July 15, 1985

Alan Bridle
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Dear Alan,

I have made some changes in the NGC 6251 paper based on your letter of May 9. The workshop paper by Wardle and Potash is now commented on, although I do **not** think it provides strong evidence against the hypothesis that large-scale jets in high-luminosity sources are relativistic. They do not appear to have eliminated the emission from the jets, which might be beamed, from the total flux densities used to define their sample. A similar sample of 3C quasars, defined by using the lobe flux densities only, shows one-sided jets in 4 of the 8 largest sources -- quite different from Wardle and Potash's result. So I am not convinced that the "complete samples" from the 3C and 4C surveys are really oriented randomly.

I have included additional references where necessary, and have removed the arguement about hot-spot radiative lifetimes (you are right that this does not really apply in the case of NGC 6251). The possibility of asymmetric dissipation is mentioned, as is the fact that the "precessing beams plus wind" model does not fit the observed large-scale structure in detail. However, I still feel that it does reproduce the over-all shape of the source quite well, which was all it was intended to do.

Thanks for your comments. It is clear that you have read the various drafts far more carefully than most of the coauthors, and I appreciate it. The paper is certainly better as a result.

Sincerely,

Dayton

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May 9, 1985

Dr. D. L. Jones
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Dear Dayton,

I enclose my comments on the re-drafted paper. It is improved, but there are still some deficiencies which I hope you will remedy before it goes to the journal. The main ones are 1) failure to note the statistics of jet one-sidedness in complete samples of powerful sources – these mean that the Doppler approach cannot explain *all* jet one-sidedness, though one is of course allowed to construct models for *individual* cases such as this, 2) lack of references to prior discussions, particularly of sidedness effects and flip-flop models, 3) failure to mention the possibility of roughly symmetric energy transport with asymmetric dissipation to synchrotron radiation, 4) transference of the hot spot time-scale arguments from powerful sources to the warm spot of NGC6251 without justification (if you want to make these arguments here you must use the data for NGC6251 itself), 5) lack of comment on the poor detailed fit of the precessing jet model to the brightest curved parts of the VLA jet and counterjet in NGC6251. If the paper goes to print with these items undealt with, I shall be obliged to refute the arguments made here under (1), (3), (4) and (5) when Rick and I write up the other data we have on the very large-scale structure.

Note that the Proceedings of NRAO Workshop No. 9 which were mailed to you recently are a copious source of missing references, as I have indicated in several places in the text. If you want to cut down on the number of references, you should at least refer to the review papers in that Workshop – it has been very widely circulated. The full reference should be "Physics of Energy Transport in Extragalactic Radio Sources", Proc. NRAO Workshop No. 9, ed. A.H.Bridle and J.A.Eilek (NRAO: Green Bank) 1985. There are also some places where reference should be made to the conclusions in the main VLA paper on NGC6251 – I have indicated these in the text.

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April 30, 1985

To: NGC 6251 collaborators

From: Dayton Jones

Subject: Final draft of paper

Enclosed is a new version of the NGC 6251 paper. It has gone through substantial revisions during the past few months, and I think is quite a bit better than the earlier version you received before. If you have any comments or suggestions on this paper, please send them to me by the end of May. I intend to submit the paper to Ap. J. on June 17, unless major changes are needed (I do not expect this to be the case). Keep in mind that it is not possible to please everyone on all points with this many coauthors -- some of the sections in the first draft which were cited as being particularly good by some people were the same sections that others wanted to have removed entirely!

I hope you find this draft both interesting and acceptable.

Sincerely,

Dayton

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17 September 1984

Dear Dayron,

Your draft of the NGC6251 paper has taken a while to reach me, as you sent it to the VLA instead of to Charlottesville (which is where I work these days!). As I am about to leave here for a few days, I have only had a chance to give it one reading, with these very quick comments. Most of them stem from apparent misquotes from Bridle and Perley 1983 and glossing over the detailed arguments made by Perley, Bridle & Willis 1984. In general the BPB3 reference is supplanted by the more detailed discussion in PBW84. In particular, I don't agree that we ever set unequivocal limits to ne in the kpc scale interjet, due to the field configuration assumptions that you have to make.

I'll try to get more detailed comments to you when I return in a few days.

Best wishes,

Alan B.

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p.13 last line — "is discussed", not "will be"

p.4 line 4 from end — ~40:1 100" from core, >200:1 by 240" from core (PBW)

p.4 line 2 from end — "shows", not "showed"

p.5 line 12 — indices only

p.14 last two sentences seem out of place in the context of a paper dealing explicitly with NGC 6251

p.15 line 7 — delete everything after the comma. Also delete the last sentence of para. 1 — unnecessary detail.

line 11 — hcm hybrid map with lower dynamic range, shown as Figure 6.

p.16 line 1 — "effect" → "artifact" ?

Why is it unlikely that the spectral index becomes less steep with distance? This happens at the first knots of several large scale jets (IC4296, NGC315) and is consistent with shock acceleration or turbulence running on the first bright knots by accelerating high-E particles. Either this is or is not an instrumental effect, and this should be decided on principle from the instrumental properties, not judged in advance by applying preconceptions about the physics!

p.18 line 5 — "the inner ~4' of" the convergence is linear (the outer convergence is manifestly curved).

p.19 line 3 — I don't like the description of what we said before as a

"decrease in brightness varies with decreasing distance from the core", as it implies a monotonic variation and we never said that. It's a straw man you're putting in our mouths in order to knock down! What we said was that the ratio changed, so that the counterjet is not a replica of the main jet. That seems abundantly confirmed! Why not give the ratio as a function of distance from the new data, and point out that it is not monotonic, rather than putting words into the mouths of the earlier papers? There's no need for this straw man approach!

p. 19 The S symmetry is modified here by stretching. Note also that the warm spot of the Np lobe can now be seen to correspond to the bright band in the counterjet ~12' from the core on the Sf side.

p 20 I object to the notion of precession with variable cone angle and period. Isn't this better described as wandering of the ejection axis. The notion of precession invokes ideas of periodicity and repetition. Where only a few "cycles have been seen and these do not repeat, I feel that precession is altogether the wrong word.
Why don't we just say that the data could be explained by wandering of the ejection axis.

This wandering could also supply a forcing function for the perturbation normally described as the helical Kelvin-Helmholtz mode. Thus the S symmetry could have a wavelength determined by the K-H theory and a phase set by the forcing.

p. 21 line 8 Bridle and Perley 1983 give the spectral index as "everywhere within errors" of 0.64. PBW say 0.64 ± 0.5.

↓

Change to say $\alpha \approx 0.58$ to 0.64, not $\alpha = 0.6$, and reference PBW.

p. 24 line 13 PBW are certainly to point out that you can only derive n_e in the jet if you make assumptions about the $\underline{B}$ configuration. Also that the Saunders estimate for depolarization is entirely false due to beam smearing. They conclude that $n_e \approx 1.5 \times 10^{-3}$ cm^{-3} at FWHM $= 32''$ if one assumes a disordered slab with many fluctuation scales along the line of sight in the normal manner, but could be $< 4 \times 10^{-3}$ cm^{-3} in a randomised CH model and much larger in a "sheared Laing" model. Bridle and Perley (1983) do not say $\ll 1 \times 10^{-3}$ cm^{-3} !! You should drop the last sentence of paragraph 2 altogether, and replace with "The constraints on n_e on kpc scales are model dependent, as discussed by PBW in some detail. Simple slab models for the lack of Faraday depolarization suggest $n_e < 1.5 \times 10^{-3}$ cm^{-3} $\sim 32''$ from the core, but the assumptions made in such models are open to question, and much larger densities could be present in some field geometries, such as the "sheared Laing" fields discussed by PBW."

p. 25 para. 2 This is not a "difficulty" with the reversed model, but a feature of imagination. The hard part is to make a reversal time long compared with the nozzle time scales. How about: "the mechanism responsible for such a reversal time scale (long compared to the dynamical time scale of the nozzle yet short compared to the convective time scale of the jet) has yet to be defined, however."

p. 28 last line This is discussed in much more detail in PBW than in BP83!

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