shocked steady-jet model. These explanations differ fundamentally in their assumptions about the relationship between the synchrotron emissivity and the energy flux along the jet. The "born-again" relativistic jet picture, previously discussed by BPH, attributes the abrupt disappearance of the jet to an interruption of the energy flow to the lobes. This picture accounts for the asymmetries, both in brightness and in geometry, between the main jet and the counterjet. It can also explain why the high-frequency spectral index of the counterjet is steeper than that of the main jet. In this picture, the hot spots and the lobes are remnants of earlier episodes of activity in the core, which may account for the relaxed appearance of the hotspots in 3C219. An attractive alternate picture has, however, been suggested by the numerical simulations of steady-state jet propagation with passive magnetic fields previously discussed by CNB. These simulations show that the "disappearance" of the synchrotron emission of a jet within its cocoon does not necessarily imply an interruption in the energy flow down the jet. Instead, as the jet adjusts its internal structure to come into pressure balance with the cocoon, a pattern of oblique internal shocks can compress the axial component of the magnetic field sufficiently to enhance its total brightness contrast with the lobe. The "disappearance" of such a jet can be attributed to the disappearance of the shock pattern where the jet reaches pressure balance with the cocoon, rather than to episodic outflow from the core. When such an intrinsically steady shocked-jet reaches pressure balance, the field expands to fill the volume of the jet, and the emission along the line of sight becomes dominated by that of the cocoon. The numerical simulations show that the apparent magnetic field on the axis of such a jet rapidly swings from axial to transverse, as observed in the south lobe of 3C219.

These two interpretations of the "disappearance" of the main jet in 3C219 have distinctly different ramifications for the general question of why the jets in powerful double sources are almost invariably one-sided. The "born-again" jet model is an "episodic" variant of the models in which the one-sidedness of the kiloparsec-scale jets in strong sources is attributed to Doppler favoritism produced by bulk relativistic motions. This model is supported by the evidence from depolarization asymmetries that, in double sources with one-sided jets, the brighter jets are almost invariably on the side of the source that is inferred to be closest to the observer. The assumption of intermittent, i.e. restarting, jets is also supported by the evidence for core variability in some classical double radio sources. The "shocked-jet" model offers an alternative explanation of the one-sidedness based entirely on intrinsic properties of the jets. The oblique shocks which must be present before a jet comes into pressure balance with its environment must also enhance the jet brightness for some distance from the core. The magnitude of this brightness enhancement depends strongly on the shock angle and on the initial ratio of axial and toroidal field components in the jet. The brightness asymmetry between jets and counterjets in powerful sources in general might therefore be derived from an asymmetry in the initial field configurations in the jets on the two sides of the central "engine". If one jet initially carried a helical field with a low pitch angle, while the other carried a field of similar strength but with a larger initial pitch angle, the jet with the lower pitch angle would remains brighter until force balance is reached. This model therefore suggests an explanation of the dominance of apparently axial field in the one-sided jets of powerful sources, as well as for the apparent disappearance of abbreviated jets like that in 3C219.

The numerical simulations discussed here also show that the magnetic structures transported into the jet cocoons by passive jet fields with a mixture of axial and toroidal field components strongly resemble those inferred from the polarimetry of the lobes in 3C219. Fundamentally, the toroidal component of the field must dominate after expansion into the cocoon. Both the apparent field configuration and the (small) rotation measure gradients observed over the south lobe of 3C219 are consistent with such dominance of the toroidal field component in this lobe. The simulation also shows that the trend for the magnetic field lines to follow the outer boundary of the lobe "caps" beyond the hotspots can also result from passive expansion of the jet's magnetic field. The generality of this result means that the polarization properties of the lobes cannot be used to distinguish between the steady-jet and restarting-jet models for the jet/counterjet asymmetries, but are consistent with both types of model.

How, then, can these alternatives be distinguished ? The different roles played by shocks in the two models may provide important discriminants. The "born-again" jet model invokes shocks in the jet outflows at the advancing tips of the restarting jets to remove the Doppler favoritism of the main jet over the counterjet (and thus to make the tip of the counterjet clearly visible). The shocks in the jets must be accompanied by stand-off shocks in the material along the jet path and in the surrounding cocoon. As these media contain relativistic particles and magnetic fields left behind by earlier activity, these stand-off shocks may be observable as regions of enhanced synchrotron emission at high sensitivity and high resolution. Successful searches for internal "bow shock" features near where bright jets disappear in sources like 3C219 would favor the "born-again" jet model. Evidence from hotspot morphologies or spectra for that the electron populations of these sources were older than in those with continuous jets might also support the assumption of episodic activity. The "shocked-jet" model predicts an absence of bow shocks, and "normal" hotspot morphologies and spectra. Furthermore, it requires that the brightest parts of the disappearing jets do not fill the jets volumes, but expand to fill these jet volumes once the jets come to force balance. Sensitive high-resolution radio observations of abbreviated jets should therefore show that the filling factor of the emission is significantly less than unity in the brightest regions, but that the jets become fully filled immediately downstream from the region where their brightness declines and the axial field ends. So far, the evidence for filling factors less than unity has been confined to jets in nearby weak sources such as M87 and Centaurus A. The observations required to test this prediction of the model in more distant, powerful, sources may be technically challenging. A second prediction of the shocked-jet model is that the magnetic field configurations in the counterjets of powerful sources, if they could be detected, would be dominated by transverse magnetic fields. The required counterjet polarimetry will also demand both high sensitivity and high angular resolution.

Both types of model make strong assumptions about the nature of the outflows from the central engine. Intermittency and bulk relativistic velocities are fundamental to the "born-again" jet model, the initial magnetic field asymmetry is fundamental to the "shocked-jet" model. Either model for the large-scale appearance of sources such as 3C219 could therefore be encouraged if a strong theoretical foundation appeared for the appropriate ingredients.

This work was partially supported by NSF grant AST 8611511 to J.O.B. and M.L.N. A.H.B. thanks V. and S. Blanc for support and encouragement throughout this project.

From: CVAX::ABRIDLE 10-APR-1989 17:07
To: BITNET::"dclarke@unmb",ABRIDLE
Subj: 3C219 questions

Hi Dave

All computers at the AOC are down and I couldn't reach you by phone, so here's the only route that worked today!

You should have got 2 E-mails from me over the weekend with misc levels of comment on the paper. Here are some things I'd like to talk with you about. I'll keep trying to reach you by phone but if not try me at 1-835-7282, or E-mail back to CVAX::abridle.

I think we need to make it crystal clear to the reader why your simulated jet disappears part-way out. This may need to have two things spelled out. 1) The interaction between the shock cell structure and the field, 2) what happens when the shock cells go away. I can see that fully longitudinal shocks compress toroidal field and not poloidal; fully transverse shocks would compress poloidal field and not toroidal. So I expect oblique shocks to work on both, and the field balance to depend on the shock angle. Are youimplying something about steepening of the shock angle toward longitudinal as the jet propagates outward ? Also, you say that the poloidal field is ultimately lost due to expansion. Are you assuming that the jet expands rapidly past the "disappearance point"? If not, I'm not clear why the poloidal field falls off at all once the shock adjustments are over.

Is a test for this model that the jet should not be fully filled in the bright regime at high resolution, i.e. should we only be seeing regions where the field has been especially compressed ?

On other points:

I did an inventory of the 3CRR (Laing, Riley and Longair) sample. It contains 96 FRII radio galaxies, of which 3 have disappearing jets (born-again candidates). So the occurrence rate of these things is about 3% based on present statistics (lots with no jet detected at all, of course). The more general trend is for the jets to get brighter as they enter the stronger lobe emission, the reverse of what we see in 3C219.

In order to make the u,v sampling arguments explicit, I need some numbers from you - the shortest projected baseline lengths in each of the 6cm coverages (e.g. from a UVPLT), and the FWHM's of each of the Gaussian components you put into your simulated 3C219 (I mentioned the need for these in my October comments but I guess there were so many comments you didn't notice that!).

Also essential for interpretation of your dummy spectral index map is to know how much noise was added, and at what signal to noise you blanked the spectral index map. No rush on that, but those numbers should go into the paper. If you send me the shortest baseline lengths and the component sizes, I'll write up the rest of the u,v sampling discussion based on my VLAPLAN graphs.

Cheers, Alan

blanked on <u>α range.</u> ⇒ "effective s/n" blanking.

From: VAX3::AHBRIDLE "Alan Bridle" 9-APR-1989 22:19
To: BITNET::"dclarke@unmb",BITNET::"aips@unmb",RPERLEY,ABRIDLE,AHBRIDLE
Subj: 3C219 latest draft

Dear 219 co-conspirators,

I'm going through David's rewrite of the 219 paper. It's in pretty good shape, and most of my comments will be minor ones. I'm going to accept his invitation to rewrite the section on u,v sampling and missing flux density, etc. and will send some text in the next couple of days. My substantive comments come down to two things.

1. I'm still not completely clear why the model jets disappear so *promptly*. I gather it's connected to the expansion losses of the poloidal component but I'm puzzled because neither the simulated jet nor the actual one in 219 were expanding. If I'm still missing something, I think it's missing from the words in the paper and needs to be added, as other people will probably miss it too.

2. I believe that David's enthusiasm for the new model (which I share, subject only to the above caveat) has led him to underplay the strong points of the alternative models. So I'm proposing a new introduction to Section V, to be followed by David's discussion of the restarting-jet simulation almost "as is". (By the way, I'm not worried by the statistic that "not many sources should be seen in restarting-jet mode"; not many are. At most 2% of the 3CR, I'd guess not more than 4% of the 3CR FRII's, but I'll try to check that statistic before we finalize).

What follows has 3 parts: 1) my suggested new intro to Sec. V; 2) most of the references that come with this new text, 3) some rambling notes that may provoke some discussion about the model alternatives when we meet. I'll bring detailed wording suggestions for the rest of the paper to ABQ with me, and will talk with David later this week to prepare the ground. I think we're getting close to a final version if you don't all go through the roof at what I've written below!

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V. "RESTARTING JET" MODELS FOR 3C219

3C219 is one of a subset of the 3CR radio galaxies in which a jet that is well-defined near the radio core "disappears" before reaching its presumed terminus in the lobe. Other examples are 3C33.1 (Rudnick 1985), 3C288 (Bridle et al. 1989) and 3C445 (W. van Breugel, in preparation). These all have approximately the same total radio power as 3C219. The range of models for jet "disappearance" matches that for the dominance of "one-sidedness" in the jets in powerful sources. Such models can be divided into two main groups according to whether or not they take the synchrotron emissivity to be coupled to the total energy flux down the jet. We will discuss models of both kinds in this paper.

All models that assume that the synchrotron emissivity is coupled to the energy flux invoke some form of "episodic" or "restarting" jet behavior to account for the full range of behavior seen in FR II sources. Long-term episodic behavior is plausible because there is already evidence for significant year-to-year fluctuations in the radio output of the cores of extended radio sources. High-frequency (>= 5 GHz) variability has been documented in the radio cores of several lobe-dominated sources (e.g. Wills 1975; Hine and Scheuer 1980; G\"otz et al. 1987; Alef et al. 1988; Duric et al. 1987, 1989) and the "problem" of core variability is familiar to VLA observers who combine data from different VLA configurations that are well separated in time. The "restarting jet" models postulate that the energy outflow from the central engine is 100% variable on still longer time scales. This extrapolation is plausible if the power spectrum of the core fluctuations rises toward lower frequencies, as it does in many compact variable sources.

"Restarting jets" were first postulated (e.g. Rees 1976, 1981; Willis et al. 1978; Rudnick and Edgar 1984) as a way to explain the strong asymmetry of large-scale jets in symmetric classical doubles without invoking bulk relativistic effects (Scheuer and Readhead 1979; Blandford and K\"onigl 1979) on kiloparsec scales. They were envisaged as unipolar outflows, i.e. as flows whose energy flux is intrinsically asymmetric at any time, but in which the sense of the asymmetry could be reversed when the jets shut down. Theoretical models for this "flip-flop" behavior have since been offered (e.g. Icke). Unipolar-flow models might explain "disappearing" jets as in 3C219 by an unusually short "duty cycle" of the outflow on either side relative to the lobe lifetime. Unipolar flip-flop models were encouraged by the source-geometry statistics of Rudnick and Edgar (1984), but their results were not confirmed by Ensman and Ulvestad (1984). More recent developments have had equivocal implications for the unipolar restarting-jet models. The discovery of depolarization asymmetries that correlate almost perfectly with jet sidedness has encouraged the bulk-relativistic flow model (Laing 1988; Garrington et al. 1988). Evidence has also accumulated for high jet velocities at the bases of kiloparsec-scale jets in weak Class I sources (Benson et al 1989; Hine and Owen 1989). This makes it even more likely that relativistic effects contribute to the asymmetries of the jets in powerful Class II sources. On the other hand, reanalysis of the geometrical asymmetries of double radio quasars (Hutchings et al. 1988) matches the predictions of the flip-flop model.

A second class of bipolar "restarting jet" model was discussed by BPH following their discovery of the counterjet in 3C219. This model may be applicable to the other moderately-powerful "disappearing jet" sources. This model postulates long-term variability of the central engine, mildly relativistic flow velocities, and bipolar flow without a "flip-flop" mechanism. Its simplest form has been dubbed the "born-again" relativistic jet (Bridle 1988; Bridle et al. 1989). It predicts that in sources whose main jets "disappear" on the way to the lobe the tip of the (receding) counterjet will appear to be closer to the core than the tip of the (approaching) main jet, that the brightness ratio between the tips of main jet and the counterjet will be lower than the ratio closer to the core (due to deceleration at shocks near the tips), and that any spectral difference between the main jet and the counterjet will be in the sense of the counterjet having the steeper spectrum (due to differential Doppler shifting of any intrinsic spectral curvature). All three predictions were matched in 3C288 (Bridle et al. 1989), although the magnitude of the spectral difference in this source may be larger than expected in the simple form of the model.

We have examined both the unipolar and bipolar restarting-jet models using our new data on 3C219.

In the unipolar, "flip-flop" model, both lobes of 3C219 would be the remnants of jets from earlier epochs, while the main jet (perhaps only a few hundred thousand years old) is now boring its way through this old material. The "gap" between knot S1 and the core would indicate the decline in activity on the south-west side of the central engine, and the presence of knot N1 the most recent increase in activity on the north-east side. The model's weakness is that it makes few predictions because the flip-flop mechanism is indeterminate. Its only prediction about the large scale structure would be that if the flow velocities are nonrelativistic, the brightest features near the core, which reflect the most recent flip-flops, should be anticorrelated in separation from the core. Indeed, knot N1 lies in the "gap" between the core and knot S1 if we "fold" the north-east structure onto the south-east on a line through the core at right angles to the major axis of the large-scale structure. The larger-scale structures show now evidence of such anticorrelations.

however. The ad hoc nature of the flip-flop model makes it difficult to establish or refute from the available data, as discussed by BPH.

In the bipolar "born-again" restarting-jet model, the abbreviation of the main jet and the counterjet are both real, and the brightness ratios and geometrical asymmetries between them are explained entirely by Doppler favoritism and time-of-flight effects. As pointed out by BPH, this model has an advantage over the flip-flop model in that it explains why so little of the counterjet is seen, and why the leading edge of the counterjet (knot N1) is closer to the core than the leading edge of the main jet (knot S4). The fact that N1 is closer to the core than S1 is, in this model, coincidental. Our new observations test the remaining prediction of the "born-again" jet model by providing a spectral comparison between Knot N1 and Knot S4. Between 18cm and 6cm, where the spectral indices are most accurately determined, Knot N1 indeed has a steeper spectrum (0.93 \pm 0.02) than Knot S4 (0.736 \pm 0.003). As in 3C288 (Bridle et al. 1989) the high-frequency spectral difference has the sense that is required by the model if the intrinsic spectrum steepens with increasing frequency as expected. But, also as in 3C288, the less accurate lower frequency spectral indices do not substantiate the spectral curvature. The 22cm to 18cm spectral index of Knot N1 (0.58 \pm 0.07) suggests that the required curvature is present, but that of Knot S4 (0.75 \pm 0.02) does not. The spectral differences revealed by our data therefore encourage the "born-again" jet model but do not conclusively favor it.

Because the bipolar restarting-jet model can account for the major jet/counterjet asymmetries, we have explored new observational consequences of this model using a numerical simulation. A full account of the simulation will be presented elsewhere (Clarke and Burns, in preparation) but we summarise the most relevant details here.

(Continue with David's text on the restarting-jet simulation, with a few minor modifications that I will bring to ABQ with me).

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References added by the above insertion:

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Things to think about and maybe discuss re this section:

a) Low-latitude surveys of sources in the galactic plane have found things that look like core-variable extragalactic triples with large amplitude variations on time scales of years (Duric et al. AJ 93, 890 (1987) and Nature 337, 143 (1989)). The evidence is from 20cm and 6cm; can we exclude interstellar twinkling as the cause at such high frequencies? Probably, though the Fiedler "occultations" may say no. It's a pity that the only large-scale surveys that could detect such variability systematically have been at low latitudes.

b) 3C111 (e.g. Goetz et al. A&A 176, 171 (1987) is a radio galaxy with structural and amplitude variations in the core on time scales of a few years; it has superluminal motions as well as large-amplitude variability, despite a 200" double lobe structure. It's also low-latitude but has had large variations at 3.5mm, I think well above any conceivable effect of the ISM of our galaxy. It has about 30% of its 6cm flux in the core.

c) 3C390.3 is a 213" double with 10% flux in the core at 6cm, yet the core shows rapid structural changes on time scales of years and is presumably variable, though I can't find a detailed flux history in the literature.

d) There's a long folklore at the VLA of problems with combining data from different VLA arrays for radio galaxies because the cores have changed in flux.

The theory of "feeding the monsters" is only crudely developed but has no features that would legislate a steady state; outbursts seem more likely than strict steady state behavior.

So, although Occam's Razor, gross bilateral symmetries and computational convenience mean we do not jump into unsteady models at first sight, there are also decent reasons for considering them, whether in their unipolar (flip-flop) or bipolar (born-again jet) forms.

The unipolar model is the flip-flop. What's the state of the evidence for or against it? There have been persistent rumors of evidence for anticorrelation of features from side to side in large scale sources. This began with Rudnick (IAU 97) but became unpopular after the analysis by Ensman and Ulvestad (1984). Hutchings, Price and Gower (1988 - Ap.J. 329

122) have done the most recent analysis, and explicitly refute the Ensman and Ulvestad conclusion. They find an excellent fit of core-lobe distance distribution in their QSR sample to a flip-flop model with an average g=6 (i.e. with reversal after one-sixth of the source lifetime). There also exist fully one-sided sources that might be the extreme end of a flip-flop distribution (g=1), with one-sided ejection for most of the lifetime of the source.

The bipolar (born-again jet) model says nothing about the lobe polarizations not because it can't, but because it's not even trying to. (I want to modify David's language on this point in several places, as he implies that the restarting-jet models somehow fail in this respect.) It's a model for the jet symmetries, not for the lobe polarizations. But there's nothing to stop the born-again jet model from appropriating a field geometry that explains jet polarizations, e.g. the randomized Chan-Henriksen field, letting it passively expand out into the lobe following David's precepts, and then discovering (via David's simulation) that the field has become mostly toroidal, in agreement with the data! They could can then say "ha, we've accounted for *everything*, including the detailed jet-counterjet relationships".

The point is surely that *both* David's model and the born-again jet can account for some jet properties and for the lobe polarization by adding one assumption to passive expansion of an initially mixed poloidal-toroidal field in the jet. In David's model, the added assumption is an initial condition with a fundamental asymmetry (different starting fields on the two sides). I'm perfectly happy to point out that this one assumption gets you a long way to explaining most of 3C219. (But why don't we see an RM gradient in the north lobe?) But let's not overlook the fact that the born-again jet model's basic assumptions (episodic behavior and relativistic flow) are only small extrapolations from attributes that have been observed. Or, that long-term steady state behavior is itself a simplifying assumption. If there was no evidence for core variability, or for high (not low, David!) jet velocities on kiloparsec scales from proper motions, the born-again model would be a big extrapolation. But given everything else we know, I don't find it so. I hope David will agree that the paper should contain some discussion of these points.

My view of the *critical* question is still the one I had in October: does the new restarting-jet simulation predict features that clearly conflict with the 219 data? I'm waiting to see Figure 13 to judge that.
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I look forward to our get-together on the 20th,   A.

From: CVAX::ABRIDLE   12-APR-1989 18:48
To: BITNET::"dclarke@unmb",ABRIDLE
Subj: Rick's comments

Hi David,

I got together with Rick Perley for a discussion of the 219 paper and he had some comments that had not occurred to me but with which I basically agree. Just so you can be thinking about them before we meet, I'll pass them on (Rick is snowed under with preparations for his trip to UCLA next week).

He doesn't understand the paragraph in the middle of p.10 re estimating importance of type 1 depolarization from low-resolution fractional polarization maps. When I read it through again, neither did I. We're also both concerned that the "speckled" behaviour of the depolarization imagein Figure 9, and the fact that the depolarization goes as high as 1.6, mean that thedata were not blanked at a high enough level. So we're wondering how much of the depolarization to believe.

We'd both like to give some statistical errors in places where they are now absent. One is on the spectral indices in the "fans", the other is on the RM gradient across the S. lobe. We're worried that the gray scale in Figure 10, especially after the usual poor reproduction in the journal, will not convincingly show the RM gradients are monotonic or signficant. <aybe either cuts, or estimate of the statistical errors in the RM gradients, or both, are needed if there isn't a gray scale image that shows the effect more clearly.

Rick was also concerned about integrated flux densities for hot spots, as he says optical observers plan searches for optical synchrotron emission from these and needto know the effective aperture sizes across which the flux densities have been integrated. So he would like to add the sizes of the integration regions to Table 3. I also wonder if the background was properly subtracted when the flux densities of BC were estimated. The quoted spectrum is extraordinary for a core in a radio double, but I remember from our full resolution map that the core was very much a point source on a confused extended background. Are you sure you integrated only the unresolved component here? Did IMFIT agree with IMEAN?

We both suspect that the lower polarization you observed in the jet at 6cm may simply be a resolution effect rather than a beam depolarization effect. (p.18). We may check this by simply convolving down our old data.

Rick asks where you got the "canonical" hot spot parameters from in the footnore on p.25. He doesn't believe the characteristic times you're quoting (thinks they're much too long) but more generally wonders whose "canon" you're quoting. I am not up enough on hot spot parameters to argue with him; be forewarned!

I'll work on the summary section later this week. I'm also beginning to wonder about the introduction section, which sort of self-destructs around the issue of whether active-field sources build cocoons. Maybe if we emphasize less why you originally took the new data, and more what issues we have been drawn to as a result of it, it will read better. I'll take a crack at that, also.

I took a crack at explaining in detail to Rick why your jet disappears (he had not got it fully from reading the text, either). After I had convinced him, he suggested a diagram addressing that point explicitly. Maybe it could be merged with Figure 14. By the way, I was a bit troubled drawn in at this angle ?

Finally, in Figure 17, Rick thinks the brightness profile on the lobeward end of the data for 219 looks much more unresolved than the one from the simulation. I admit it's not completely convincing. But perhaps more important, we have that profile at the full resolution from the BPH data, and could compare that with a simulation if needed. Are you happy that all is well if the experimental cutoff is really very sharp ?

Rick will be here until Friday, then gets back on Wednesday evening.

For the more distant future:
I think we should test the filling factor of the jet, the sharpness of the cutoff, and the presence or abssence of the bow shock, with VLA data at higher resolution. A array at 3 and 2 cm would probably be a very good idea. Will you guys be up for *another* round on this (after this paper has been sent off) ?

Cheers, A.

David

We seem to be having trouble with the BitNet gate at NRAO so I downloaded this abortive Email message from CVAX and resorted to the old-fashioned approach — USMail.

Also enclosed is Paddy Leahy's map of 3C228.

From: CVAX::ABRIDLE 9-SEP-1988 17:44
To: JBURNS,DCLARKE,RPERLEY,ABRIDLE
Subj: Last (I promise!) major comments

Here's a final batch of comments on the draft paper. I'm sending the copy with the typos, English, and nit-picking stuff to David. This lot are those that affect the science.

1. Disappearing jets are the key to this paper. We are trying to choose between explanations of why they disappear. David is arguing that if the field in the cocoon comes passively by expansion from the field in the jet, the jet is hard to see against the cocoon if the jet field is initially toroidal, or after it becomes toroidal. I have two questions. (a) Exactly why does the field become toroidal at the appropriate place---is the model jet expanding? (3C219's isn't). (b) In the simulations, the jet isn't actually invisible, it's just hard to see against the cocoon---could it be made visible with a high pass filter (such as the VLA's A configuration). It's not enough to make the jet hard to see, we know from the A array data that you gotta turn it off. Altogether (down to the noise).

2. How much gain for how little pain? David's argument in favor of the model is that it explains more for less. A fine criterion, but we must be fair about the accounting, to make the case. The strong point of this model is that it shows how the cocoon field structure can come for free if you start with the right jet field structure (and as one of the early backers of helical field models for jets I like the starting premise a lot, because I've been saying all along that they can explain the detailed correlations between degree of polarization and apparent field geometry in the well-resolved jets, too!). But it doesn't directly explain the uniformly one-sided appearance of all the jets in powerful sources (why do they *all* have the proposed difference in starting field configuration between the two sides, while lower power sources rarely do?). Nor does it explain why we see any part of the counterjet.

David has been particularly unfair to the flip-flop. I don't like the flip-flop model much, because it isn't a model, it's an ad hoc description. But the flip flop can "explain" why you see something on *both* sides and why the knots on the two sides anticorrelate (i.e. why the only peak in the counterjet lands in the gap in the main jet). The flip flop needs some sort of unspecified switch in the central engine, a "floppy disk"---but central engine theorists so far have been quite happy to come up with these on demand. I don't know how many of them, or which, to believe, and I think that's irrelevant here. The point is that the flip-flop traces some aspects of the appearance back to an asymmetry between the two sides of the engine, *and so does David's model*, via the starting field structure. So they're roughly even on that score.

The "born-again" relativistic jet model can explain all aspects of the total intensity distribution of the jet and counterjet, using velocities that we may need anyway in order to transfer enough energy to the lobes using a reasonable mass flux. It accounts in one fell swoop for the brightness ratios, the geometrical asymmetry, and the high frequency spectral difference. It also allows the main jet and the counterjet to vanish without a trace even under a high pass filter. A mildly relativistic backflow could also help to explain the circular hot spot in the north lobe, after Wilson and Scheuer (1983). That's not bad shooting, and that's of course why BPH emphasized the model. It says nothing about the field configurations in the lobes, but only because it doesn't try to. The main notion of David's model, that that the field in the lobes has come from the past activity of the jet, could of course be grafted onto the "born-again" model, too. It *does* "predict" that there are shocks at the ends of both the jet and the counterjet, so we should see B-parallel convert rapidly to B-perpendicular at the tips of both of them. What little data there is on this point says this prediction is right---there is evidence for depolarization, and swinging of the vectors, at both jet tips on the high-resolution images. But it's marginal. But could we tell the difference between this and the behavior in David's model---another reason why the physical reason for the field flip in the simulation needs to be talked about some more.

To summarize, there are 3 models that can do good things. We need to weigh how many good things they do against how many assumptions they make. Let's do it carefully.

3. Laing's lobes.

David has not commented at all on the now-ancient Laing lobe model (Ap.J. 248, pages 95 and 99, also Figure 6). We gotta say something, because it too fits the lobe data, and either the flip-flop or the born-again relativistic jetters can choose to ejaculate into it if they wish. Then they get the lobe picture for free, leaving us arguing about the jet and counterjet (which strictly speaking shouldn't be there at all in David's picture). What's wrong with the Laing field ? One problem may be that it's not a solution of Maxwell's equations, it's a postulate. In other words, he doesn't say how extragalactic sources make it. David's model does say how to make the field in the cocoon from the one in the jet. But can we go further to say that the Laing field can't be made? That would be a strike against it. The Laing field also should not show transverse RM gradients, while David's field can (that's why we proposed this test in BPH). Score one for David. But to emphasize the point, I'd like to see (a) a statement of statistical significance for the RM gradient---at what confidence level can we rule out just a random RM distribution over the south lobe, (b) something said about the RM gradients, or lack of them, in the *other* lobe. We got a 2-sided source, and can't just choose the side that suits us, unless the statistics on that side are overwhelming. I'm not clear, from reading the paper, that they are.

Incidentally, the statement in the second line of the second paragraph of p.24 has got to go, because it's wrong. It says that the E-vector configuration in Figure 3 implies a toroidal field component. It doesn't, and an important aspect of field diagnosis from radio data (emphasized by Laing and again by BPH) is that you can't tell the 3-D field configuration just by looking at the E-vector orientations. You gotta match to the fractional polarization and the RM too. The Laing model also fits the E-vector orientations (by eyeball inspection) and the fractional polarization. The RM gradient is the key, and you *just can't say* what the draft says on p.24.

4. The hot spots

A fair bit is made in the text about the brightness distributions and the fractional polarizations and apparent fields in the hot spots. That's good, and this is the right place to do it. But I think these are important enough that we should add clear displays of their properties---gray scales and/or contours and p,chi vector displays for just the hot spot regions from the high-resolution data. These will make many of the points about the hot spot morphologies much more appreciable to the reader. Once this paper is out, there won't be another good place to show this stuff for 219, so we should take the opportunity here.

5. Lobe filaments

These are referred to several times, and are offered as evidence that the field is not active. Let's get a display that shows them, and identify them for the reader. If we can't bring them out in a simple gray scale, the Sobel filter in AIPS will probably do the trick (it's inside NINER). Or any other mask you'd like to use. But I don't think we should refer to them so much without demonstrating to the reader that they're there. They are not as obvious as those in Cygnus A, which most people will take as the benchmark, so let's work on convincing people that they're not just a figment of our imagination.

From: CVAX::ABRIDLE 8-SEP-1988 10:09
To: RPERLEY,JBURNS,ABRIDLE
Subj: 3C219 paper p.8-p.9

Re the "spectral index simulation" discussion.

1. Everything said about imaging without the zero spacing or primary beam correction should be deleted. It's obvious that you add zero spacing flux densities and do the primary beam corrections when doing this sort of work. If it wasn't obvious to Dave when he started, I'm sure it is now! ✓

2. The discussion is not meaningful unless the angular sizes of the model components are specified. Also, the method of flux density estimation should be given. CLEAN components? Pixel summation? Do!

3. Was noise added? Was a realistic noise cutoff then applied when estimating spectral index? Was the CLEAN run to convergence in the presence of the noise? Was a residual zero level correction made to correct any failure of an incomplete CLEAN to compensate the cereal bowl effect? Without such details, I can't judge how seriously to take the "worst case error" of 0.7 in the spectral index. For example, would those pixels have been blanked out with a sensible noise level cutoff? Do!

4. To see how much of this can be predicted from elementary considerations, I ran the observation design through my VLA Observing Strategy Planner worksheets (the VLAPLAN program). All you really need to know is that a Gaussian component of FWHM X arcseconds falls to half amplitude at 91/X kilowavelengths, and the scale of the inner uv coverage of the VLA. If we take 60" as the FWHM of the largest circular thing in 219 (the North lobe cocoon), its visibility falls to about 0.6 on the shortest baseline present in the 6cm C array on the meridian (roughly 80m), and to about 0.78 on the shortest baseline present at +/- 5 hours HA (roughly 55m). The C array therefore must "miss" about 15% of the flux density of the largest feature even in a full synthesis, and 40% of it in a meridian snapshot. That's without having the chance to lose some more of it via CLEAN. You flat out need the D array to be sure of sampling a 60" component properly at 6cm. I'll send the curves from the worksheet program if you're interested.

I think Dave's result about missing flux is obviously correct on elementary grounds. If he took about 60" as the FWHM of his "cocoon" component, you'd expect to have trouble recovering about 0.2 of the 0.5 Jy from any C array data at 6cm, certainly unless the CLEAN was driven deep into the noise compared with the 20cm one, and likely not even then. What I think we actually need is an estimate of the spectral index uncertainty resulting from the lack of D array data, on the steepest spectra that are actually passed to a gray scale or given in a Table, plus the additional details of how he did the simulation. I also suggest including the elementary sums based on Gaussian component size and the actual shortest baselines in the coverage, to demystify the situation.

Note that the observations done in 1982 and 1983 were intended specifically to get high-resolution images of the jet, not of the large scale structure ! It's staggeringly clear that you can't image the large structure just with the B array at 6cm!

From: CVAX::ABRIDLE 8-SEP-1988 10:15
To: RPERLEY,JBURNS,ABRIDLE
Subj: Typos in last mail

From: CVAX::ABRIDLE   8-SEP-1988 11:47
To: RPERLEY,JBURNS,ABRIDLE
Subj: Still more on 219 paper

(As you can tell, I'm reading this thing through systematically and sending comments on each item as it comes up -- do either of you have a working E-mail address for David; if so, I'll collect all this and send copy to him at the end.)

There are two interesting spectral effects that are very likely real which are not mentioned in the text.

1. Table 3 gives a significantly higher spectral index between 18 and 6 cm for N1, the counterjet knot, than for any of the other jet features. If the errors are o.k., this deserves a mention, because it is an effect that is predicted by the relativistic-jet picture. I found an even steeper spectrum for the counterjet knot in 3C288 between 6 and 2cm and noted both the result and its place in the relativistic-jet model in my Atlanta writeup (you expect to see synchrotron ageing effects first in the red-shifted jet, i.e. the counterjet). I've almost finished a draft on 3C288 for the A.J. and will circulate this for info. √ done

2. Figure 3 nicely shows an effect that was already apparent on the crude spectral index analysis I sent to David with the data -- there are "fans" of lower-than average spectral index extending from both hot spots toward the outer boundaries of both lobes in the regions where the outer brightness gradients are steepest. I think this is contrary to the effects of missing spacings and is therefore very √ done

likely real. To be fully understood, it needs to be deconvolved from the magnetic field variation, but to first order one could argue that these fans trace the secondary outflow from the hot spots toward the edges of the lobes. I think the effect should at least be mentioned, but we might debate how much to make of it.

3. A general comment. How can we distinguish the proposed passive field model from one in which a "borne-again relativistic jet" is making its way into a lobe with Laing's field model "C" (his Figure 6a)? Rick's and my reaction when we first saw the "invisible jet" of B-perpendicular going on from where the actual jet left off was to speculate about precisely the sort of model David has computed. But then we realised that this is also what you would see if the jet really does stop at the end knot, leaving you staring at a lobe containing Robert's field model. The key is the Faraday RM gradient, but this is introduced in the present paper early on (and Robert's model is never actually mentioned). I think it would be better to set up all the alternatives, and then systematically go through what in the data does, and does not, support them. As it is, the present discussion only talks seriously about the toroidal field model and the flip-flop. The more interesting alternative is given no space at all (although both of its ingredients, the "born-again" jet and the Laingian lobe) were explicitly talked about in BPH. √ partly done

From: CVAX::GATEWAY::"AIPS@UNMB" 8-SEP-1988 16:55
To: ABRIDLE AT NRAO
Subj: 3C 219

Date sent: Thu, 8 Sep 88 09:13 MDT
To: abridle@nrao

Alan:

have had long discussions over this point, and we spent a good deal of time during his dissertation defense discussing this point. It is interesting to note that one might explain the jet/counterjet asymmetry if one could invoke a difference in magnetic field structure opposite sides of the engine. But herein lies the rub. How does one generate such an asymmetry near the engine. We've discussed several possibilities but I find them all to be complex and unconvincing. So, this model must be viewed as being as ad hoc as the flip-flop model until some better explanation arises. That is why I would phrase a lead in as " it is interesting to note..." versus the more definitive statements that David has made. We expected that both you & Rick would want to discuss this in more detail.

By the way, David is now at NCSA. You can reach him via BITNET. I'll send you a BITNET address as soon as I have it.

Cheers,
Jack

From: CVAX::ABRIDLE 9-SEP-1988 17:44
To: JBURNS,DCLARKE,RPERLEY,ABRIDLE
Subj: Last (I promise!) major comments

Here's a final batch of comments on the draft paper. I'm sending the copy with the typos, English, and nit-picking stuff to David. This lot are those that affect the science.

1. Disappearing jets are the key to this paper. We are trying to choose between explanations of why they disappear. David is arguing that if the field in the cocoon comes passively by expansion from the field in the jet, the jet is hard to see against the cocoon if the jet field is initially toroidal, or after it becomes toroidal. I have two questions. (a) Exactly why does the field become toroidal at the appropriate place---is the model jet expanding? (3C219's isn't). (b) In the simulations, the jet isn't actually invisible, it's just hard to see against the cocoon---could it be made visible with a high pass filter (such as the VLA's A configuration). It's not enough to make the jet hard to see, we know from the A array data that you gotta turn it off. Altogether (down to the noise).

still true

2. How much gain for how little pain? David's argument in favor of the model is that it explains more for less. A fine criterion, but we must be fair about the accounting, to make the case. The strong point of this model is that it shows how the cocoon field structure can come for free if you start with the right jet field structure (and as one of the early backers of helical field models for jets I like the starting premise a lot, because I've been saying all along that they can explain the detailed correlations between degree of polarization and apparent field geometry in the well-resolved jets, too!). But it doesn't directly explain the uniformly one-sided appearance of all the jets in powerful sources (why do they *all* have the proposed difference in starting field configuration between the two sides, while lower power sources rarely do?). Nor does it explain why we see any part of the counterjet.

David has been particularly unfair to the flip-flop. I don't like the flip-flop model much, because it isn't a model, it's an ad hoc description. But the flip flop can "explain" why you see something on *both* sides and why the knots on the two sides anticorrelate (i.e. why the only peak in the counterjet lands in the gap in the main jet). The flip flop needs some sort of unspecified switch in the central engine, a "floppy disk"---but central engine theorists so far have been quite happy to come up with these on demand. I don't know how many of them, or which, to believe, and I think that's irrelevant here. The point is that the flip-flop traces some aspects of the appearance back to an asymmetry between the two sides of the engine, *and so does David's model*, via the starting field structure. So they're roughly even on that score.

that we may need anyway in order to transfer enough energy to the lobes using a reasonable mass flux. It accounts in one fell swoop for the brightness ratios, the geometrical asymmetry, and the high frequency spectral difference. It also allows the main jet and the counterjet to vanish without a trace even under a high pass filter. A mildly relativistic backflow could also help to explain the circular hot spot in the north lobe, after Wilson and Scheuer (1983). That's not bad shooting, and that's of course why BPH emphasized the model. It says nothing about the field configurations in the lobes, but only because it doesn't try to. The main notion of David's model, that that the field in the lobes has come from the past activity of the jet, could of course be grafted onto the "born-again" model, too. It *does* "predict" that there are shocks at the ends of both the jet and the counterjet, so we should see B-parallel convert rapidly to B-perpendicular at the tips of both of them. What little data there is on this point says this prediction is right---there is evidence for depolarization, and swinging of the vectors, at both jet tips on the high-resolution images. But it's marginal. But could we tell the difference between this and the behavior in David's model---another reason why the physical reason for the field flip in the simulation needs to be talked about some more.

To summarize, there are 3 models that can do good things. We need to weigh how many good things they do against how many assumptions they make. Let's do it carefully.

3. Laing's lobes.

David has not commented at all on the now-ancient Laing lobe model (Ap.J. 248, pages 95 and 99, also Figure 6). We gotta say something, because it too fits the lobe data, and either the flip-flop or the born-again relativistic jetters can choose to ejaculate into it if they wish. Then they get the lobe picture for free, leaving us arguing about the jet and counterjet (which strictly speaking shouldn't be there at all in David's picture). What's wrong with the Laing field ? One problem may be that it's not a solution of Maxwell's equations, it's a postulate. In other words, he doesn't say how extragalactic sources make it. David's model does say how to make the field in the cocoon from the one in the jet. But can we go further to say that the Laing field can't be made? That would be a strike against it. The Laing field also should not show transverse RM gradients, while David's field can (that's why we proposed this test in BPH). Score one for David. But to emphasize the point, I'd like to see (a) a statement of statistical significance for the RM gradient---at what confidence level can we rule out just a random RM distribution over the south lobe, (b) something said about the RM gradients, or lack of them, in the *other* lobe. We got a 2-sided source, and can't just choose the side that suits us, unless the statistics on that side are overwhelming. I'm not clear, from reading the paper, that they are.

Incidentally, the statement in the second line of the second paragraph of p.24 has got to go, because it's wrong. It says that the E-vector configuration in Figure 3 implies a toroidal field component. It doesn't, and an important aspect of field diagnosis from radio data (emphasized by Laing and again by BPH) is that you can't tell the 3-D field configuration just by looking at the E-vector orientations. You gotta match to the fractional polarization and the RM too. The Laing model also fits the E-vector orientations (by eyeball inspection) and the fractional polarization. The RM gradient is the key, and you *just can't say* what the draft says on p.24.

4. The hot spots

A fair bit is made in the text about the brightness distributions and the fractional polarizations and apparent fields in the hot spots.

properties---gray scales and/or contours and p,chi vector displays for just the hot spot regions from the high-resolution data. These will make many of the points about the hot spot morphologies much more appreciable to the reader. Once this paper is out, there won't be another good place to show this stuff for 219, so we should take the opportunity here.

5. Lobe filaments

These are referred to several times, and are offered as evidence that the field is not active. Let's get a display that shows them, and identify them for the reader. If we can't bring them out in a simple gray scale, the Sobel filter in AIPS will probably do the trick (it's inside NINER). Or any other mask you'd like to use. But I don't think we should refer to them so much without demonstrating to the reader that they're there. They are not as obvious as those in Cygnus A, which most people will take as the benchmark, so let's work on convincing people that they're not just a figment of our imagination.

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9 Sept 1988

Dear 219-ers,

David's draft of the paper has got us off to a flying start. I thought I'd send around the almost-final version of a paper I am doing on 3C288, which is also a moderately powerful radio galaxy with a converter knot detection. It may be of interest partly for its further mention of the "born-again jet" option, partly for its style of handling the "missing flux" problem, and partly because 3C288 itself is kind of interesting! I'm also sending copies of the E-mail dialog so far, to ensure we've all seen same info flying around!

I am annotating my copy of David's draft in detail as I go and will circulate in time (I hope) for his projected return to the Land of Enchantment in Sept.

Best wishes.

Alan

From: CVAX::GATEWAY::"AIPS@UNMB" 8-SEP-1988 16:55
To: ABRIDLE AT NRAO
Subj: 3C 219

Date sent: Thu, 8 Sep 88 09:13 MDT
To: abridle@nrao

Alan:

You are not missing anything in the 219 draft. David and I have had long discussions over this point, and we spent a good deal of time during his dissertation defense discussing this point. It is interesting to note that one might explain the jet/counterjet asymmetry if one could invoke a difference in magnetic field structure opposite sides of the engine. But herein lies the rub. How does one generate such an asymmetry near the engine. We've discussed several possibilities but I find them all to be complex and unconvincing. So, this model must be viewed as being as ad hoc as the flip-flop model until some better explanation arises. That is why I would phrase a lead in as " it is interesting to note..." versus the more definitive statements that David has made. We expected that both you & Rick would want to discuss this in more detail.

By the way, David is now at NCSA. You can reach him via BITNET. I'll send you a BITNET address as soon as I have it.

Cheers,

Jack

10698@NCSAVMSA

From: CVAX::ABRIDLE 8-SEP-1988 11:47
To: RPERLEY,JBURNS,ABRIDLE
Subj: Still more on 219 paper

(As you can tell, I'm reading this thing through systematically and sending comments on each item as it comes up -- do either of you have a working E-mail address for David; if so, I'll collect all this and send copy to him at the end.)

There are two interesting spectral effects that are very likely real which are not mentioned in the text.

1. Table 3 gives a significantly higher spectral index between 18 and 6 cm for N1, the counterjet knot, than for any of the other jet features. If the errors are o.k., this deserves a mention, because it is an effect that is predicted by the relativistic-jet picture. I found an even steeper spectrum for the counterjet knot in 3C288 between 6 and 2cm and noted both the result and its place in the relativistic-jet model in my Atlanta writeup (you expect to see synchrotron ageing effects first in the red-shifted jet, i.e. the counterjet). I've almost finished a draft on 3C288 for the A.J. and will circulate this for info.

2. Figure 3 nicely shows an effect that was already apparent on the crude spectral index analysis I sent to David with the data -- there are "fans" of lower-than average spectral index extending from both hot spots toward the outer boundaries of both lobes in the regions where the outer brightness gradients are steepest. I think this is contrary to the effects of missing spacings and is therefore very

likely real. To be fully understood, it needs to be deconvolved from the magnetic field variation, but to first order one could argue that these fans trace the secondary outflow from the hot spots toward the edges of the lobes. I think the effect should at least be mentioned, but we might debate how much to make of it.

3. A general comment. How can we distinguish the proposed passive field model from one in which a "borne-again relativistic jet" is making its way into a lobe with Laing's field model "C" (his Figure 6a)? Rick's and my reaction when we first saw the "invisible jet" of B-perpendicular going on from where the actual jet left off was to speculate about precisely the sort of model David has computed. But then we realised that this is also what you would see if the jet really does stop at the end knot, leaving you staring at a lobe containing Robert's field model. The key is the Faraday RM gradient, but this is introduced in the present paper early on (and Robert's model is never actually mentioned). I think it would be better to set up all the alternatives, and then systematically go through what in the data does, and does not, support them. As it is, the present discussion only talks seriously about the toroidal field model and the flip-flop. The more interesting alternative is given no space at all (although both of its ingredients, the "born-again" jet and the Laingian lobe) were explicitly talked about in BPH.

From: CVAX::ABRIDLE 8-SEP-1988 10:09
To: RPERLEY,JBURNS,ABRIDLE
Subj: 3C219 paper p.8-p.9

Re the "spectral index simulation" discussion.

1. Everything said about imaging without the zero spacing or primary beam correction should be deleted. It's obvious that you add zero spacing flux densities and do the primary beam corrections when doing this sort of work. If it wasn't obvious to Dave when he started, I'm sure it is now!

2. The discussion is not meaningful unless the angular sizes of the model components are specified. Also, the method of flux density estimation should be given. CLEAN components? Pixel summation?

3. Was noise added? Was a realistic noise cutoff then applied when estimating spectral index? Was the CLEAN run to convergence in the presence of the noise? Was a residual zero level correction made to correct any failure of an incomplete CLEAN to compensate the cereal bowl effect? Without such details, I can't judge how seriously to take the "worst case error" of 0.7 in the spectral index. For example, would those pixels have been blanked out with a sensible noise level cutoff?

4. To see how much of this can be predicted from elementary considerations, I ran the observation design through my VLA Observing Strategy Planner worksheets (the VLAPLAN program). All you really need to know is that a Gaussian component of FWHM X arcseconds falls to half amplitude at 91/X kilowavelengths, and the scale of the inner uv coverage of the VLA. If we take 60" as the FWHM of the largest circular thing in 219 (the North lobe cocoon), its visibility falls to about 0.6 on the shortest baseline present in the 6cm C array on the meridian (roughly 80 m), and to about 0.78 on the shortest baseline present at +/- 5 hours HA (roughly 55 m). The C array therefore must "miss" about 15% of the flux density of the largest feature even in a full synthesis, and 40% of it in a meridian snapshot. That's without having the chance to lose some more of it via CLEAN. You flat out need the D array to be sure of sampling a 60" component properly at 6cm. I'll send the curves from the worksheet program if you're interested. — attached.

I think Dave's result about missing flux is obviously correct on elementary grounds. If he took about 60" as the FWHM of his "cocoon" component, you'd expect to have trouble recovering about 0.2 of the 0.5 Jy from any C array data at 6cm, certainly unless the CLEAN was driven deep into the noise compared with the 20cm one, and likely not even then. What I think we actually need is an estimate of the spectral index uncertainty resulting from the lack of D array data, on the steepest spectra that are actually passed to a gray scale or given in a Table, plus the additional details of how he did the simulation. I also suggest including the elementary sums based on Gaussian component size and the actual shortest baselines in the coverage, to demystify the situation.

Note that the observations done in 1982 and 1983 were intended specifically to get high-resolution images of the jet, not of the large scale structure ! It's staggeringly clear that you can't image the large structure just with the B array at 6cm!

From: CVAX::ABRIDLE 7-SEP-1988 16:09
To: JBURNS,ABRIDLE
Subj: Q. for JOB re 219 draft

Am I missing something in Dave's draft ? He seems to dispose of the counterjet emission simply by asserting that the counterjet starts out with a pure toroidal field while the jet starts with a poloidal component. Why should this be ? Does this require a Deus Ex Machina as arbitrary as that of the flip-flop ? And why should it be so in all FRII sources (see p.24). If there's a good physical reason, it should be emphasized.

Note also that the lack of counterjet relative to jet in other sources is quite clear in some cases even when there is not a confusing cocoon. Now maybe these can come from the active-B group; but I again find the generalization to include *all* FRII's a teeny bit premature. Unless, as I said, I'm missing something about jet/counterjet field asymmetries.

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Dr. Alan H. Bridle, Tel. 804-296-0375, FTS 940-7375

November 20, 1986

Prof. Jack O. Burns, Jr.
Dept. Physics & Astronomy, Univ. New Mexico
800 Yale Boulevard, N.E.
Albuquerque, New Mexico 87131

Dear Jack:

Here are some superpositions of VLA radio data for 3C219 on the V band CCD frame obtained by Stefi Baum with the KPNO 4-meter in very good seeing. These show (a) the general relationship of 3C219 to the parent galaxy, (b) the other cluster members around 3C219 and (c) the optical identification of "baby 3C219", the small almost-parallel double source that is blended with the 3C219 south lobe. For reference, the optical data are the image "3C219.V", the 3" resolution 6cm data are called "3C219C ABC 3" and the 1.4" resolution 6cm data "219C ABC 1.4". Sorry for the inconsistency, but these files have been getting themselves named over a period of several years now !

The CCD data confirm the very extensive and flat envelope of 3C219 that was also indicated on the old Saslaw/Tyson image. This envelope does not record very strikingly on the two optical grey scales, but is well outlined in the contour plot of the CCD image. The units of the original CCD image are ergs/sec/sq.cm/sq.arcsec. The image as superposed on the 3" resolution radio data has been HGEOMed and the units are not as trustworthy.

The contour plot of the CCD image also has two very small crosses, one at the Perley et al. position for the 3C219 radio core, and the other at the position I got for the core of "baby 3C219" from our untapered A array data at 6cm, i.e. at 09 17 49.677, +45 51 55.33 (1950.0). The positional agreement with the "smudge" I had seen on the Sky Survey, now a very clear peak in the CCD frame, is excellent, especially if you allow for the not quite perfect registration of the radio core of 3C219 with the CCD peak. So I think the optical ID of "baby 219" is truly settled !

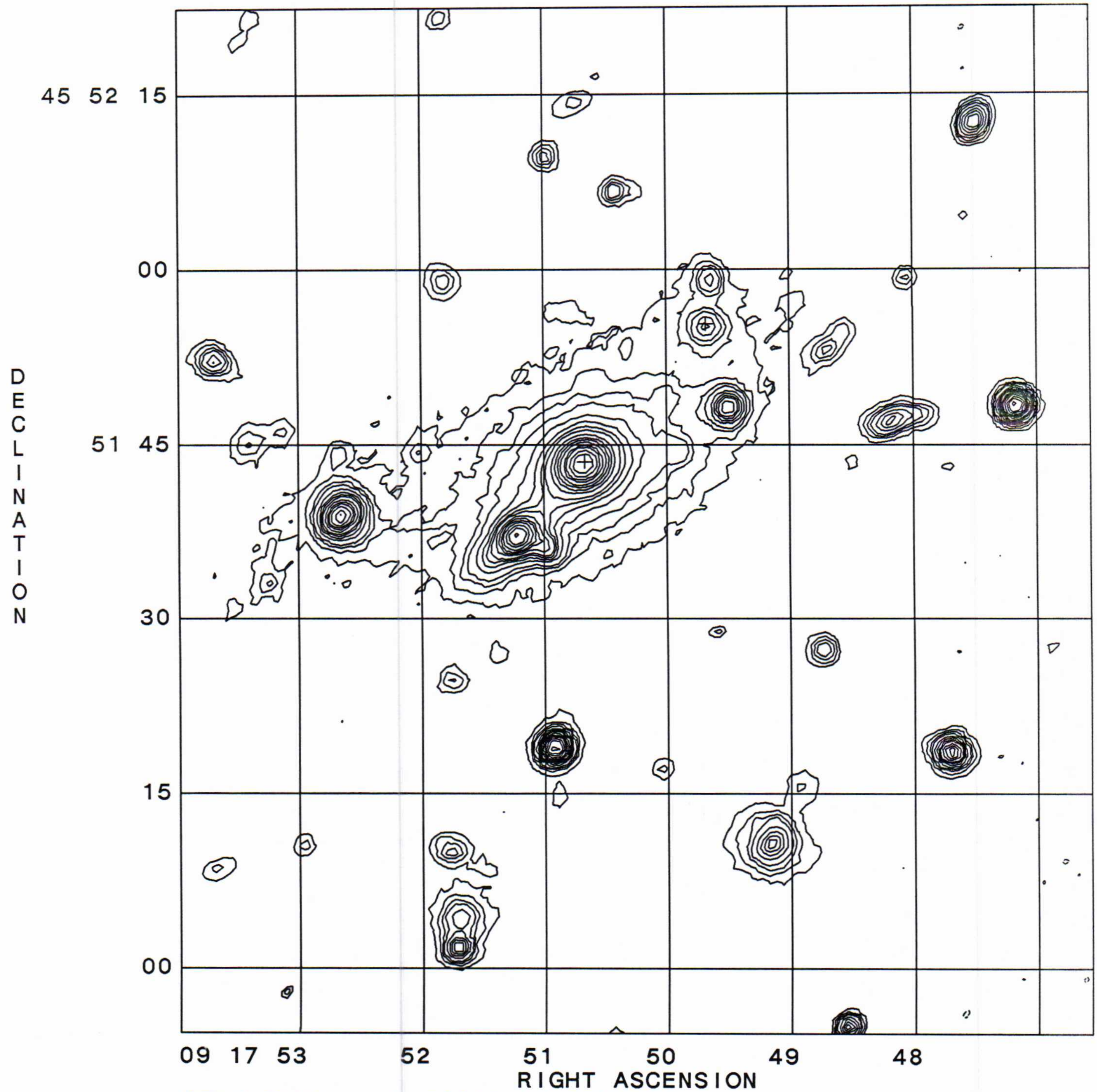
I will take some slices to see whether the image of the ID is broadened significantly. The much brighter feature to the South-East that has often been described as a "second galaxy within 3C219's envelope" now appears multiple.

Let me know if you're interested in having a copy of the CCD frame next time I am sending/bringing you a tape.

Alan

PLOT FILE VERSION 6 CREATED 19-NOV-1986 15:46:12

ave of 3C219.V.CAL.1



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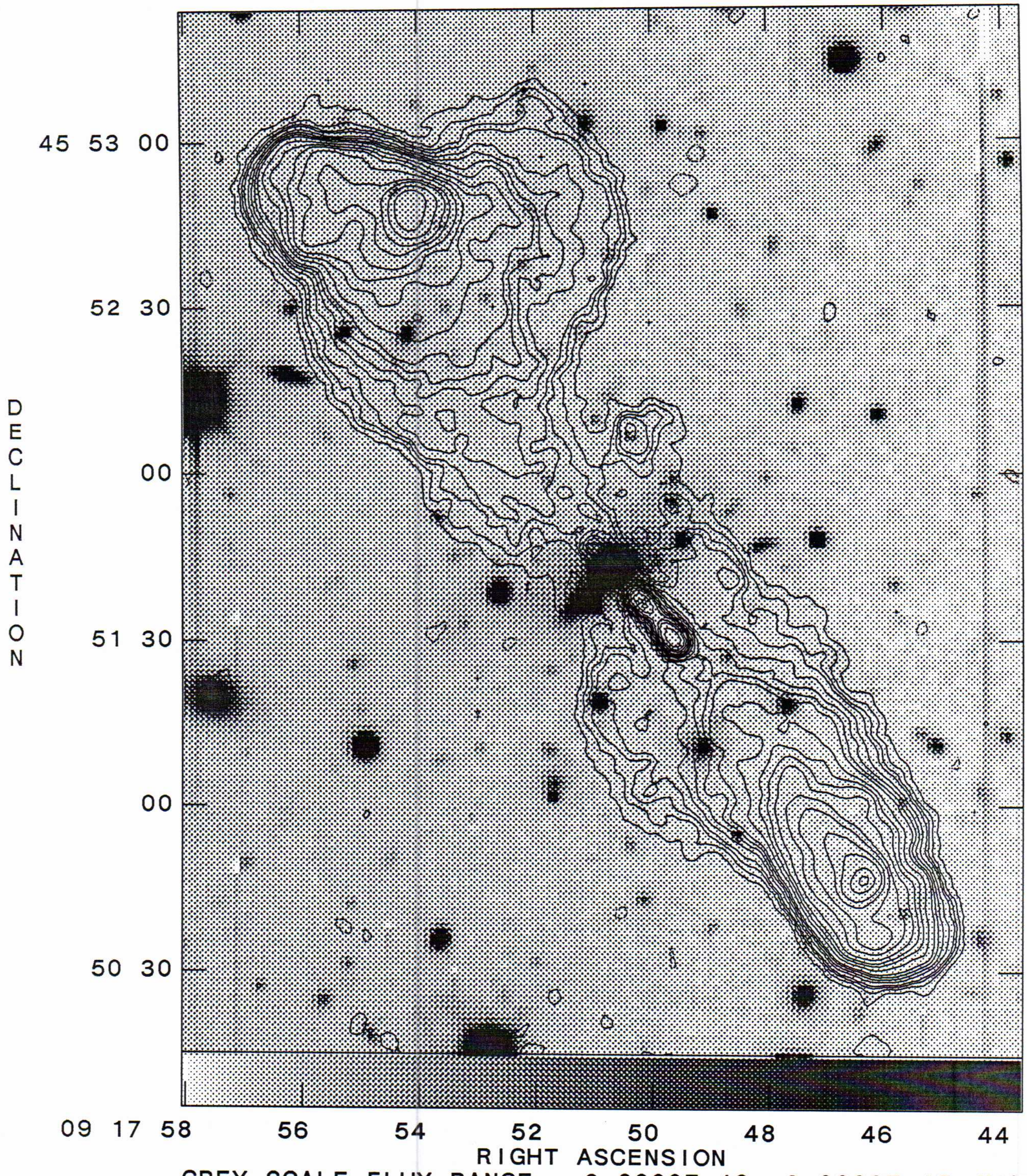

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800.0, 1200.)

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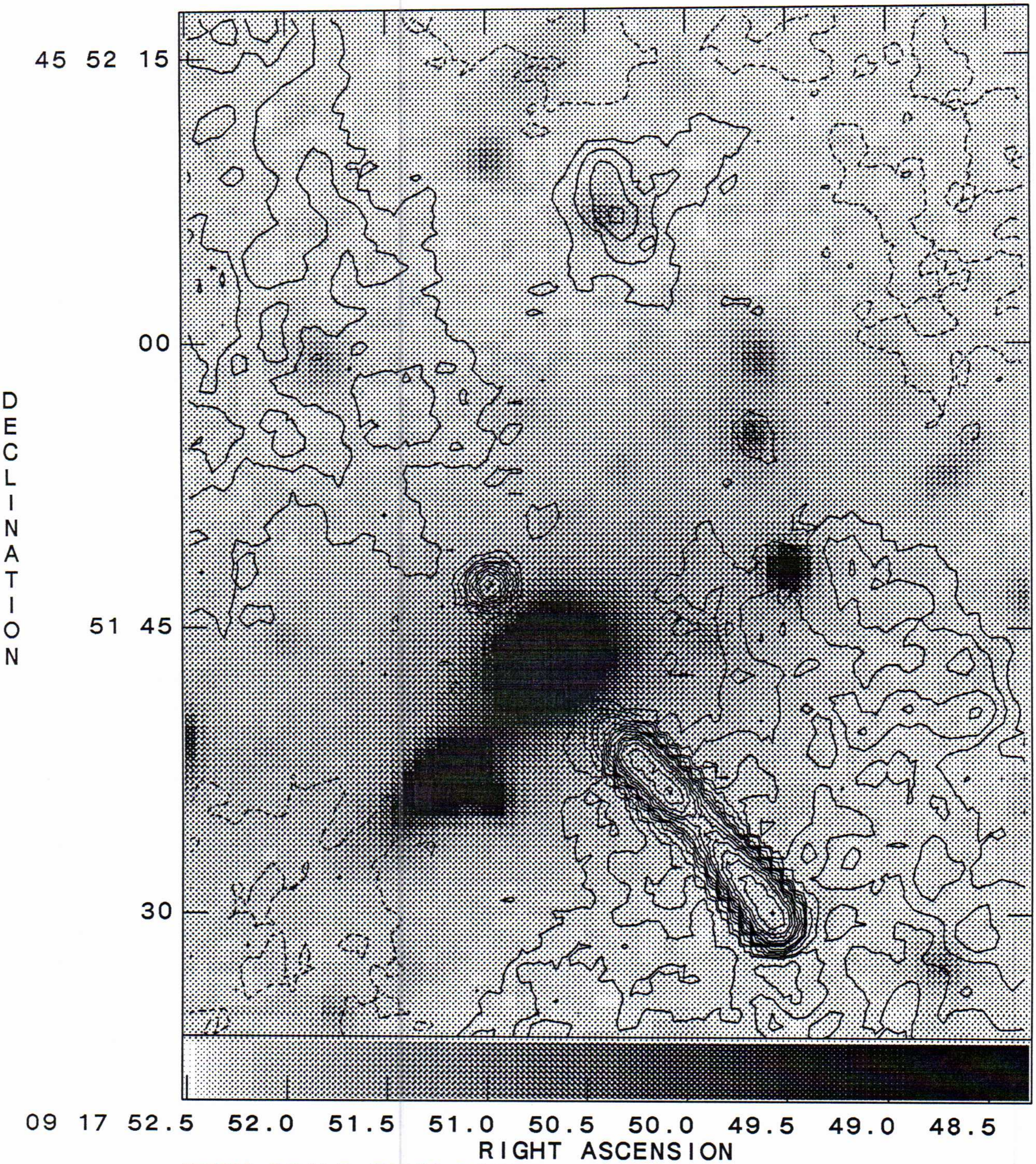

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