12/09/92 16:29 ☎ 5056461602

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FACSIMILE COVER SHEET

DATE:

TRANSMIT TO: DR Alan Bridle

COMPANY/INSTITUTION: NRAO

ADDRESS:

FAX NUMBER: 804-296-0278

****

TRANSMITTED FROM:

NAME: Jack Burns

DEPARTMENT: ASTRONOMY

ADDRESS: Box 30001/Dept. 4500

TELEPHONE NUMBER: (505) 646-4438

CHARGE TO ACCOUNT NUMBER: 43658

NUMBER OF PAGES: 2
(including this coversheet)

****

NOTES: Alan:

Here's the referee's report.

JB

Astron. Jl. 920372 referee report (Fernini and Co.)

The issue of unification schemes for radio-loud objects is a topical one. The manuscript describes an important approach to / test for this scheme, namely the relative jet prominence. Two other relevant issues, the radio depolarization asymmetry and radio-optical asymmetries are also investigated.

This is a well written paper, asking simple questions and providing one simple answer. I look forward to subsequent articles on these studies, and have no objections against rapid publication in the Astronomical Journal. Specific comments are listed below. I do not need to see a revised version.

Specific (minor) comments:

As an illustration of the difficulty of detecting jets in luminous radio galaxies, the authors might wish to refer to the Cygnus-A jet discovery paper (Perley et al. 1984) already in their Introduction.

In addition, it may be worth mentioning that even at 90 degrees from the sight line relativistic jets will appear fainter than nonrelativistic jets.

I am curious as to how the redshift and linear size distributions of their QSR and RG samples compare, and would urge the authors to add a figure, or to comment on this issue in the text.

Finally: could the authors comment on the fact that 3C22, having the prominent jet, also has the highest core/total 5GHz flux density ratio and is not a particularly large source!

From abridle Thu Nov 5 16:10:10 1992
From: abridle (Alan Bridle)
To: jburns@nmsu.edu, rperley
Subject: Last look at RG draft
Date: Thu, 5 Nov 92 16:09:10 -0500

Jack -- final comments from AHB:

Fig.1:
~~~~~~
a. This brings out strongly (maybe over-emphasizing through saturation?) the compactness of F2-->F6 in 3C55. I wonder if there is any chance that F2-->F6 is another double source projected against 3C55, with F2 and F6 as its hot spots? Are there any other optical objects near the centroid of the F2-->F6 line?

b. The Figure is not mentioned in the text for 3C324 or 3C356, so these panels are "orphaned". This is probably because the grey scales for these sources add very little to the content of the paper, but I don't think it's worth rearranging the Figure at this late stage. Can we instead add the clause "and Figure 1 shows a grey scale image" to the third sentence of para.1 of Sec 4.2.4 (3C 324) and to the second sentence of para.1 of Sec 4.2.5 (3C 356)? That way the reader who "dips into" the paper just at these sections will still be made aware of these grey scales.

Fig. captions:
~~~~~~~~~~~~~~
Everywhere: replace "peak flux is" with "peak flux density is".

p.18
~~~~

a) delete "optical" before "galaxy" on line 7. (It is surely there at other wavelengths!)
b) replace "spectrum" with "system" or "region" on line 13. (The spectrum cannot be described as extended, it's the gas that's extended?).

Minor English/style suggestions:
~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~
Abstract, line 5/6. Change to:
"The jets in these RGs are less prominent, relative to the lobes, than those in the quasar sample."

p.3, para.1, line 10 delete "of" after "all"

p.5, line 7 delete "of the" after "all"

p.7, line 9 Replace "With the exception of" by "Except for"

p.12, line 8 replace "which" by "that"

p.19, line 1 add "galaxy" after "which"

p.25, line 3 replace "which" by "that"

p.25, line 6 Replace "Subsequent" by "Later"

As you can tell from the minute nature of the last few comments, I'm happy to donate this paper to the referee's in-tray now.

Any comment yet from Rick?

Cheers, A.

From abridle Tue Oct 27 11:33:49 1992
From: abridle (Alan Bridle)
To: jburns@nmsu.edu, rperley
Subject: Last changes based on lit review
Date: Tue, 27 Oct 92 11:31:10 -0500

Here's the final instalment of changes that I would like to suggest to the RG paper based on the literature search:

Delete first sentence of Section 4.2.2 (3C55) and replace by:

This source is a 69" double previously imaged at radio wavelengths by Jenkins {\it et al.} (1977), Schilizzi {\it et al.} (1982), Strom \& Conway (1985) and Leahy {\it et al.} (1989). Laing {\it et al.} (1983) gave an optical identification with a galaxy at $z=0.27$ but Spinrad (private communication, reported in Hewitt \& Burbidge 1991) has since reidentified the source with a galaxy at $z=0.7348$.

Add after the present third sentence of Section 4.2.2:

Leahy {\it et al.} also report the detection of this radio core at 5, 8 and 15 GHz, at a preliminary position (Leahy, private communication) 0.004 s earlier than ours in Right Ascension and 0.07" north of ours in declination. Both of these offsets are well within the errors of the radio observations. Both radio positions disagree by about 2" in δ with that quoted by Spinrad (private communication) of ..... for the revised optical identification, which is marked by a cross in Figure 3. This discrepancy in declination leaves the new optical identification in some doubt.

(Delete balance of Section 4.2.2, para. 1)

Insert after first sentence of Section 4.2.4 (3C324), para.2:

This object is apparently one of the most luminous known galaxies. It is now thought (Le F\'evre {\it et al.} 1987; Hammer \& Le F\'evre 1990) to be a gravitational mirage produced by the superposition of a foreground, possibly spiral, galaxy at $z=0.845$ on a background narrow-line emitting galaxy at $z=1.206$.

Add to references:

Hammer, F. and Le F\'evre, O. (1990), Ap.J. {\bf 357}, 38.

Hewitt, A. \& Burbidge, G. (1991), Ap.J.Suppl. {\bf 75}, 297.

Le F\'evre, O., Hammer, F., Nottale,L. and Mathez, G. (1987), Nature {\bf 326}, 268.

Leahy, J.P., Muxlow, T.W.B. and Stephens, P.W. (1989), MNRAS {\bf 239}, 401.

==========

Re Figure 1 -- the new version shows more details than the previous one, so is probably an improvement. What I would really like to do is to search on "Figure 1" in the .TEX file to check quickly whether everything we say based on Figure 1 can in fact be discerned on the new version. If Jack could ship me the .TEX file for that purpose, I'll do such a check. (I won't confuse anything by editing in it, Jack!) Just scanning the paper visually I can't find the references to Figure 1 in the text for 3C356 or 3C324 (Secs. 4.2.4 and 4.2.5) so I may be missing things at the moment.

Yes, it's much changed since Ilias' version and I guess if he's out of reach we just have to hope he agrees with what we're doing? I don't relish doing this much modification to an incommunicado "first author" but do we actually have an alternative in this case? Is he completely unreachable at this point, Jack? I guess Jack is weighing the balance between Ilias getting a publication added to his record soon and him not having input on the final version. If it's conceivable that we can get his input in a reasonable time, I would feel much more comfortable. But Jack should decide that issue; if he can live with it, I can.

Cheers, A.

From abridle Mon Oct 26 16:39:43 1992
From: abridle (Alan Bridle)
To: jburns@nmsu.edu, rperley
Subject: 3C356 identification section
Date: Mon, 26 Oct 92 16:38:01 -0500

Hello guys, I'm back from a bad bout of flu that hit me last Tuesday evening, and I've just groped my way back out of the accumulated pile of chores into the FRII RGs paperwork. I still have some changes to suggest for the identification sections based on the literature search. Here's the first of these:

For 3C356, I suggest we replace paras 2 and 5 of Section 4.2.5 with the following three paras:

~~~~~~~~~~~~~~~~~~~~~~~~~~

Our observations show two compact radio features, (D) and (E), in the inner part of the source. The peak of the brighter feature, (D), is at $\alpha =$ .... in good agreement with that of the optical identification suggested by Spinrad {\it et al.} (1985), which is at ..... This peak also appears to coincide with the compact peak of the resolved K-band (2.2μ) feature (b) detected by Eales \& Rawlings (1990) and by Eisenhardt \& Choksi (1990). This coincidence does, not, however, uniquely establish the optical identification of the extended radio structure, because there is a similarly good positional agreement between the fainter radio peak (E) at ....., the alternate optical identification previously proposed by Riley {\it et al.} (1980) at ....., and the compact 2.2μ feature (a) of both Eales \& Rawlings (1990) and Eisenhardt \& Choksi (1990). Rigler {\it et al.} (1992) suggest that (a/E) is the parent of the radio structure because this galaxy has a higher ionization spectrum. They also suggest that (b/D), which exhibits an extended optical emission line system, is a companion galaxy that has wandered into the path of an unseen radio jet emanating from (a/E).

Gaussian fits to our 6cm data show that both (D) and (E) are unresolved (Table 5). Our 3.6cm data detect both (D) and (E) at a resolution of 0.84", with peak flux densities of 1.05 and 0.25 mJy respectively. Thus the 6cm to 3.6cm spectral indices (defined by S$_{\nu} \propto \nu^{-\alpha}$) of (D) and (E) are 0.1 and 1.1 respectively. (D) therefore has the more typical spectral index for the compact core of an extended radio galaxy, whereas (E) has a spectral index more typical of a steep-spectrum, compact source.

We conclude that the available data on the two compact radio features near the center of 3C356 do not resolve the identification ambiguity uniquely. Instead, both (a/D) and (b/E) display several characteristics of active galactic nuclei and it cannot yet be determined which is responsible for the large-scale radio structure. This ambiguity may remain until more sensitive radio images detect a large-scale radio jet linking one of these two nuclei to the radio lobes.

~~~~~~

If so, also add to references:

Eales, S.A. and Rawlings,S. (1990), MN {\bf 243}, 1P.

Eisenhardt, P. and Choksi, A. (1990), Ap.J. {\bf 351}, L9.

Rigler, M.A., Lilly, S.J., Stockton, A., Hammer, F. and Le F\'evre, O. (1992), Ap.J. {\bf 385}, 61.

~~~~~~

Others to come asap, Alan
~~~~~~

From abridle Fri Oct 16 11:21:46 1992
From: abridle (Alan Bridle)
To: jburns@nmsu.edu, rperley
Subject: Changes to text based on depol literature
Date: Fri, 16 Oct 92 11:20:41 -0400

>From the literature search, I feel that our present text is delinquent in not making comparisons with the previous depolarization estimates that were out there. After going through the references, I feel we should make some changes in Sections 5, 6 and 7 and in the references as follows:

====== start Sec.5 para. 3 replacement =====

The compilation of integrated polarizations by Tabara \& Inoue (1980) estimates half-depolarization wavelengths of 17cm for 3C\,356m 23 cm for 3C\,324, >21 cm for 3C\,22 and 3C\,55, abd >31 cm for 3C\,265. Our depolarization data (Table 7) generally agree with Tabare \& Inoue's results, in that neither 3C\,55 nor 3C\,265 shows significant depolarization between 8.4 GHz and 1.4 GHz, and the other three sources show depolarization only between 4.9 and 1.4 GHz.

Strom and Conway (1985) report asymmetric depolarization in 3C\,55 and 3C\,265 between 1,4 and 0.5 GHz at 20.3 \times$ 20.3 \cosec\delta$ resolution. Their lower-frequency data are the only evidence for depolarization in these sources. For 3C\,324, there is an apparent discrepancy between our results amd the earlier work of Conway {\it et al.} (1983) and of Pedelty {\it et al.} (1989). We find that the north-east lobe is significantly more depolarized at 20 cm, whereas Conway {\it et al.} quote half-depolarization wavelengths of 15cm and 22cm for the south-west and north-east lobes respectively. Pedelty {\it et al.} find the lobes to be equally depolarized on average, with the strongest depolarization being in the trailing portion of the west lobe. We conclude that the depolarization asymmetry of 3C\,324 is poorly established at present.

For 3C\,22, our data provide new evidence for a significant depolarization asymmetry between 6cm and 20cm, the side that depolarizes at the shorter wavelength being that with the fainter, or counter, jet. For 3C\,356, we find significant depolarization in both lobes between 6cm and 20cm, symmetric to within our errors. This result conflicts with that of Pedelty {\it et al.} (1989), who found depolarization only in the south lobe between these wavelengths. We conclude that the depolarization asymmetry of 3C\,356 is also poorly established.

====== end Sec.5 para. 3 replacement ======

(I sent Jack comments on the last para in section 5 in my previous message. I do not think these are altered by the additional changes suggested above. We should, however, modify the discussion of Section 5's results in Sections 6 and 7. Perhaps as follows?)

===== begin Section 6, para 2 partial replacement ======

For 3C\,265, the brighter [OII] emission is on the SE side of the nucleus, but there is evidently also an extended [OII] emission region towards the NW. Thus, the brighter emission line region is on the side of the shorter radio lobe, but it is notable that there is

significant line emission on both sides, no significant depolarization at 20cm on either, and greater depolarization at 49cm on the side of the longer radio lobe. For 3C\,324, the emission line gas is extended along the axis of the radio source, but there is no clear indication either of an [OII] emission asymmetry or of a depolarization asymmetry. The symmetries of the optical data for this source may also be complicated by gravitational lensing (see Section 4.2.4). For 3C\,356, the relation between the [OII] line-emitting gas and the source is complicated by the uncertainty in the optical identification (Section 4.2.5). We note, however, that the south-east lobe must be the closer whether either feature D or feature E marks the parent object of the extended radio structure. Our data show that the south-east (closer) lobe is not strongly depolarized at 20cm, and the north-west (further) lobe is the more strongly depolarized at longer wavelengths (Strom \& Conway 1985).

We conclude that there is no clear correlation between the depolarization asymmetries of these sources and either their emission-line asymmetries or their lobe-length asymmetries.

====== end Sec.6 partial replacement ======

====== start Sec.7 para.4 replacement ======

Section 5 showed that there is little depolarization in these five radio galaxies as the wavelength increases from 3.6 to 6 cm, but three (3C\22, 3C\,324 and 3C\,356) show significant depolarization between 6 and 20cm. 3C\,22, the only source in this group in which we have detected an unambiguous radio jet, has a strong depolarization asymmetry with the jetted lobe being the less depolarized at 20cm. In both 3C\,324 and 3C\,356, the sign of the depolarization asymmetry is unclear, while both 3C\,55 and 3C\,265 exhibit asymmetric depolarization at longer wavelengths. Combining our data with those of Garrington {\it el.} (1991), we found no evidence for differences in the lobe depolarization asymmetry between radio galaxies and quasars {\it at similar redshifts}, but the sample is small.

===== end Sec.7 para.4 replacement ========

===== add to references =====

Conway, R.G., Birch, P., Davis, R.J., Jones, L.R., Kerr, A.J. and Stannard, D. 1983, MNRAS 202, 813.

Strom, R.G. and Conway, R. G. 1985, A. \& A. Suppl. 61, 547.

===== end add to references

Note also that the Pedelty et al. ref is incorrectly given as Ap.J. in present text. Correct journal is A.J.

I would also like to put together some changes to the individual-source sections to deal with the source-identification issues that were raised in the other literature, but will not have time to provide text on this until after the Visualization Workshop -- i.e. until next Wednesday. Is that acceptable to you guys?

From abridle Tue Oct 13 11:18:27 1992
From: abridle (Alan Bridle)
To: jburns@NMSU.Edu, rperley
Subject: Re: Lit search for RG paper
Date: Tue, 13 Oct 92 11:17:51 -0400

I'm still working on this, it's turning into a long paper chase ...

I am uncovering the following:

3C55 -- past discussion of ID ambiguity, and published versions of the Spinrad redshift.
-- depolarization asymmetry at longer wavelengths (Strom & Conway 1985)

3C265 -- depolarization asymmetry at longer wavelengths (Strom & Conway)

3C324 -- claimed gravitational lens in optical (many refs, see NED stuff I sent you on Friday)

-- depolarization asymmetry quoted by Strom & Conway

3C356 -- extensive discussions of the two-core two-galaxy ambiguity by Rigler et al., Eisenhardt et al., Eales and Rawlings, with detailed IR and [O] imaging. All should be referenced in our paper, though I believe our conclusion (two active nuclei, don't know which is ID), will probably stand. There are several references to private communications about radio cores from Robert Laing, I may roust him for any info he could add to spectra or compactness.

As I'm still turning things up, I will keep going on this as I can make time among other things today, and will send you both some suggested text revisions a.s.a.p.

Some of this stuff is post-thesis, so we can understand Ilias did not turn it up in his drafts. Some, however, is old stuff that seems to have slipped through his net ....

From root Wed Oct 7 20:59:41 1992
From: jburns@NMSU.Edu
To: abridle@NRAO.EDU
Date: Wed, 7 Oct 92 18:59:38 MDT

Alan & Rick:

In the next E-mail, I am sending along the revised version of the Fernini et al. RGs paper. The file is a postscript file & you should be able to just print it out. However, I'm concerned that it might be too long & some of it will drop off the edge of the known Universe. If you have trouble printing it, please let me know & I'll FTP it to you instead.

Now, let me tell you about all the changes. Once again, the paper has evolved significantly since the last iteration thanks to your many useful comments. I have attempted to address each comment in detail in the revision. I appreciate the very specific nature of your comments & the replacement wording which made it easier to make the revisions. Here's some details:

(1) There is a new table (Table 5) which reports core sizes & powers from my IMFITS to the images. This seemed an important missing table in the previous draft.
(2) Table 6 has been revised & expanded.
(3) I spent a good deal of time over the past month looking at the individual images, measuring sizes & fluxes, and trying to reproduce & extend what Ilias had done. This was motivated by your questions about specific sources & our definitions about hot spots & cores. I now feel more confident in the numbers & statements made in the text. In particular,

(i) 3C 55 - F8 meets the criteria for a HS as discussed in detail in the text. I've complied with Alan's request to add some specifics when there is a close call on an HS.

(ii) 3C 265 - Feature C is the HS.

(iii) 3C 324 - A & E just make it as hot spots.

(iv) 3C 356 - D & E remain core candidates. Both are unresolved with limits listed in Table 5. However, E has a steep radio spectrum more typical of a CSS source as discussed in the text. I'm not sure that we can say anything further on this subject.

(4) There is a new Fig. 7 on QSO & RG depolarization ratio vs. z provided by Alan. I believe that Alan sent a copy to Rick as well as to me. Rick, please look it over & see if you agree to keep it in the paper.
(5) Discussion on [OII] has now been substantially firmed up thanks to comments from you both.
(6) I'm looking again at the grey scales in Fig. 1 as per Alan's suggestions.
(7) Linda XXX now has a last time. I know it removes the intrigue but it had to be done!

Overall, I hope the paper is now about ready to submit. I'd welcome any last comments from you both at your earliest convenience.

The one thing that troubles me is that we have effectively removed much of Ilias' words from this paper. Yes, the paper is now more readable & more correct, but has Ilias learned anything from this? Given the fact that I cannot easily communicate with Ilias, I did not see what else we could do. By the way, I'm sorry to report that Ilias' job in Saudi Arabia has again fallen through due to politics in Saudi. I'm not sure what he is going to do at this stage. It will be very difficult for him to work on the 2nd set of runs on this project.

That's all for now,
Jack

From root Fri Oct 9 09:21:23 1992
From: "JBVAD::JPL"@UVAX1.AOC.NRAO.EDU
To: ABRIDLE@polaris.cv.nrao.edu
Subject: 3C55
Date: Fri, 9 Oct 1992 7:21:27 MDT

Alan,
No, I havn't published the core position: I'm STILL working on that project. The Strom et al was a red herring, sorry. I was thinking of Strom et al (1990) A&A, 227, 19; but in fact 3C55 is not in there!
Act
OOps
Actually where I saw it was a draft ms of an update of LRL by Laing & Riley in preparation, which cites Laing & Owen ("1990" but also still in preparation [of course]) giving the core at
01 54 19.06 28 37 02.8
we have detected the core clearly at 5, 8, & 15 GHz and also somewhat confused by the large-scale emission, at 1.4 and 1.6 GHz. All these datasets relied mainly on self cal so we did not use a nearby phase reference and so the positions are not very accurate. However my best guess is
01 54 19.051 +/- 0.009 28 37 02.91 +/- 0.05
(errors from the scatter in individual measurements).

Actually, thinking about this, we had the 15 GHz map in November 1985. I pointed out the discrepancy to Julia Riley, who wrote to Hy Spinrad, who measured the new position and redshift. He sent Julia a preliminary position and redshift in September 1986, giving position:
01 54 19.03 28 37 00.6
and commented "off a bit in declination from the MERLIN" - which he mistakenly thought provided my radio position. Julia and I felt that this offset was more likely to be due to optical measurements, but did not persue the matter.

I really am working on this data: I hope to get a paper submitted early next year. It's very difficult work as the results depend crucially on accurate calibration of the data and so I have had to check and recheck this... with 64 different datasets! But I'm in the home stretch at last.

cheers, Paddy

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JULY 22, 1992

NEW MEXICO STATE UNIVERSITY

TO: ILIAS, ALAN, & RICK

FROM: JACK Jack

SUBJECT: REVISION OF RADIO GALAXIES PAPER

Please find enclosed another substantially revised draft of our radio galaxies paper. I have attempted to address each of your comments from the last draft. The changes include:

(1) A new Figure 1. This figure was produced using some newly acquired software (Spyglass, Adobe Printshop) running on a Mac II which drove a new $20k Kodak photographic printer here at our NMSU visualization center. We were able to control the contrast and transfer function on each image while mosaicing the 5 maps. The final dynamic range looks pretty good. I tried to emphasize those important features in each map which could not be clearly seen on the contour maps. I'm particularly interested in Alan's reaction to this figure with regard to possibly using this camera for the QSO greyscale images.

(2) The introduction has been reorganized per Rick's suggestions.

(3) Section 3 was cut back by nearly a factor of 2.

(4) A new section, 4.1, was added which defines core, jet, lobe, and hot spot in a manner consistent with that used in the QSO paper. I tried to then follow these definitions carefully in the remainder of the paper. In particular, I reviewed the source structures and made appropriate flux density and size measurements of components to be sure that our descriptions matched the definitions. As a result, some revising of Section 4.2 was necessary.

(5) Rick suggested that we look at the spectral index of features D and E in 3C 356. In doing so, I found that D is flat but E has a rather steep spectrum. As a result, one must be suspicious about E as the optical ID. This is discussed in Section 4.2.5.

(6) Alan asked who says the [OII] line emission in 3C 22 is unresolved. McCarthy stated this in this thesis and described the northern extensions as "artifacts". This is now noted in Section 6. Also, in response to Alan's request, I have included a new figure (Fig. 7) from Ilias' thesis that has the overlays of the optical emission line images onto the radio.

There are many other little changes to the text, tables, references, and figure captions as you will see. Please read over this latest version very carefully and give me your comments. I would like this to be the final round of internal review before submission. I will take your comments, revise the paper again, and then submit it to the Astronomical Journal. If possible, I'd like to do this by the end of August, so I'll need your comments as soon as you can get them to me.

Thanks very much for all your help.

From abridle Wed Sep 2 18:04:01 1992
From: abridle (Alan Bridle)
To: jburns@nmsu.edu, rperley
Subject: Details re RG paper
Date: Wed, 2 Sep 92 18:03:41 -0400

Title page: AHB address -- 520 Edgemont Road (we now need street #)
22903-2475 (we seem to need 9-digit)

Abstract:
line 8 "asymmetries between 6cm and 20cm"

Introduction:
suggested rewrite to cover AHB and RAP suggestions sent separately

Section 2.1:

First line: "Our sample of 3CR radio galaxies was originally ...."
Third line: "Our objective was to have two similar samples ..."
p.6, item(2): "To match that of the quasar sample, with the exception of 3C9." ??
(I agree with Rick's comment that the lobe power distributions are also interesting here -- how similar were they for the quasars and the RGs?)

Section 2.2:

First line: "This paper presents the results of the first of two rounds of VLA observations allocated to this project, in which we observed the five radio galaxies shown in bold type in Table 1."

Section 3:

Item(iii): "used the bandwidth listed in Table 2. These bandwidths were chosen to maximize sensitivity while limiting the distortions produced by chromatic aberration (\it e.g.,} Cotton (1989)) at the outer edges of the lobes to $<5\%$ (as measured by the intensity reduction for a point source)."

p.7, last lines:
"For all the observations, the primary flux density and position angle calibrator was 3C\,286, which we assumed to have flux densities of ??Jy, 7.4 Jy, amd ??Jy at 1.4, 5 and 8.4 GHz, and a polarization position angle of 66^{circ} at all frequencies. The resulting flux density scale is that of Baars {\it et al.} (1977)."

Section 4.1:

p.9, line 2: "For consistency, we use the definitions proposed by Bridle {\it et al.} (1992), which we summarize as follows:"
line 8: "(c) aligned with the nucleus of the parent object where it is closest to it."
(4) Hot Spot: "If no jet is detected, a feature that (a) is the brightest feature in the lobe, (b) has a surface brightness more than four times that of the surrounding emission and (c) has a linear FWHM (after deconvolving

the synthesized beam) that is $<5\%$ of the largest diameter of the source. If a jet is detected, the hot spot must additionally be further from the nucleus than the end of the jet, which is defined by (1) its disappearance, or (2) an abrupt change in direction by at least 30° or (3) decollimation by more than a factor of two."

Add: "The hot spot definition is intended to isolate a class of compact, bright, feature that marks a major change in the apparent direction and/or collimation of a jet, whether or not the jet itself is detectable. Where a jet is detected, the definition seeks to distinguish hot spots from the jet knots that may be only minor disturbances in an ongoing flow."

(Note to Jack and Rick; I'd like to make this all as clear as possible as it is very likely that this version of the definitions will be in print before the "original" version, in which the rationale for them, and their application in tricky cases, are carefully spelled out).

Section 4.2.1:

p.10, line 4: "No core or jet was detected."
line 2 from end: "fairly well aligned"
p.11 line 4: Robert has asserted in the other paper that this should be a reference to Laing (1989), not (1988). Same applies to Perley (1988)?
line 6: " .. overlaid. (b) shows the SE lobe and (c) the central feature, jet, and NW lobe."
(As Robert complained in the other paper, we don't consider the jet part of the lobe and so we should keep emphasizing that throughout with consistent use of the terminology! He's right, though it is occasionally a bit long-winded.)

Section 4.2.3:
line 5: delete the sentence starting "The NW lobe has several ..."
This is stated more precisely later.
line 13: "two bright regions (A and C), connected by ..."

Now to go through the definition:
A cannot be a hot spot as it is not the brightest feature in the lobe, at least at our resolution. Thus A should never be described as a "spot". C, if taken literally from Figure 4(c), is just the peak of the inner bright complex, and this seems to fit the numbers in Table 5. This is compact enough to be a hot spot, but is it four times brighter than the surrounding emission? Jack can tell this best from the IMFITS to the region, it looks marginal just from reading the contours, If it does not make the cut as a hot spot, then we should say:

"Neither A nor C meets the definition of a hot spot, so there is no hot spot in the NW lobe."

Whichever is the case, we can't say, as in p.13 last line:

"The three hot spots are relatively highly polarized"

as by definition a source cannot have more than two hot spots. This source either has one, or two (if C makes it through).

Note that many of the quasar *counterjet* lobes have no hot spot by this new rigorous/tortuous definition, an attribute that is *not* shared by the jetted lobes! It won't hurt to emphasize again that our new definition permits there to be no *hot spot* in a lobe that's full of "bright features", especially as ability to pass through this filter may end up correlating with which side of the nucleus the feature is on!

p.13, line -4: "core-lobe distance" What's that, in a lobe that has no hot spot? May need redefinition.

Now back up: (!! sorry, but the order seems forced on me !!)

What happened in 3C55 east? We have F2, F6 and F8 all almost equally bright, but F8 looks more resolved. Only the detailed IMFITs (or an MEM reduction) could suggest which is the brightest (at our resolution), given the underlying emission corrections. Has this been checked out carefully enough to be sure that F8 meets all the criteria and that F6 and F2 don't? If so, I'd like to say so explicitly. Jack -- I guess I'm saying that I'd like to see the hot spot issue talked about source by source as explicitly as we do it in the QSR paper, but it needs access to the images themselves to look at this. If you don't have time, could I ftp the images across to C'ville and check these things out for you?

Section 4.2.4:

I presume that features A and E just make it as hot spot candidates by the skin of their deconvolved FWHMs (are the numbers in Table 5 raw or deconvolved, by the way?) but are they clearly more than four times brighter than the surrounding emission. Again, this is not obvious to me from the contour plots, they both look marginal. This may of course be saying that we don't have enough angular resolution to decide if there are hot spots in this case. That happens. However, I don't see how we can say (p.15, line 3) that feature D "emerges from the southern part of the hot spot E". The end of D as a distinguishable feature is a long way from the 0.48" by 0.33" component of E. Is the term "hot spot" here being applied to all of the extended emission *around* E? If so, this is inconsistent, and we should instead be saying:

"(D) that emerges from the southern part of the extended emission around (E)"

(Note that the grey scale image of 3C324 doesn't help to address this ambiguity, nor does it convince me feature D is really narrow.)

p.13, line 6: "A and C, and the flux densities of the NE and SW lobes at 5 GHz".

Section 4.2.5:

p.16, line 14: substitute "show", for "reveal"?
last 3 lines: I'm confused. Are you saying that E is unusally large for a core, or that it's spectral index is unusually large for compact core?
In either case, there is an issue here, as E galaxies that make large-scale radio sources can have extended, steep-spectrum "cores" as an alternative to flat-spectrm, compact ones.
Also, should sub "optical identification" for "galaxy ID" in line -2.

I'm not quite convinced that we've run the D versus E thing into the ground yet. We do seem to know that D is flat spectrum. Is it also unresolved? If so, it's a compact flat-spectrum source that could either be the core of the whole extended structure or an unrelated weak nuclear radio source. We also seem to know that E is steep spectrum. Is it unresolved? If not, it might either be a steep-spectrum extended core of the whole extended structure, or a stand alone CSS source unrelated to 3C356, or an unusually steep-spectrum jet knot that happens to be superposed on a background galaxy. The first and last of these leave it in contention as part of 3C356, the first leaves it in contention as the optical ID still. But either way we're missing out part of the argument if we don't give the size limits for D and E as part of the discussion.

Section 5:

The first two sentences are now incorporated (in effect) in the the redraft of the Introduction, so if we use that I don't think they need to be repeated here. The section can therefore start with "Because our three-frequency ...."

Jack used my rewrite from last time for the rest of the para, but deleted a sentence that is needed to make sense of the sentence that starts "The images made from these tapered data ..."
The sentence was:

"We have therefore tapered the u,v data to obtain similar resolutions of 1.1" at 3.6 and 6 cm, and of 4" at 6cm and 20cm".

This tells the reader what "these tapered data" refer to in the following sentence. I suggest that we put this sentence back, but instead delete everything from the third line from the end of p.17 to the end of the first paragraph on p.18. we could then go straight from the definition of depolarization into a sentence that says:

"Table 6 reports the mean depolarization ratio $DP^6_{3.6}$ and DP^{20}_6 on each side of each radio galaxy."

This streamlines things and leaves out the boilerplate about how to do a polarization calculation from Q and U.

p.18, line 14: "... from the integrated polarimetry, in that neither 3C55 nor 3C265 shows significant depolarization between between 8.4 and 1.4 GHz".
p.18, line 17: delete "most rapidly", use "at the higher frequency"
p.18, line 19: replace "cannot correlate it with the asymmetry of the jets" with "cannot correlate the depolarization and jet asymmetries for this source"

I agree with Rick that the next para is too much of a throwaway. I think we should either throw it away, or say a bit more, perhaps based on the plot I drew up with our data and the Garrington data both shown as functions of redshift.

Section 6:

second para: replace "our" by "the" -- they are public domain!
first line

p.19, last line:

"the brighter [OII] emission is on the SE side of the nucleus, though there is evidently also an extended [OIII] emission region also towards the NW. Thus the brighter emission line region is on the same side as the shorter radio lobe, but it is noteworthy that there is significant extended line emission on both sides and no signifcant depolarization on either."

p.20, line 6:
"For 3C356, there is much more [OII] line-emitting gas on the southern side of the source, which has the closer lobe whether either feature D or feature E is the radio core."

p.20, line 12:
"Furthermore, for the two sources with excess line-emitting gas on the shorter-lobed side, there is no significant depolarization asymmetry. There is therefore no evidence from these data that the emission lines and the depolarization probe asymmetries in the same medium."

Section 7:

line 2: for "our" use "these" ?

p.21, line 1 : delete "we feel that"
line 3 : "D has the flat radio spectrum more typical of compact radio cores"
line 18: "three (3C22, 3C324 and 3C 356)" (drop "RGs")

Acknowledgements:

Is Linda XXX related to the H.Joseph I've occasionally thanked for help with VLA work?

References:

Baars et al. - isn't it "Witzel", not "Wizel" ?

Fomalont and Perley 1989 - delete the "by" on line 2, "eds." is short for "editors" and no "by" is needed .

Table 5:

"Core-lobe distance" is undefined.
"Sizes (JMFIT)" is a horrible title -- don't we mean "deconvolved FWHM" ?
Are they the deconvolved widths?

I haven't checked Fig. Caps. yet or looked up the references. Do you want those checks done, Jack?

Re the grey scale images:

The one of 3C22 helps to convince the reader that there may be a continuous jet in this source, and the one of 3C265 shows the limb-brightening of the lobe a bit more clearly. But it's not clear that the others add anything beyond the info in contour plots---would it be better for these to use ones that were not saturated on the hot spots, to help address the hot spot questions instead?

Re whether we need to see the paper again:

Not for any major rewrite, but I'd like to know how the hot spot issues turn out and it would be real easy to pull the "about-to-be-submitted" version across the net so we could all check it for typos and not place the entire burden on you, Jack. Why not make it available for us to copy across and check through if we have time, but not necessarily to wait for further comments if there's no controversy left after this round?

From abridle Tue Sep 1 17:21:34 1992
From: abridle (Alan Bridle)
To: jburns@nmsu.edu, rperley
Subject: RG paper comments to come ...
Date: Tue, 1 Sep 92 17:21:18 –0400

Hello Jack and Rick,

In case you're wondering where I'm at in reading the RG paper draft, this is to let you know that I've gone through it once and have made some notes about things to do. I want to collect these together and send them to you both, probably some time tomorrow.

I think the science (what there is left of it!) is now in reasonable shape, and that we are indeed close to having the final draft. Jack's compression of the data reduction section has gone particularly well -- please use your text-compression skills on the QSR paper in equal measure, Jack!

The main points that bother me are:

(a) I'm not entirely happy with some of the language in the Introduction, especially as it is a tenet of unification that RG and QSR jets are *equally* beamed but that there is a systematic difference in the optical classification based only on orientation. The intro implies that QSRs are more beamed. I'll take a crack at rewriting this section this evening, and I will try to cover Rick's comments as well while I do this.

(b) It's not made clear enough which RG lobe features meet the new hot spot criterion from the QSR paper, as the term "bright spot" is still around in this text. Does everything that's called a "spot" in this text meet the numerical criteria for a "hot spot"? I think we should aim at this, and the confusing term "bright spot" shouldn't be used. The "core-lobe" distance needs to be defined for lobes that have nothing that meets the hot spot criterion. by the way. Note also that Robert has asked us for some reshaping of the hot spot criterion in his comments on the QSR paper. We will have to keep the 5% size limit, not his preferred 2%, but we'll go with his other new language in the QSR paper and should therefore use it here. Note also that Rick's problem with the "alignment" part of the jet criterion came about because you left out half of it -- the "where closest to it" clause!.
I'll collect specifics re hot spots and lobes for tomorrow's message.

(c) When I saw your point about the spectral difference between 3C356D and 3C356E I had a flashback to a paper with Ed Fomalont aeons ago -- AJ, 83, 704 (1978) -- in which we discovered from the old NRAO-GB interferometer that there are two kinds of "cores" in extended RGs on arcsec scales -- compact with spectral index <0.4, and extended with spectral indices >0.4 and a spectrum-luminosity relation. So I looked (in vain) for what we say about resolution limits for our "cores" and for D and E in particular. Could we distinguish between the possibilities (a) that E is an SSC in the extended radio galaxy, (b) that it is an isolated CSS source? Your text implies "yes, it's more likely a CSS than an SSC", but I couldn't find the evidence. Maybe we need a table of central feature properties, including size limits?

(d) I disagree with Rick re the significance of quoting calibrator positions. I think we should keep them, especially as positional discordances with the identifications are an important part of the paper. There's no way a future reader can evaluate the possible systematic errors in

our positions unless we tell her how we referenced them. (Just like the flux densities and polarization position angle scales, in my opinion).

(e) I agree with Rick that the line emission asymmetries are a bit more subtle than the text implies (this is why I was keen to show the line pictures in the paper and not leave them languishing in McCarthy's and Ilias' theses! -- do you think the line guys sometimes go a little overboard in making their correlations fit, like modern Gregor Mendels?). 3C265 in particular needs some rewording. I'll try to come up with a suggestion!

(f) I agree with Rick that the comparison of the DP asymmetries between the RGs and the QSRs is worth including given the fuss we make about it in the intro. Would you consider using my DP versus redshift plot for RGs and QSRs as a further Figure, and thus making the point that -- at the same redshift range -- there is no evidence yet for any difference between them?

I'll send you both the detailed comments tomorrow.

Cheers, A.

From root Thu Aug 27 18:09:37 1992
From: Rick Perley <rperley@aoc.nrao.edu>
To: jburns@nmsu.edu
Cc: abridle
Subject: Here they are:
Date: Thu, 27 Aug 92 16:09:32 MDT

O.K. Jack. I've been through the 'final' version, and have lots of comments, all of which are, I think, minor.

Section I. Introduction.

1) I don't think we need to mention the 'other models', referred to in the third sentence. These are mostly of historical interest, and the link between them and the current ('Barthel') model is not clear at all in the text. What we are interested in is testing predictions made by the 'Barthel' scheme. Referencing back to old ideas adds nothing to this paper.

agreed.

2) The last sentence in the first paragraph implies that the model has something to say about relative prominence of hotspots, and lobes. Really! I don't think the scheme predicts anything at all about lobes and hotspots. Perhaps a reword here is in order.

3) I think the text at the beginning of Sec. 5 should be placed in Sec. 1. The first two paragraphs of the Introduction lay the basic picture, but in the current version, we wait until Sec. 5 to describe the second observations test (polarization asymmetries). Since we state the primary test (jet prominence/sidedness) in the introduction, I think we should also lay down the (de)polarization test there as well.

✓

4) page 5, second line. ... were recently observed, ... 'Recently' is a very relative, soft, term. Perhaps we should mention a real date -- even a month and year will be adequate.

✓

Section II, Source Selection.

1) The first phrase 'The 3CR radio galaxies...' doesn't say what we mean it to say. What we want is something like 'Our sample of 3CR radio galaxies ...'

✓

2) Third line, first paragraph. 'Basically identical' -- another undefinable term. How about 'statistically identical' ? (Down with Colloquiallisms!)

✓

3) Selection Criteria... Was the selection in redshift set in order to reduce the sample size? Or to match the redshift range of the QSR sample? We should probably state which (or both). Incidentally, why wasn't a luminosity criterion imposed instead? Presumably, the 10" minimum size criterion was imposed also to match the QSR sample?

✓

4) Section 2.2, fifth line. 'all sources', should presumably be 'all five sources'.

Section III, Observing and Imaging Techniques

First paragraph, section (iii). I think this should be reworded just a tad. Something like 'Used the bandwidths shown in Table 2. These values were selected to cause less than a 5% reduction in intensity due to chromatic aberration at outer edge of each object.' The current wording is tortuous and misleading. (Perhaps Alan can further improve on my attempt)

✓

2) The flux density of 3C286 is a factor of 1000 wrong! (p 7, second line from bottom).

✓

3) I suggest dropping the whole of the paragraph on top of page 8, and Table 3 as well. Who cares what the calibrators were, or what their flux densities or positions are? I don't, and I doubt a single reader out there gives a hoot. They need to be assured that we know how to calibrate, and that we did it correctly. That paragraph, and that table, don't do it! The procedures are boringly standard now.

— fourier of calibrators are relevant.

Section IV. The Images.

Congratulations on a brave attempt to do what nobody has done before -- define a hotspot! I have a couple of quibbles:

For (2) Jet, condition (c) 'aligned' with the nucleus. I would think that, since many jets are both aligned and curved, we might note that precise linear alignment is not necessary, but rather a 'plausible physical connection' be seen from the nucleus, through the jet candidate, to somewhere beyond.

For (3), I would vote that 'Lobes' exclude 'Hot Spots', as well. Thus, put 'Lobes" after 'Hot spots'. But I'll happily be voted (or shouted) down on this one.

For (4) 'Hot Spot', I'll note that conditions (a) and (c) are essentially the same. (Where did the factor of four come from?)

A couple of syntactical things: page 10, top paragraph, last line: 'No core or jet was detected'. (Not 'were'). And, last paragraph, first line, '...are fairly WELL aligned ...' (Missing word).

page 11, and elsewhere. When discussing brightnesses, I vote (strongly) that we adopt a standard unit of brightness, and use it whenever we have a well resolved object. I think the standard unit should be mJy/sq. arcsecond. (NOT mJy/beam). Is it possible to adopt this in our paper?

page 13, on 3C265, first paragraph, last line. 'Several Components...' It seems there are only two, A, and C.

page 15, 3C356, the famous pair of possible nuclei. Since we have three frequencies, it seems that we should be able to make as statement on which of our two nucleus candidates is the more plausiable, based on spectrm. (Of course, if they are both nuclei, this test will likely fail, but it seems worth mentioning, at least). I've raised this before, but I can't recall the answer.

Section V, Depolarization Analysis.

I've already stated my believe that the first paragraph should, at least in part, be put into the general introduction.

page 18, middle. When you say 'depolarizes most rapidly', you really mean 'depolarizes at a higher frequency' (Right?) Why not say it this way? 'rapidly' normally implies speed, not wavelength.

page 18, middle. We should state, in words, that 3C55 and 3C265 show now depolarization asymmetry over the wavelengths we used.

Section VI, Optical Emission etc.

Well now. IT's time to judge what is symmetric, and what is not. My first candidate for a false asymmetry is the [OII] emission from 3C265.

What I see when I look at the image is a wonderfully symmetric emission region centered on the galaxy, plus two (unresolved?) blobs, one above the radio source, one far beyond (to the E) the lobe. On the basis of this, I find it a little far-fetched to claim wonderous physical effects like explaining the lobe distance asymmetry (which is very small in this particular object). I WOULD be convinced if there was any reason to believe the OII emission was distributed everywhere around and beyond the radio lobes, and was CLEARLY more densely distributed on one side than the other. I don't see that here! (But maybe you do, and can explain this to me).

Beyond this, it's not clear if there should be any connection between a DP asymmetry and the presence (or absence) of [OII]. There Could be, but Should there be? Attempts to connect these phenomena (as stated on top of page 20) are rather like grasping at straws.

For 3C356, I will agree there is a strong OII asymmetry, but since

the asymmetry is located far from any radio emission (or at least, the region where the optical measurements were made is), it's rather hard to make any statement whatever about connections.

a questionable business indeed, this optical emission stuff.

Section VII. Discussion.

page 21, second line 'substantiate the proposed ids'. Is this the right word? Is 'confirm' better?

Second paragraph, middle. 'Qualitative result'. It seems that the detection rate ratios (1/5 vs 13/13) is pretty quantitative. The sensitivies are about the same, so perhaps we could drop or change the damning adjective 'qualitative'.

p 21, third paragraph. Probably, a few words to compare the DP results for these radio galaxies to that for qSRs should be put in (even if no difference can yet be discerned). I don't think we should be silent on this important question.

ANd FINALLY...

I can hardly wait to meet Linda XXX, (referenced in the acknowledgements). Does she have a sister, Brenda XXXX? Pretty risky stuff, there Jack.

I don't think I need to go through another round. Make what changes you think are appropriate, and let it go (if Alan agrees).

Rick

From root Tue Jun 30 11:44:08 1992
From: rperley@sechelt.AOC.NRAO.EDU (Rick Perley)
To: jburns@nmsu.edu
Cc: abridle@sechelt.aoc.nrao.edu
Subject: Comments on latest draft of Fernini paper
Date: Tue, 30 Jun 92 09:49:18 MDT

Hi Jack 0! I have gone over the paper, here are the results.

Overall, I'm fairly happy with the paper. There are a number of small, almost picky, comments, but no major ones. I think the claims being made now are reasonably and supportable. This is not a ground-breaking paper, but is good enough to be published.

1) Title Page. My address is wrong. (Also, I usually prefer to go by my initials, but it is more important that all four authors be identified the same way. If you and Ilias want your first names spelled out, then I will too.)
2) page 3. I should think that of the two motivating studies which preceded this work, the Barthel paper, and its predecessors, should be identified first. The Bridle et al paper (1992) would, in my view, be ranked second (even if the proposal preceeded the Barthel work). My reasoning is that the Barthel paper proposes a clear, testable model (not for the first time, incidentally, Alan and I had stated essentially the same idea in our 1984 review), which has generated much attention and plenty of observing. The QSR study of Bridle et al. can be considered one of many detailed studies, and the current paper is another.
3) page 3, 3rd and 4th lines from bottom. The term 'radio galaxies' is mentioned twice, in different meaning, from what has earlier been defined. Earlier in this paragraph, radio galaxies are defined as 'unbeamed' quasars, while here, they are considered to be the general class, from which quasars are drawn. I suggest we define the overall class as 'luminous extragalactic radio sources', so we can call quasars those which are beamed, and oriented near the the line of sight, and radio galaxies as those which are not.
4) page 4, top. The Barthel model presupposes that jets are relativistic. We should probably mention this specifically.
5) page 4, line 6. We drop the Laing-Garrington effect in without introduction and explanation. A few words more here might be helpful.

Sections II and III. I have few comments here. I suggest, though, that if length is a problem, we whack down section III significantly. The process of calibration, self-calibration, etc., is now so familiar, we shouldn't have to go into this level of detail.

6) p7, section (iii). The bandwidth reduction quoted is, strictly speaking, for a point source.
7) p7, bottom, and in numerous other places. At the risk of being pedantic and boring, I will repeat my complaint against use of the word 'array', when 'configuration' is what is really meant! (I promise not to bother you again with this one).
8) p9, middle. AHA! You used 'Configuration' here! Congratulations.
9) p13, and in numerous other places. Why is the word 'core' repeatedly surrounded by parentheses? It's ugly. If suggestive language is the problem, I suggest we define our interpretation of the word 'core' in the introduction, along with 'lobe', 'jet', and 'hot spot'. If 'core' is to be paren'ed, we should do the same with all those other words. We can escape all of this with a short, defining paragraph, in the intro.
10) p13, bottom. Do the POSS show anything under central feature D?

11) p16, middle. Usually, the spectrum of a compact feature will tell us what is a core. Object D appears to have a steeply inverted spectrum (judging from the published maps), so would be my candidate. Note that I am not accepting what the paper suggests -- that both knots are cores. I'm betting that E is a jet knot, which happens to lie upon a galaxy image. Low probability, possibly. Your conclusion is right -- a real jet will likely have to be found to be really sure. But, what are the spectra of the two knots? Perhaps E has a steep spectrum.
12) Equation 1. You have (wavelength squared) in both numerator and denominator. It should be just the ratio of the fractional polarization at the two wavelengths. Since later uses of DP have the two wavelengths attached, you should perhaps attach them to the definition as well.
13) p18, middle paragraph. This confused me. I presume, in the 5th line of this paragraph, you meant 'I and P maps', not 'I and p maps'. Presuming this, I am a little wary of calculating the mean DP in this way, for this heavily weights the brightest areas (both in I and P). This results in an intensity weighted depolarization, whereas an area-weighted one is probably more meaningful. Given the poor SNR, your perhaps have little choice, but I'd feel better if you blanked the p maps at, say, 5 sigma (or even more), then averaged over them.
14) p19, top. The Laing and Garrington papers deal (almost entirely, Ithink) with QSRs. This is an important note, and should be mentioned when you are comparing the RG DPs to Laing and Garrington. Mentioning this will also help deflect criticism that you are singling out 3C47 for comparison.
15) p20, middle. The word 'excess' is (a) I think inappropriate here, and (b) is repeated twice in the same line (7). How about 'asymmetry'. 'Excess' implies too much (like in eating, oink, oink), while asymmetry is a prettier, more meaningful word, especially in this context.
16) p21, top. Nucleus of 3C356, same point as (11), above.

O.K., That's it.

Rick

From root Wed Feb 12 03:52:42 1992
From: rperley@zia.AOC.NRAO.EDU (Rick Perley)
To: abridle@zia.aoc.nrao.edu
Subject: My comments on Ilias' latest draft
Date: Wed, 12 Feb 92 01:53:03 MST

Good morning. Insomnia (which strikes me quite often) can be useful. I'm going to use the current occurance to set down my basic problems with the Fernini et al. paper.

As I said yesterday, the basic problem is that Ilias and Jack are bound and determined to find counterjets and to derive results from these 'findings' despite the absence of any real evidence. There is in my opinion only one discovered jet here, and no counterjets. Period. As you have noted, this low detection rate, compared to QSRs, is itself useful and interesting. And I think the paper should only go about that far.

A few details:

1) The Introduction is rather weak. It does not convince the reader that the author has mastered the subject.

2) The selection criteria, especially the upper limit, and the use of the optical subset have, or might have, (respectively) introduced dangerous biases. This subject I needn't lecture you about!

3) There are many, many imprecise statements littered throughout. Things like 'well aligned', without any description of what this means (p. 10), and 'somewhat resembles' (p 12).

4) a specific note: on page 11, Ilias notes the jet polarization to be less than 4%, but this indicates the jet must have been detected with about 25:1 SNR (assuming the polarized and I noises about equal). Yet the image shows no such DR. How did he get such a low limit?

5) The common discrepancies between radio and optical cores is rather worrisome.

6) Many references to depolarization are made, with results discussed. Yet no data are presented, and the reader is referenced to an unwritten article. Bad Form!

7) The 'minimum' and 'maximum' flux densities for jets (p 17) are predicated on the very dubious identifications proposed by Ilias. Especially ludicrous is the 'minimum'! In my view, the 'maximum' for all but one of these sources would be given by the rms noise multiplied by the best-guess solid angle of one jet.

8) the K-S test result on p 18 could be gotten without recourse to the dubious methods mentioned above.

9) The word 'closer' on p 20 confuses physical closeness with angular closeness.

10) To my mind, the summary on p 21/22 is the nadir of the paper. In stating that the 6 radio galaxies satisfy at least 3 of the following criteria, he has stretched the truth way too far! In my view,

point (i) is always true to some level in every object
point (ii) is based on one source (maybe 2, including Garrington's)
point (iii) is based on results not shown
point (iv) is based on two objects.

And the sentence at the end of these points left me speechless!

11) I was dismayed to see Ilias still holding onto his optical depth argument. With the parameters given (which assume a filling factor of 1!) would mean that NO radio source with line emission would show any bridge or lobe emission at 20cm or lower. I think a quick perusal of the data will show that many r.g. with line emission also have straight low-frequency spectral, and lots of bright bridge emission. Also, Ilias fails (again) to tell us what frqeuency his eq (1) is calculated for, or indeed to note that the absorption is HIGHLY frequency dependent. Note that on page 23, Ilias calculates a filling factor of 10**-6, which if applied to his absorption argument will eliminate this effect.

12) p 25, more mushy statements: 'may favor', and 'sometimes observed'.

13) The last sentence (before the acknowledgements) confused me greatly.

In summary, if athe paper is published as it is now, we'll be laughed off the planet. It's time to put our foot down!

Rick