DISCUSSION ON THE PAPER BY BROWNE (p. 1)

SAUNDERS : If BL Lacs are to be considered as radiogalaxies seen end-on, shoudn't we see emission lines in BL Lacs ?

BROWNE : I am in fact suggesting that BL Lacs are counterparts of the weak Fanaroff-Riley class-I radiogalaxies which, of course, do not have strong emission lines.

SCHEUER : The evidence for the slow non relativistic motion of double radio structure is mostly based on old data and refers chiefly to the diffuse structure. I think it is still true that, for all we know so far, the very compact hot-spots could be moving at a few tenths of c, so that the fact that jets point at the most compact hot spots may not be evidence for the intrinsic one-sidness of jets. We need more evidence on this point.

BROWNE : Yes, I agree with you. If hot spot emission is beamed, it might also alleviate some of the problems such as the confinement of very compact hot spots in some lobes.

BURKE : You cannot invoke statistics in the case of a single example like 4C32.69. Single observationally selected objects are often remarkable and improbable. Even with a carefully framed a priori hypothesis, a single measurement gives an estimate of the mean, and no estimate of the dispersion.

MILLER : In the unified scheme for BL Lac-type objects, presumably the optical emission must be relativistically beamed. In the quasars' unified scheme, the optical emission should not be so beamed that the emission-lines are drowned out (the line-to- continuum ratios are similar in compact and extended quasars). Are the two schemes compatible?

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BROWNE : Yes, provided the beamed component of quasar optical emission never exceeds by very much the unbeamed continuum. Some optical beaming is necessary to explain why core-dominated quasars are more likely to be OVV's.

VAN BREUGEL : If BL Lac-objects are relativistic jets seen end-on, wouldn't you expect that VLBI-jets would point towards those lobes?

BROWNE : Yes.

VAN BREUGEL : Schilizzi and I have found a 25 milli-arcsecond jet in Mk501 which is nearly perpendicular to its large scale structure as found using the WSRT (e.g., van der Laan et al., this volume).

BROWNE : You are talking about one object. On average jets and lobes line up, but if there are small bends these will be amplified in end-on sources.

BRIDLE : If a jet with bulk relativistic motion also wiggles, it may then appear as only a few bright knots along its path, due to variations in the Doppler boosting. It may then not be termed a "jet" by observers. This makes it difficult to infer the statistics of whether "jets" brighten or dim where they bend. The ones we call jets are selected to be the ones which show the most continuous emission.

DISCUSSION ON THE PAPER BY CAWTHORNE (p. 7)

WRIGHT : What is the median redshift of the QSOs in your sample for which redshifts are available?

CAWTHORNE : The red-shift distribution is strongly peaked in the interval 1.0 - 1.5, with some 20 % above 1.5.

JAUNCEY : Accurate radio and optical position measurements will be particularly important in identifying the radio component that lies in the nucleus of the optical object. These should be made, where possible.

CAWTHORNE : Unfortunately MERLIN does not give absolute positions. However good radio and optical positions would help us to understand some of the more mysterious sources such as 3C454.

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DISCUSSION ON THE PAPER BY VAN DER LAAN (p. 5)

READHEAD : Have you done any simulations of the self calibration scheme at Westerbork. I ask because in a number of your examples there are north-south extensions which may not be real.

VAN DER LAAN : Yes, De Bruyn and Noordam have done many simulations and can assess the realiability of such features. I refer you to De Bruyn's paper.

READHEAD : The dominant p.a. of 3C371 is east-west in VLBI scales. The hint of structure in p.a.-30 deg, which can be seen in our first survey map, is probably due to poor (u,v) coverage since we do not see it in maps at both 6 cm and 2 cm.

VAN DER LAAN : The large scale faint feature in p.a.-37 deg is one we are very confident about. It is, moreover, consistent with a larger scale unpublished 50 cm Westerbork map showing a halo of about 3'. At 6 cm this is resolved out except for its brightest ridge.

PERLEY : My VLA observations of 3C371 do not show any trace of a feature extending from the core near p.a.- 40 deg. My map has 16" resolution at 20 cm wavelength.

VAN DER LAAN : The feature is present only at well below the 1% of peak flux level. Your dynamic range is marginal for this purpose, although I see a hint in your map near p.a. +140 deg.

DISCUSSION ON THE PAPER BY PEARSON (p. 15)

JAUNCEY : The properties of the "Compact Doubles", i.e. two widely separated components with roughly equal brightness, and their high association with galaxies, make them good candidates for gravitational lensed objects. Our observations of 1934-63 (see Preston's talk) support this.

SUBRAMANIAN : We have tried to model the "compact-doubles" as examples of gravitational lenses. The small separations in these doubles are not easy to model. Two kinds of model roughly fit : 1) a model with 3 images, with 2 of the images very close together and a faint third image farther away (about 1"); 2) a model with a black hole of 10 Mo producing two images. Our work is as yet very preliminary.

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BARTHEL : Concerning the compact doubles: what limit on the size of extended structure can you give? did the VLA find all the flux density?

PEARSON : The VLA data have been published by Perley (1982, Astron.J., 87, 859). The resolution was about 0".4 and the upper limit on extended structures was 0.2% of the peak brightness.

VAN BREUGEL : Does the galaxy identifed with 2021+614 have forbidden narrow line emission?

PEARSON : Bartel has obtained a spectrum of 2021+614. He tells me that it is a narrow-line radio galaxy with a redshift of 0.22.

WRIGHT : I would like to comment that all the known radio-selected QSO's with red-shift larger than 3 show evidence of a "humped" radio spectrum.

MUTEL : Hodges, Phillips and I have recently completed a VLBI Survey of 10 sources selected on the basis of a 'peaked' spectrum (between 0.5 and 2.0 GHz). We found that only 3 of them possibly have simple double structure at 18 cm. This confirms that not all peaked spectrum sources are compact doubles (but probably all compact doubles have peaked spectra).

SHAFFER : Is it clear which component in the 5 GHz map of 3C390.3 is really the core? In some sources, the brightest component is not the core.

PEARSON : The weak north-western component in the 5 GHz map is partially resolved; it has also been detected in Linfield's 10.6 GHz observation. Whether the bright, unresolved component is "really" the core, one cannot say, however.

DISCUSSION ON THE PAPER BY WILKINSON (p. 25)

READHEAD : I just want to add a comment on the class of steep spectrum compact sources you discussed. In two cases (3C147 and 3C309.1) Simon has found evidence based on the low X-ray emission, for relativistic motion towards us. This may therefore be a common property in this type of source.

WILKINSON : Whyborn in the next talk will show new evidence on the

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3C147 core. The situation regarding these "predictions" of superluminal motion is clearly more complicated than we realised.

VAN BREUGEL : Steep spectrum cores in galaxies tend to have simpler radio structures. I would expect that these cores are in gas poor environments i.e. ellipticals. Could you specify what type of galaxies you mean?

WILKINSON : They are ellipticals.

REES : You showed evidence that the gas in quasars was more disturbed (and at higher pressure) than on the radio galaxies. Although this could indicate that quasars involve mergers, could it not simply indicate that the quasar activity has itself heated and disturbed the gas?

WILKINSON : The only direct evidence that these objects may be associated with mergers is in the specific case of 3C48 which resembles the 0351+026 system. This system is clearly a violent merger of 2 rich gas galaxies. The arguments for the other quasars being in mergers is therefore purely circumstantial. The evidence is consistent with the hypothesis but does not prove it.

HUTCHINGS : Optical imaging data exist on 3C48 and should be compared with your 1" resolution radio maps.

WILKINSON : Yes! We would like to do this but haven't yet.

NEFF : Are the distorted quasars in clusters, i.e. are there surrounding galaxies with which to merge?

WILKINSON : They are all very distant objects, with red-shifts larger than 0.5, apart for 3C48, so very little is known about their environments as yet.

DISCUSSION ON THE PAPER BY WHYBORN (p. 29)

REES : To display superluminal motion, a source must not only involve relativistic outflow, but also fluctuate (or be "blobby") on the scales that can be observed. The absence of measured superluminal motions is not therefore in itself incompatible with relativistic outflow. There is therefore no inconsistency with inverse Compton arguments for

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relativistc motion (e.g. those of Simon et al., 1983). The only strong argument against relativistic outflow in the M87 jet would come if significant limits could be set to the proper motion of the knots.

PREUSS : What Rees said is in my view the reason why one should avoid "predicting" superluminal motion from evidence for relativistic outflow.

COHEN : The archetypical superluminal sources 3C273 and 3C345 both show the brightest component to be in the middle, not at one end, at low frequencies (2.3 GHz) where the "core" is self-absorbed. Thus 3C147 is not necessarily different from the superluminals, except for the NW bulge. How certain are you that the NW bulge is real?

PREUSS : We carefully looked at the data and we are quite certain that it is real. In fact it is just here, along the direction of this NW extension, where the only structural changes may have taken place between April 1981 and December 1982.

SIMON : 1) The prediction of v/c = 9 by Simon et al (1983) assumed maximum possible angle to line of sight of 8 degrees. In that model, $v/c \leq 0.5$ implies $\theta \leq 2$ degrees. 2) There were significant changes at 18 cm over 6 years, but it is not possible to unambiguously interpret these changes as superluminal motion (Simon, Readhead, and Wilkinson, this meeting). 3) Model of Simon et al (1983) was of course very simplified; the calculation of bulk relativistic motion is still good, although simple superluminal motion is apparently not occuring.

DISCUSSION ON THE PAPER BY BACKER (p. 31)

SCHILIZZI : Could you remind us what the spectra for the sources are?

BACKER : Both 3C84 and 3C345 have slightly negative logarithmic indexes at mm wavelengths ; both have been decreasing slowly over the past year or two. 3C273 has been varying rapidly since 1981; its spectrum at 89 GHz was inverted during the early stages of the 1983 outburst.

DISCUSSION ON THE PAPER BY ROBERTS (p. 35)

JAUNCEY : I would like to ask the speakers to use the properly designated IAU names for their sources.

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MUTEL : Could you not eliminate the time-variable phase difference (RL) problem at the VLA by using the 'VX' mode (i.e. apply a previously obtained gain solution) instead of phasing in real time?

ROBERTS : The right-left phase drift typically takes place on the scale of hours. Thus the VA-mode is suitable for each coherent MKIII scan. The scan-to-scan variations are removed from the VLBI by use of the RR/LL ratio on the VLA-Green Bank baseline. At 6 cm this can be done on a timescale of minutes if needed, as the signal-to-noise in the parallel fringes is necessarily very high (1000s) if we are able to detect the cross fringes reliably.

MOFFET : Is it clear that the cross-polarization coefficients (the D's) are constant in time? For example, they depend on antenna pointing errors, which are certainly variable under windy conditions.

ROBERTS : As yet we have no information on the time-constancy of the instrumental constants, although we are aware of the possibility of the kind of problem you suggest. With more dual-polarization equipment it would be possible to check for this possibility.

DISCUSSION ON THE PAPER BY GRAHAM (p. 43)

SHAFFER : Both 3C 236 and 3C 298 look like they are consistent with Larry Rudnick's suggestion of alternating injection in that the brightness peaks on one side of the core would "fill in the gap" on the other side.

MENON : Which component do you suggest produces the low frequency turnover?

GRAHAM : The small, bright western hot-spot will obviously turn over, but it may well do this much higher than 100 MHz.

READHEAD : Surely 3C298 is a steep spectrum compact source, and the turnover at 100 MHz is from structure 0.5" in size which you don't see on your VLBI map. If observed with low dynamic range 3C147 would look very like this.

ANANTHAKRISHNAN : The source has a large percentage of its flux in a diffuse component of 400 mas size since IPS at 327 MHz shows that only about 20% of its flux is in a component smaller than 400 mas. One

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should be able to see this in low resolution low frequency VLB.

GRAHAM : The VLB map shows about 70% of the total flux at 18 cm. The total extent of the source at lower frequencies is about the same as at 18 cm, so there is no very large diffuse component.

DISCUSSION ON THE PAPER BY MENON (p. 45)

VAN BREUGEL : The emission measure for free-free and optical line emission are similar. One might expect therefore that low frequency turnovers for bright emission-line objects occur at higher frequencies. Wouldn't it be possible to explain the red-shift dependence of the low frequency turnovers as being due to free-free absorption ?

MENON : The measured angular sizes, where available, agree very well with the sizes computed on the basis of synchrotron self-absorption model. It is possible that for the smallest sources the shape of the spectrum below the turnover frequency is influenced by free-free absorption.

DISCUSSION ON THE PAPER BY JONES (p. 47)

SHAFFER : Apropos of the fact that your dynamic range is considerably less than expected from SNR considerations, one should remember that amplitude closure may not hold at high frequencies, where non-linear phase variations within an integration may affect the several baselines differently.

JONES : I agree. All of the assumptions that various errors remain constant during an integration interval are less valid at higher frequencies, and consequently the current self-calibration techniques will not necessarily assure closure as well as at lower frequencies. Baseline-dependent errors may be limiting the dynamic range of VLBI maps at any frequency, and are very difficult to correct without good point sources.

DISCUSSION ON THE PAPER BY BRIGGS (p. 49)

ROBERTS : Could you comment on the relation of the changes in the VLBI

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visibility and the source outbursts? How does this relate to Wolfe's model for the source?

BRIGGS : The model that has the source continuum driving the cloud spin temperatures, and therefore optical depths, was lightly constrained by the observations taken in the period 1976-1981 (Wolfe, Davis, Briggs, 1982). Since then the source continuum has varied without the line depths varying in the manner predicted by the model. The model has been abandoned.

SIMON : Based on 1661 MHz observations of 0235+614 (used in calibrating a VLBI experiment), the size is roughly 1 mas, with no extended emission to the 0.5 % level.

BRIGGS : The source may be a little different at lower frequencies and may change with time. The model may work well for a 1 mas source. The gradients in opacity will simply need to be steeper.

DISCUSSION ON THE PAPER BY WEISTROP (p. 51)

JAUNCEY : Do you have any Merlin map at 1" resolution of this object to match with your optical photo ?

WEISTROP : No, we don't, but de Bruyn will show tomorrow a map made at Westerbork.

JAUNCEY : Radio astronomers making these type of high resolution surveys dicussed at this Symposium, should remember that 4 m optical telescopes make perfectly acceptable maps at 1" resolution and should be included more often.

NEff : Is there any evidence for spiral structure in the optical nebulosity ?

WEISTROP : We have not yet looked for such structure in our data, but plan to.

HUTCHINGS : We have no optical data on this object. If the galaxy is a triaxial ellipsoid, or an inclined disk, the projection on the sky of an orthogonal radio jet need not be at right angle to the optical axis.

WEISTROP : If the disk has symmetry with respect to the minor axis, an

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orthogonal radio jet will appear orthogonal projected on the sky.

BIRKINSHAW : The "optical" structure relevant to the VLBI jet is not that of the nebulosity of 1219+28, but rather the very central optical structure (hidden by the BL Lac light). A comparison of the morphology of the fuzz and the VLBI structure is not very meaningful.

WEISTROP : If the central optical structure is aligned with the minor axis of the fuzz , the comparison is meaningful.

DE BRUYN : I will show a low-resolution WSRT redundancy map tomorrow, indicating that 1219+28 is one of the best "point sources" (on arcsec scale) known sofar, but that there is a very faint extended emission region to the south-west in the direction of the companions.

DISCUSSION ON THE PAPER BY COTTON (p. 53)

MENON : Optical quietness should not be decided on the basis of the single epoch plates of the PSS since variability is a characteristic of these sources.

COTTON : For a single source this is true but there are a number of sources in my sample and none appears on the PSS prints.

WEISTROP : Are there any plans for deep optical observations of these sources using CCD detectors and large optical telescopes? Such observations can go several magnitudes fainter than the Palomar Sky Survey.

COTTON : The possibility of this type of observations is being explored.

ZANINETTI : I would like to know why you have considered inverse Compton losses and not synchrotron losses?

COTTON : Both inverse Compton and synchrotron losses were included in the detailed models; in all of the examples I have shown that inverse Compton losses are more significant than synchrotron losses.

COHEN : Bulk relativistic motion towards the observer apparently makes the problem of explaining the spectrum more difficult. Why not look into bulk relativistic motion away from the observer?

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COTTON : Relativistic motion away from the observer will greatly reduce the observable flux density. Since several of these sources are relatively bright (about 5 Jy at 400 MHz) they would be extremely bright sources if viewed from the forward direction.

SAUNDERS : Your theme seems to be that there may be a new class of object with compact radio properties which are optically "quiet", the criterion for optical silence being no identification on the PSS. In fact, several sources are known (from surveys such as Peacock and Wall, 1982, M.N.R.A.S., 198, 843) with the same radio properties which have no PSS identification but which, on deeper investigation, simply turn out to be faint galaxies. Their radio and optical properties are unexceptional. Is there any reason to think that your sources are different from those?

COTTON : The main point I was trying to make is that these sources radiate via synchrotron radiation, though probably have significantly weaker magnetic fields than the flat spectrum sources. On the basis of the current data for 2147+145 a simple synchrotron model predicts more optical inverse Compton than is observed and very short relativistic electron lifetimes. None of the observed properties of these sources are peculiar. Only an attempt to determine the physical conditions indicates a difference between these sources and the better studied flat spectrum sources.

DISCUSSION ON THE PAPER BY FANTI (p. 57)

SIMON : A curiosity in 3C138 is that there is a 200 mJy component 1-2 minutes of arc away, along the jet, to the south-west. It may be related; a VLA map is still in production.

WILKINSON : Just to clarify the point about whether galaxies or quasars have different structures, there is a range of radio structures visible in both optical types but the "pathological" cases where the source is really distorted are only found among the quasars.

MUTEL : It appears that the sources with a dominant double structure may be in the same class as the so-called 'compact doubles' (Phillips and Mutel, 1982, A.A., 106, 21) but with larger linear sizes (1-3 Kpc) and lower peak frequencies. There appears to be much correlation between the wavelength of maximum flux density and linear size. Also,

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all of the doubles reported in your paper and most of the previously known VLBI doubles are galaxies (or EF).

FANTI : Resolution effects can be very important. 3C138 would look like a close double with Merlin.

VERON : Sometimes ago Roland et al.(1982, Astron.Astrophys.,116,60) have observed with Merlin four compact very steep spectrum sources ($\alpha > 1.3$). All of them turned out to be doubles with separations in the range of 2 - 10 arcsec with unresolved components (smaller than 0.8"). Three of these sources are as yet unidentified, the fourth is identified with a faint galaxy with red-shift about 0.5.

DISCUSSION ON THE PAPER BY VAN BREUGEL (p. 59)

SHAFFER : In most objects, the low-frequency turnovers are consistent with synchrotron self-absorption since compact components are seen with the VLBI. NGC 1068 is a rather unique in having free-free absorption as the dominant cause of the low-frequency turnover.

VAN BREUGEL : I agree that high surface brightness and spectral curvature can be explained by synchrotron self-absorption. I disagree that the free-free absorption in NGC 1068 is just a special case. There is mounting evidence that a large fraction of quasars are embedded in spiral-type galaxies and also many radio and optical properties of Seyferts and steep spectrum cores in quasars are similar. Thus steep spectrum radio cores in quasars are probably also embedded in dense environments. For typical Seyfert parameters one than expects also free-free absorption at low frequencies.

DE BRUYN : I agree that the flattening of the spectrum of NGC 1068 is due to free-free absorption. Yet in the steep spectrum cores, like 3C 147, the turnover is at the frequency where you would expect synchrotron self absorption to become important on the basis of the brightness temperature (as derived from size).

VAN BREUGEL : I believe that both synchrotron self-absorption and free-free absorption contribute to the spectral turnovers in compact radio sources (see also answer to Shaffer's question).

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DISCUSSION ON THE PAPER BY PHILLIPS (p. 63)

PORCAS : How strong is the central component of CTD 93 at 5 GHz ?

PHILLIPS : About 0.05 Jy.

BACKER : Can we clear up the rate of discovery of compact doubles in a recent survey : is it 1/10 as you report or 3/10 as reported by Mutel this morning ?

PHILLIPS : A difference in semantics. We found one clear double and 2 "candidates" that may be triples, out of ten.

DISCUSSION ON THE PAPER BY PRESTON (p. 67)

BACKER : I realize that the galactic center is not a quasar, but can you comment on the single baseline result for this object?

PRESTON : The galactic centre appears to be extended in a general east-west direction, though with only one baseline the size and shape is very model dependent.

MUTEL : Can you comment on the variability of 0438-43?

NICOLSON : 0438-43 had a strong outburst in mid 1960's. This died away smoothly over about 10 years. The 13 cm flux decreased from 8 Jy in 1970 to 4 Jy in 1980. The 13 cm flux density of 1934-63 has not varied significantly since 1967.

SHAFFER : Although 0438-43 may be the first very high redshift QSO to be mapped, other high z quasars, like OH471 and OQ172, definitely have structure detectable with VLBI.

PRESTON : VLBI maps of five very high redshift QSO's were made by Walker (unpublished). Four of the sources show little structure (including OH471). The other source (OQ172) does show some weak extended structure.

READHEAD : You have to be very careful in classifying sources as having double morphology on the basis of observation at a single frequency. Core sources often have "double" structure at one frequency. You really need to know the spectra.

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DISCUSSION ON THE PAPER BY ZAMORANI (p. 85)

SCHEUER : Have you tried correlating the X-ray luminosity of steep-spectrum radio quasars with their core radio flux only? and, if so, what is the result ?

ZAMORANI : Yes, this has been done by Tananbaum et al. (1983a), for the complete sample of 33 3CR quasars, for most of which good measurements of the core radio flux are available. While there is no evidence within the sample for a dependence of X-ray luminosity on the total radio luminosity, they find a significant correlation between X-ray luminosity and radio luminosity of the central radio component for the 3CR quasars which have a "triple" radio structure.

MILLER : In answer to Scheuer's question, we have found a strong correlation between the X-ray luminosity and the radio cores of powerful double radio galaxies. The cores of double extended quasars lie on this correlation.

BEGELMAN : I am aneasy about one aspect of the logic which leads one to reguard the X-rays as a second order consequence of the radio flux in the radio-loud sources. While the presence of strong radio emission is sufficient to guarantee a strong X-ray flux, the converse is not true. Therefore it might be sensible to explore the hypothesis that the presence of X-rays is necessary for the production of the radio emission. One possibility is that Compton heating by X-rays is necessary to reduce the free-free absorption in a thermal wind, in order for the radio to get through. Have you considered this alternative approach ?

ZAMORANI : No, I have not examined this possibility.

PANAGIA : Concerning Begelman's question, since to produce enough free-free absorption one needs an high emission measure, one should expect a clear anti-correlation between radio flux and hydrogen recombination line intensity. To my knowledge such a relationship is not found from actual observations.

REES : If you are right in conjecturing that the X-ray component correlation with the radio emission comes from a region larger than 10E19 cm, then the radio loud quasars would be less likely than the radio-quiet quasars to snow large amplitude X-ray variability on short timescales. Is there any evidence of that ?

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ZAMORANI : A systematic study of the X ray variability properties of a large number of quasars observed with the Einstein Observatory has been recently completed (Zamorani et al. 1983). On a time scale of about one day, they found four variable quasars; two of them are radio-quiet and two are radio-loud (flat spectrum). Among the 31 quasars analyzed on this time scale , 24 are radio-loud (14 with flat radio spectrum and 10 with steep radio spectrum) and 7 are radio quiet. Hence, the percentage of variable objects in the two classes of quasars would seem to suggest, if taken at its face value, a higher frequency of short timescale variability in radio quiet quasars.

COHEN : The very large dispersion in surface brightness seen by VLBI is mainly due to the fact that most components are optically thin. In the few cases where there are reasonable diameter measurements near the synchrotron maximum, the brightness temperatures are between 10E11 and 10E12 K.

WRIGHT : In any luminosity-luminosity plot it is important to consider selection effects. I would like to see the effect of randomizing the red-shifts between objects in your plots.

ZAMORANI : I have not applied the "randomization" test that you are suggesting. However, I am confident that the correlations discussed in this paper are real for two different reasons. First, correlations are present in all our samples even if we use the observed flux densities, instead of the luminosities; second, you would be right in worrying about the reality of a luminosity-luminosity correlation if the objects were selected independently from flux limited surveys at two different frequencies. In fact, in this case there would be no information at all for those objects which lie beyond the detection limit in one of the bands, but not in the other. But this is not our case : the objects were pre-selected on the basis of their radio and/or optical properties and then observed with the Einstein Observatory at X-ray frequencies. The use, that we make, of both the X-ray detections and the X-ray upper limits assures that any correlation found in a comparison of radio and/or optical data with X-ray luminosities is real.

HUTCHINGS : There is a correction that needs to be made to L_{opt} to remove the luminosity of the QSO galaxy. This ranges from zero to more than one magnitude in the objects we have resolved. In our sample of 33 objects there is a correlation, with slope 1, between the X-ray luminosity and the red band luminosity in the QSO rest frame.

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DISCUSSION ON THE PAPER BY UNWIN (p. 105)

JAUNCEY : How sure are you that, within a given source, the components have significantly different velocities ?

UNWIN : In 3C 273 we measure the same speed to within the errors for two components (C3 and C4). The new expansion in 3C 279 is at 1/3 the rate found in 1971, but the motions are not seen in the source simultaneously.

WALKER : Components with velocities that are different by a factor of two are seen in 3C 120. These components were seen at different times. Components seen at the same time may have similar velocities. These results are presented in another paper.

REES : There is a class of models, invoking induced scattering in a foreground medium, which can generate apparent superluminal motions even in the absence of bulk motion, provided that the brightness temperature is such that kT is less than $m_e c^2$. Your peak contour levels are well above this at 5 GHz, but may be not at 10 GHz. Could the case for superluminal effects be proven on the basis only of observations where the contour levels are below $m_e c^2$? If the answer is " yes ", then the class of models based on induced scattering can be discarded.

UNWIN : In both 3C 273 and 3C 345, all the superluminal motion is measured relative to the 'core' whose brightness is well above $m_e c^2$ at both 5 and 10 GHz; however the moving components in 3C 273 have 10 GHz brightness less than $m_e c^2$. Such models cannot therefore be eliminated at present.

DISCUSSION ON THE PAPER BY MOORE (p. 109)

KELLERMANN : Could your observations be explained if the relativistic ejection occurred from an engine which itself was in motion around some other body? This would allow you to have rectilinear motion with apparently different origin, but, since the same engine is involved, there is no problem in explaining the equality of the fluxes.

MOORE : Both components must be strongly boosted. Therefore the ejection angle for the core and the western component would still have to be nearly the same, even though ejection occurred at different points in the two engine's motion. Also the core is always present and does

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not move relative to NRAO 512 (Bartel et al., this Conference), so it seems likely that the core engine is stationary.

ALLER : Could another source component have appeared between your first two epochs at 1 cm? The cm flux curves indicate the possibility of a new component appearing in late 1981.

MOORE : The trajectory has been sampled fairly frequently at 1.3 cm and 2.8 cm and it is very continuous. It is unlikely that what we observe is due to another component. There could be a new component which is still unresolved in the core.

ALLAN : As you have position differences between 1.3 and 2.8 cm observations, you must be very careful of optical depth effects in interpreting the observations. Taking optical depth effects into account, can you make a model with simple linear motion?

MOORE : Optical depth effects (in either component) may well account for the slight difference in radial separation observed at 1.3 cm and 2.8 cm. However, I doubt these effects alone could explain the non-radial motion or acceleration.

MARCAIDE : I'm surprised to note that the ratio of strengths between the core and the outgoing component does not change monotonically in time. Could you comment on that ?

MOORE : First, I would say that the second epoch map may have a 180 degree reversal as it was made with a different processor and software than the first and third epoch maps. While this would not affect our results of non-radial motion or acceleration, the change in the flux ratio would than be monotonic. Second, the absolute calibration of 1.3 cm maps is not yet completed, so it is unclear how the flux of each component has changed. Untill these questions are addressed, a definite answer is premature. It may be necessary to invoke intrinsic variability of the components.

BEGELMAN : Could you explain the variation of component strength, as well as the trajectory, in terms of a precession model ? The apparently moving material might not in fact be the same gas, but rather material that happens to be brightest at any given time. The picture I have in mind is that of a lawn sprinkler which swings into and then across our field of view. Coupled with relativistic effects, this could involve large change in both position angle and brightness of the apparently moving component.

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MOORE : A precession model in which the western component is discrete and ejected linearly from the core can be ruled out by the non radial motion. However, the model you suggest is feasible (as long as there is relativistic motion). It depends on the dynamic range of the maps and the time scale for decay of previously emitting regions. Our dynamic range is good (about 40 to 1) and the data are well fitted by only two components, so the decay time scale would have to be short.

DISCUSSION ON THE PAPER BY BARTEL (p. 113)

JOHNSTON : Could the 200 micro-arcsec scatter in your observations be due to self absorption effects in the core source? That is, the position of the core source in 3C 345 is not stable to 200 micro-arcsec with time.

BARTEL : Yes, that is a possibility.

ALLER : The epoch of the 1980 position measurement is coincident with the large outburst at cm wavelengths. The source had a substantial opaque region at 3.6 cm at that time, so that the "peak" in the intensity may appear down the jet where the optical depth is about 1. This is the same point that Johnston brought up.

MARCAIDE : Can you tell what your chi square per degree of freedom is and, if not unity, can you comment on the reason ?

BARTEL : If we consider the observed fringe phases to be independent samples with the statistical standard errors calculated from the signal-to-noise ratio only, chi square per degree of freedom is definitely not unity. The reason is the effect of systematic errors which dominate fringe phase uncertainties. We used the root-mean-square of residuals to estimate the errors of our observed fringe phases, so that the final chi square per degree of freedom of the phase differences is unity. The statistical standard errors we quote come from this least-square analysis. They do not take account of possible correlation in the errors of the observables.

VAN DER LAAN : Is NRAO 512 completely quiescent? If it is flux variable, indicative of changes in opacity structure, its centroid may move about at your remarkable levels of accuracy.

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BARTEL : NRAO 512 has an inverted and time variable spectrum between 2.3 and 8.3 GHz, so that changes of the position of the brightness center, caused by changes in the opacity, are conceivable. However, the source structure at 10 GHz appears to be nearly pointlike (FWHM less than 0.7 mas) at contour levels greater than 5 % of the peak. Hence it is questionable whether the peak moves around by as much as 200 micro arcsec.

GORENSTEIN : Regarding the 200 microarcsec error in the difference of positions, do you anticipate that you will be able to account for this error with systematic effects? Do you have any indications now that the 200 micro arcsec "jitter" between epochs might represent true center of brightness changes as suggested by opacity effects?

BARTEL : Neglected effects, such as errors in the earth orientation or unmodeled changes in the atmospheric delay, may account for position errors substantially larger than the quoted uncertainty of , e.g., 20 micro-arcsec for the July 1980 epoch. However, the position at this epoch is offset by about 200 micro-arcsec from the mean, perhaps a bit too much to be caused by neglected effects only. Changes in position of the center of brightness may well have taken place.

WILKINSON : Can you confirm that 2-frequency observations were crucial to get the accuracy which you have reported ? In other words what would have been the accuracy if you had only observed at 3.6 cm?

BARTEL : If we had only observed at 3.6 cm, the relative deduced position in right ascension of 3C 345 would have changed by about 75 micro-arcsec in July 1980, about 140 micro-arcsec in march 1981 and about 200 micro-arcsec in june 1981. Hence our ionospheric-free positions, obtained from dual frequency observations in the 1980's, are more accurate than those obtained from single-frequency observations alone, like those at 3.8 cm in the 1970's.

DISCUSSION ON THE PAPER BY MUTEL (p. 117)

DE BRUYN : Do you believe in the reality of the features to the north of the peak ?

MUTEL : Yes. It appears that the component " A ", in fact, has been expanding preferentially along the 10 degree p.a. channel, with no evidence for expansion orthogonal to the channel. The one-dimensional cuts, however, show emission to the north of component " A " at all four

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epochs, so there appears to be long-lived emission in the channel unrelated to components " A " or " B ".

DISCUSSION ON THE PAPER BY ALLER (p. 119)

MUXLOW : A comment on the identification of the core within the VLBI structure. There is a suggestion of a weak component of few mJy at 1.67 GHz, about 0.4" to the south, derived from Merlin calibration data.

DISCUSSION ON THE PAPER BY WALKER (p. 121)

ALLER : Historically there have been a range of position angles given for the VLBI structures. What are the best current estimates of the range in position angles of the different events?

WALKER : One of the earliest observations gave a rather different position angle (about 60 degrees as I recall) than all subsequent observations and may have been wrong. The current observations give position angles between about - 100 and - 110 degrees. Any fluctuations could be a result of measurement uncertainties for this low declination source. The structure from about 10 to 200 mas reported by Benson in the next paper clearly has a different position angle (- 92 degrees).

SCHEUER : Is there evidence for different apparent velocities at different times but at given position in a source ?

WALKER : The various moving components in 3C 120 have been followed through similar distances from the core although the faster, more recent components, were only seen at one or two points in the common distance range. The velocities of the faster components appear to be constant over their full range, but the constraints are not strong within the region over which the slower features were observed.

UNWIN : The early data on 3C 279 gave a much higher rate than more recent data on a different component at the same distance. The data on this source is not extensive at this time.

JAUNCEY : The 3C 120 expansion plot that you showed seems to me, as a skeptic, to be totally consistent with a constant expansion velocity fitting all events.

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WALKER : The rates of the 1972.5 - 1974.5 component are about 1.51 ± 0.13 mas/year. For the more recent components we found the following rates. Feature A : 2.57 ± 0.09 mas/year; feature B : 2.34 ± 0.2 mas/year; feature C : 1.65 ± 0.4 mas/year. The rates of features B and C will be refined by future observations. Some of these rates would have to be in error by a few sigma for them to be the same.

ALLAN : Do you see any evidence in your maps for components running into each other?

WALKER : There is no clear evidence yet for features running into each other.

DISCUSSION ON THE PAPER BY BENSON (p. 125)

SCHILIZZI : On the model proposed by de Bruyn and I for the relation of the small and large scale structure in the superluminal sources, the wiggles in the 200 mas jet in 3C 120 should be regarded as a nutation on the nutation on the long-term precession!

BRIDLE : It is encouraging to see a radio "jet" that is very long and very thin, with positive evidence for something moving along it at its base. It is meaningful to call a thing like this a " jet ". The term has been applied much to loosely in other papers here and in the VLBI literature to denote "barely resolved structure".

DISCUSSION ON THE PAPER BY BAATH (p. 127)

SAUNDERS : I note you say Cambridge was used as a VLBI station. I thought we were supposed to be too elitist to do VLBI ! Perhaps it was before my time.

BAATH : Yes, we did use Cambridge in 1978 and did found fringes even though the coherence time was short.

ECKART : Do you see any motion in Mk421 and 1749+70? Do the secondary components of these sources become stronger when the "core" does so ?

BAATH : It looks like a radio component was squeezed out through the jet in Mk421. The superluminal limit at the red-shift of Mk 421 (z =

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0.03) is 1 mas/year, so the motion is only weakly superluminal if any. There is no evidence for motion in 1749+701. The peak outside the core actually moved inward but this is probably due to the birth of a new component closer to the core. Both sources showed similar behaviour in that the core brightened between the first and the second epoch and than stayed bright during the third. During this time secondary components appeared and moved through the jet of Mk 421 and died out in 1749+701. What we observe in 1749+701 fits very well with the flux density monitoring made by Geldzahler et al. at NRL.

DISCUSSION ON THE PAPER BY MUXLOW (p. 141)

HUTCHINGS : Our work (Gower et al. 1982) of modelling sources like these suggests that there is a class of one-sided jets which cannot be fit with a simple precession model, but all of which can be fit with one in which the core angle increases with time.

BURKE : Instead of using arbitrary parameters, why not use physics like the allowed solid-body motion? Have you tried nutation as well as precession?

MUXLOW : I have not as yet tried nutation in addition to precession. This may account for the structure. There is likely to be a great deal of uncertainty, however, in the type of solid-body which may be involved.

BRIDLE : I have a general comment on precessing-jet models in which the cone angle is allowed to vary. These open up a huge parameter space in which a very wide range of structures may be matched with very little uniqueness. To make these "fits" convincing one should have either more cycles of the structure on one side of the core, or evidence of the shape of the structure on the other side - to show the expected S symmetry. I doubt very much that the present "fits" force us to believe in opening cone angles, as Hutchings suggested.

MUXLOW : I agree. Some form of additional complexity is required over and above simple precession in order to account for the jet in 3C418. What form this takes is open to question. I can, however, say that in the absence of other complications I do not think that it takes the form of a variable jet speed. I have found that allowing the jet to slow within 1 arcsec of the core degrades the quality of fit. Beyond 1 arcsec from the core some improvement results. With just 2 cycles of

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precession visible and no counterjet we are clearly not forced to believe in opening cone angles. This has however been postulated in other radio sources and seems the most reasonable suggestion at this time.

VAN BREUGEL : Is 3C418 variable? If so, this might provide constraints on the precession cone-angle.

MUXLOW : The core has a flat spectrum and is probably variable. Unfortunately, very little is known about the nature of this variability since the source lies at low galactic latitude and has thus escaped the attention of many workers.

BARTHEL : You mentioned a pressure gradient in order to get a good fit for your precessing jet with constant cone angle. From high-resolution VLA observations Barthel, Miley and Schilizzi (in prep.) argue that such pressures may be present in high-redshift quasars ($z > 1.5$). I note with interest that 3C418 is at high redshift.

DISCUSSION ON THE PAPER BY REID (p. 145)

GORENSTEIN : Do you have a limit to flux density of counter-jet?

REID : In both epoch maps any counter jet must be weaker than 0.02 of the peak intensity and less than about 0.05 of the intensity approximately 15 mas down the jet.

NEFF : Is there evidence for wiggles in the VLBI jet of M87, and if so are they in agreement with the optical wiggles reported by Nieto?

REID : The second epoch map has less pronounced wiggles than the first epoch. Higher spatial resolution may be required to conclusively establish the wiggles. Even if the wiggles in the VLBI jet are established, they would be quite different from those seen optically with less than 1/100th of the angular resolution.

DISCUSSION ON THE PAPER BY NIETO (p. 147)

VAN BREUGEL : Is there a difference in the radio-optical spectral indices of the knots and inter-knot regions?

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NIETO : No real accurate quantitative work has been done on the electronographic material, nor on the photographic ones (Nieto and Lelievre, 1982). Therefore it is difficult to say. But at first no major difference appears between the last VLA map (Biretta et al. 1983) and the restored photographic images at a 0.2" scale (Lorse and Nieto, 1983), which suggests a rather constant radio-optical spectral index for the knots throughout. The index of the inter-knot regions require an even more careful quantitative study since they are quite faint. In all cases the comparison is not easy because the radio and the optical data have different resolutions and different signal to noise ratios.

DISCUSSION ON THE PAPER BY PERLEY (p. 153)

REES : If the bends in the outer parts of the jet and counter-jet of NGC 6251 were due to "environmental" effects rather than ballistic motions, then the absence of a 4 to 1 asymmetry in scale would surely not be evidence against relativistic jet speeds.

PERLEY : My comment refers to jets in which identical knots or blobs can be identified as having been ejected at the same time and which move ballistically, or through identical media. If bends are due to environmental effects, the analysis will not apply.

DISCUSSION ON THE PAPER BY DE BRUYN (p. 165)

BRIDLE : Is it fair to compare source sizes from maps having very high dynamical range with source sizes from maps having much lower dynamical range? Perhaps, if the comparison sources were also mapped at very high dynamic range you could find faint outer extensions to them as well (this may be particularly the case if the extended structures are edge-darkened, i.e. Fanaroff/Riley class I).

DE BRUYN : The comparison of largest angular sizes of superluminal sources and the other 3CR sources can be used to infer information about their relative intrinsic sizes and "precession" angles only within the context of Doppler boosted kinematic models (see Schilizzi and de Bruyn, 1983). If the cores in the superluminal sources are intrinsically weak cores, the dynamic range in our maps does not really differ from that in the maps of other presumably unboosted 3CR sources. If the cores are unboosted, the superluminal sources are very different type of sources

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and the comparison is not meaningful.

SAUNDERS : Your deprojected superluminal sizes are so large that such giant sources, if they exist, may have been missed in surveys because : a) low luminosity giants have very low brightnesses and may anyway be resolved out; b) high luminosity giants would have their tails removed by synchrotron and inverse Compton losses, so that one would have to guess that two widely separeted hot-spots were a single source. The requirement for precession is than not that the deprojected sizes of your sources would otherwise be too large, but that so far you have not seen any superluminals with deprojected sizes of about 100 Kpc, the size that countless doubles have.

DE BRUYN : The deprojected angular sizes of most of the sources (excluding 3C 120) are not so large that such sources would have been missed or failed to be identified. Also the largest angular sizes that we measure in our and other maps of the superluminal sources really do refer to fairly sharp edges in the sources, suggesting that loss mechanisms have had little effect on the sizes of these sources.

MILLER : If the "precession" modification to the unified scheme is correct, then we expect a large fraction of the steep-spectrum extended doubles to show similar precession (typically over 30 degrees). Yet in those sources the VLBI jets are well aligned with the outer lobes and hot-spots. Are there samples large enough to provide a definite statement of this inconsistency ?

DE BRUYN : I think the superluminal source sample is large enough to make our result a significant one. One has to bear in mind, however, that the superluminal sources have intrinsically relatively faint lobe emission. So when comparing with steep spectrum doubles one should select doubles of the same absolute lobe power. I am not sure whether such a comparison can already be made.

DISCUSSION ON THE PAPER BY PADRIELLI (p. 169)

READHEAD : The observations of symmetric sources are very interesting because they suggest that the basic jet is intrinsecally two-sided. Can you be confident that the strongest component is the core and not a knot embedded in a core-sided jet in which the core is invisible ?

PADRIELLI : The definition of core is not easy without high frequency

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VLBI observations, because it could be self-absorbed at 18 cm. However in the case of 0859-14 we are quite confident that the two elongated features in north and south direction correspond to a symmetric structure about the core. This is also confirmed by the VLA arcsec structure.

DISCUSSION ON THE PAPER BY DENNISON (p. 177)

JAUNCEY : Is there any sign of opacity effects below 300 MHz ?

DENNISON : That is a very important question, to which we do not have an answer at the moment. I suspect that at frequencies somewhat below 300 MHz, dominance by more extended components will dilute the effect. However, accurate flux measurements and monitoring at these frequencies are urgently needed to understand what is going on.

ANANTHAKRISHNAN : At Ooty we have observed a sample of 50 LFV sources at 327 MHz for compact components by interplanetary scintillation. We find: i) all the low frequency variable sources in our sample contain components smaller than 300 mas; ii) 80 % of compact components have sizes smaller than 30 mas; iii) for 22 of the sources in ii) for which VLBI measurements at 18 cm or high frequencies exist, the VLBI flux is less than the scintillating flux by about a factor two. Therefore it seems to me that only the compact scintillating flux could be involved in the variability phenomenon and their spectra are likely to be quite flat.

DENNISON : That is consistent with our observations and, as I pointed out in reference to Jauncey's question, at quite low frequencies (below 300 MHz) the dominant components are likely to be more extended and therefore less variable. Indeed, there is possible evidence that the variations are weaker at 318 MHz when compared with the 430 and 606 MHz.

DISCUSSION ON THE PAPER BY LEGG (p. 183)

JOHNSTON : BL Lac had a large outburst in the early 1970's. Could the decay differences in the outbursts between the 1960's and 1970's be due to multiple bursts during the large outburst in the early seventies versus single bursts for the 1960's ?

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LEGG : No. I think it is clear that there is a genuine difference in the decay rates of individual events after february 1970. Decay rates after this date are consistently a factor of two smaller than decay rates before.

DISCUSSION ON THE PAPER BY MILLER (p. 189)

BURKE : There is a strong evidence on other grounds against the gravitational lens model. Roberts, Turner, Gott and I examined 25 high luminosity quasars with the VLA and the data imply that lensing is not a frequently occurring phenomenon

MILLER : I agree. However many of the arguments against gravitational lensing are of a statistical nature, whereas these comparisons place constraints on the physical parameters which would be required for these sources.

KONIGL : Could you comment on the correlation displayed specifically by the OVVs and BL Lac objects ? Do they differ from the general compact sources that you considered ?

MILLER : I have not included BL Lac type objects, as many do not have measured red-shifts, and presumably they would not lie on the correlation of fig. 1, as they are X-ray bright but have only weak or absent emission-lines. Consequently there is no limit on the amount of beamed X-ray emission. However, I do not believe that they necessarily form a completely separated class, as the correlations between X-ray, optical, and radio continua are the same as for the compact quasars. I have not been able to investigate the OVV's, as there are few objects with measured H-beta, radio, and X-ray luminosities.

SETTI : In case of BL Lac's, how did you take into account both of the optical and X-ray variability ?

MILLER : For the known variable sources I have either taken the radio flux density measured at the closest time to the X-ray observations , or else the mean of the observed extremes. It is not too important in this case, since the correlation extends over a wide range in luminosity.

PRESTON : Luminosity - luminosity plots of data from flux-limited samples can produce apparent correlations due to selection effects. Would you comment on such selection effects in your analysis?

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MILLER : The selection effect arises in data containing a much wider range of luminosities than the flux densities. The effect is to spread the data along a line of slope unity. There are two methods of ensuring that this selection effect is not significant. The first is to check that there is also a correlation between flux densities. The second method is to observe all the sources in the sample with no a priori bias, and to include, as worst case, any non detections. Both approaches show that these correlations reflect intrinsic physical associations between the variables.

SAUNDERS : The correlation between X-ray luminosity and (clearly unbeamed) H-beta is tight, but is there in fact any other evidence that the X-ray emission is unbeamed ?

MILLER : It would always be hard to prove that there is not a component of beamed X-ray emission. However, fig. 1 shows that the total X-ray emission could not be enhanced due to any beamed component by more than a factor about 3. This would be consistent with previous findings that radio-loud quasars are, on average, about a factor 3 brighter in X-rays than radio-quiet QSO's.

DISCUSSION ON THE PAPER BY SAUNDERS (p. 193)

REES : Could there be so much dust in Cygnus A that even the near infra-red, e.g. Paschen-alfa, does not get out in the plane ?

SAUNDERS : The required extintion is about 50 magnitudes in the visual. The extintion has to occour inside the NL region because we see narrow Balmer lines (thus the well known dust lane in Cygnus is irrelevant). Putting the required amount of dust in the broad lines region and assuming a galactic gas to dust ratio, would completely wipe out the nuclear X-ray emission that is observed from NL radio galaxies. A different argument is that sources with small Pnucl/Ptot and faint broad lines, e.g. 3C381, have Eb-v less than 1 magnitude; if there is continuity of properties between NL and BL radio galaxies, it is hard to see how Cygnus, with Pnucl/Ptot only a little less than that of 3C381, could have so much more extintion.

Panagia : Your test on the broad Paschen-alfa line is based on a single object observation. Are you planning to extend your observational sample to make your conclusion more general ?

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SAUNDERS : Yes. But for this purpose, Cygnus A is a perfectly representative NL radio galaxy and there is no reason to think other NL radio galaxies would give a different result: Cygnus A will always give the clearest result because of its high radio and narrow lines fluxes.It seems clear that we cannot account for the known nuclear properties of classical double radio galaxies with a relativistic beaming model.

DISCUSSION ON THE PAPER BY ALLAN (p. 195)

HUTCHINGS : Do the statistics of observed bends tell you anything about the frequency of relativistic and non relativistic cases ?

ALLAN : The frequency of the two cases can indeed be derived from the observations, and I am doing that at present.

ROWNEY : What are the effects of change in velocity or brightness at the bend ?

ALLAN : If you believe that you know the change in intrinsic velocity or brightness at the bend, than this will simply alter the value of the dynamic range parameter D.

DISCUSSION ON THE PAPER BY SCHEUER (p. 197)

ROBERTS : Would you come to comment on the gravitational lens model for super-luminal motion, since you did'nt get a chance to say anything about it ?

SCHEUER : Magnification by gravitational lenses will occour from time to time, particularly for sources at large red-shifts, but I doubt whether there will be enough to affect any statistical test significantly.

BEGELMAN : Another problem with the tapered cone model is that if there are any inhomogenieties or time dependence at all, then unless the emissivity drops off very rapidly with angle from the jet axis (with an angular scale length less than $1/\gamma$), you are likely to see the compact jet pointing in the opposite direction from that of the large scale jet

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WEILER : As you mentioned, your "fringing beam" or "computer controled Christmas tree" has similarities to Sanders or Bachall and Milgrom models, except that yours is one sided. Their model predicted, among other things, high degree of circular polarization of opposite signs with net cancellation when the relevant parts of the source are unresolved in normal polarization observations. Would not your single sided jet then produce much higher degrees of circular polarization than are observed because it lacks the cancellation?

SCHEUER : The Bachall and Milgrom model contained a very specific radio emission mechanism, of electrons going out along magnetic field lines at small pitch angles, and that produces the large polarization. I do not postulate any particular emission mechanism, but would prefer to think of diverging streams of plasma emitting ordinary incoherent synchrotron radiation.

PREUSS : How crucial really will be the (desired) proper statistics of super-luminal motion for the survival of the relativistic beam model? Suppose a considerable number of nuclei in extended radio sources should turn on to be "superluminal", would this mean the end of the relativistic beam model ?

SCHEUER : Nearly all of the superluminal sources are the nuclei of extended radio sources and of course that does not mean the end of the relativistic beaming model, even in its simplest form. To reject that model by statistics one must measure a sample which is free from orientation bias, and that requires a lot of care.

ALLAN : You rule out the tapered cone model on the basis of the fat emission predicted. Have you calculated the predicted emission on your "computer controlled Christmas tree" model ?

SCHEUER : In the tapered cone model we observe emission from material that has always travelled at an angle of about $1/\gamma$ to the line of sight. In the "computer controlled Christmas tree" the emitting material has travelled most of the way at angles much larger than $1/\gamma$ to the line of sight; γ can be arbitrarily large and the observed jet can be arbitrarily narrow.

DISCUSSION ON THE PAPER BY REES (p. 207)

BEGELMAN : You have some serious observational constraints if you wish

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to put a screen with substantial Thomson optical depth at about 10E20 cm. In order to polarize the lines without washing them out, the temperature of this gas must be considerably less than 10E7 K. However you would then have problems with soft X-ray absorption, so you have to be very careful with your geometry.

REES : I agree that there are constraints both on the electron temperature and on the geometry (free-free absorption in the radio sets another constraint).

DE BRUYN : Does induced Thomson scattering destroy intrinsic synchrotron polarization of VLBI jets ? We do find radio polarization in the compact cores.

REES : Generally not. You would need a Thomson depth exceeding unity to destroy high polarization. This probably cannot occur on the VLBI scale, but could be important on the smaller scale of the optical continuum (especially if there is an opaque " false photosphere " of e+ e- pairs, as Guilbert, Fabian and I have recently discussed).

VAN DER LAAN : Barthel yesterday reported results of the first round in a "crucial esperiment" : large double radio galaxies with VLBI jets which were found not to exhibit superluminal motion. What is required is theoretical attempts to make relativistic beams produce sheaths of slow bulk motion and high synchrotron emissivity. Such "stationary jets" would look like bona-fide jets when opaque, but might be limb brightened when transparent. Would you comment please ?

REES : A relativistic jet has less inertia, for a given energy flux, than a slow one. It is therefore more vulnerable to entrainment and to some kind of instability. It is my impression that it would be all to easy to dissipate energy in a sheath as the jet plough into the emission-line region. The surprising thing is that it is not stopped completely.

ALLAN : Do you think that VLBI polarization measurements at different frequencies can in principle rule out the electron-positron model ?

REES : A pure e+ e- plasma obviously gives no net Faraday rotation. Indeed it was to account for the lack of Faraday depolarization (which would occur if mildly relativistic or sub-relativistic particles were present in number exceeding those of the high-gamma radiating particles) that Jones and O'Dell several years ago invoked an e+ e- plasma. On the other hand, it would be hard to find convincing an unambiguous evidence

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for Faraday rotation (i.e. against an e+ e- plasma), because any apparent wavelength-dependence of the polarization vector could be due to unresolved substructures and spectral index variations within the source.

ULRICH : There are ojects, like 3C390.3, where the lines and the continuum are similarly polarized, but there are other objects such as NGC4151, where the continuum is polarized, but the broad emission lines do not have the same angle and percentage of polarization.

REES : I agree that electron scattering is only one possible cause of linear polarization (and perhaps not the most likely one in most cases). However it is certainly possible to have optical depth of about 0.1 due to electron scattering by hot gas in the BL emitting region (though there would be excessively broad wings to the lines if the gas were too hot). The gas would then produce some optical polarization. Also, as I mentioned, the effect of induced scattering would then complicate the interpretation of variable components with KT $\gg$ 10E11 K.

DENNISON : Regarding the effects of Compton drag on an electron-positron plasma, what happens in the case in which the radiation field is highly anisotropic and, in particular, directed radially outwards?

REES : A test particle is accelerated by the Compton effect, provided that the radiation flux comes predominantly from behind when transformed to the particle rest frame. The reason the acceleration cannot achieve high γs is that the aberration and Doppler effects tend to reduce the flux of radiation coming from behind, and to enhance the drag effect of photons coming from transverse or forward direction.

DISCUSSION ON THE PAPER BY BLANDFORD (p. 215)

SCHEUER : In your introductory remark you mentioned the claim that radio emission is just the smoke and therefore radio astronomers cannot tell us basic truths about quasars. If one goes one step deeper and works out the power flowing through VLBI jets, using the classical minimum energy formula for synchrotron radiation, one finds powers similar to the X-ray and optical luminosities.

BLANDFORD : Of course, I agree. In fact, in the most powerful radio galaxies the jet power dominates the other luminosities. Nevertheless

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in the majority of active galactic nuclei radio jets probably carry no more than a small fraction of the total power and are therefore reguarded as secondary phenomena by optical and X-ray astronomers. My main point was just the truism that in our attempts to comprehend the innermost workings of a quasar, we should all use the evidence from the complete electromagnetic spectrum and not fall prey to " spectral chauvinism ".

JAUNCEY : It is important to see how the observed low frequency variability and the large γs implied, fit into these models. Do you have any suggestion ?

BLANDFORD : Lorentz factors γs of about 10 are necessary to avoid catastrophic inverse Compton losses if, as is typically the case, the variability brightness temperature is about 10E15 K. This is comparable with those suggested by observations of apparent superluminal expansions. There is a further point. When devising a model to explain low frequency variability, you must be carefull to make it radiatively efficient. Some models require unreasonably large powers.

REES : A comment on low frequency variability. The field strength near a black hole may be 10E3 gauss, implying a cyclotron frequency in the GHz band. We know that it is easier to get coherence for cyclotron than from synchrotron radiation, so conceivably detectable flux of radio emission could emerge from radii smaller than 10E15 cm, permitting very rapid variability. The main problem is , of course, how this radiation could survive synchrotron absorption and induced scattering further out from the nucleus. An evacuated channel would certainly be required .

BLANDFORD : I certainly agree. The Sun and Jupiter assure us that there are many alternative mechanisms to synchrotron radiation for producing radio outbursts.

DISCUSSION ON THE PAPER BY BEGELMAN (p. 227)

ALLAN : Do you think that we get anywhere near the high densities and temperatures relevant to your nuclear limit in real objects ?

BEGELMAN : The existence of nuclear-limited tori depends on the material in the center of the torus having a very small viscosity, corresponding to α values in the range of 10E-10 to 10E-6 for a 10E8 $M_\odot$ hole and the standard α parametrization. It is an open question

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whether α can be this small although the Sun appears to be characterised by a very small α. A second requirement for a long lived stady-state is that mass be supplied to the torus at a sufficiently high rate. If material is poured into the torus without being drained away, then the pressure and density in the center should approach the nuclear limit.

HUTCHINGS : Can you make any predictions on the regions of the $\dot{m}/M$ plane where you expect your mechanism to be observably different ? Do you have any BL region predictions ?

BEGELMAN : My hypothesis is that the BL clouds will form only when $\dot{M}$ is large compared with the Eddington value, and the viscosity is sufficiently small. If BL clouds or filaments are produced by a torus which is up against both the photospheric and nuclear burning limits, then the model "predicts" that they should always be observed at a density of about 10E10 cm^{-3}, with an ionization parameter Ξ = 10 - 30 and full width of velocity dispersion of 8000xM_8 Km/sec. At this stage I would not like to predict detailed line profiles, but it appears that the model may be able to explain the systematic line shifts (relative to the NL, and between low- and high-ionization lines) claimed by Gaskell (1982, Ap.J.,263,79). The model predicts a 'grey body' spectrum with a colour temperature of about (1 - 3) 10E5 K, consistent with Malkan and Sargent's spectral decomposition (1982, Ap.J., 254, 22). However, to explain the observed line ratios, it is probably necessary to involve an additional hard (non-thermal) component of the continuum. Such continuum may come from magnetically driven flares on the surface of the torus.

DISCUSSION ON THE PAPER BY BIRKINSHAW (p. 229)

BEGELMAN : Provided that jets are indeed highly supersonic, I am not convinced that one should be too concerned about the jet braking up when one of your instabilities becomes non linear. Supersonic flow must dissipate energy if it is forced to follow too curved a trajectory. This dissipation may prevent the instabilities from growing beyond the marginally non linear phase, without being so severe that it slows down the jet in a short distance.

BIRKINSHAW : Naturally, this calculation cannot consider large momentum losses by the beam. Rather the results describe the length-scale for production of a significant sheath. The effect of Kelvin - Helmoltz

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instabilities on the sheath is another story.

FERRARI : Shouldn't you be able to fix a lower limit to unstable wavelengths introducing gradients across your cylindrical beam ? This is in fact the result of previous linear analysis in growth rates.

BIRKINSHAW : With plausible lower limits the growth length of the instability at low n is about 100 R in the equal density case.

DISCUSSION ON THE PAPER BY FERRARI (p. 233)

ALLAN : What is the maximum velocity in your wind solution ?

FERRARI : The asymptotic velocity in the wind model depends essentially on two factors : the temperature at the basis of the flow and the momentum deposition along the flow. The highest are obtained when momentum deposition lasts very long and basis temperature is high. If one uses $L \gg L_{edd}$, collimation factors $\varepsilon > 0.8$ and $T \gtrsim 10^{9}$ K, asymptotic velocities can approach the velocity of light. Obviously these assumptions must then be justified in terms of the disk model, but for large absorption of radiation, as in Begelman's model, they are perfectably reasonable.

DISCUSSION ON THE PAPER BY SALVATI (p. 239)

COHEN : How do you explain the contraction seen in 4C39.25?

SALVATI : Contractions are a typical signature of phase effects. Should this finding be confirmed in a number of cases, phase models would become much more attractive.

ALLER : Can you account for the Doppler boosting apparently required to account for the high intensity derived for low frequency variability, or for the broad frequency spectra (radio to X-ray)?

SALVATI : Relativistic bulk motion and Doppler boosting can be added to this geometry. However, the feature which you allude to depend partly on light-travel-time effects; i.e., a rapidly propagating wave in a slowly moving emitting material does at least part of the job.

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DISCUSSION ON THE PAPER BY GORENSTEIN (p. 243)

ROBERTS : We have observed 0957+561 with the VLA at 6 cm, giving angular resolution of 0.3" and dynamic range of 1 to 1000. We find that the small source C is resolved (0.3"x0.15") so that it cannot be wholly the third quasar image. In addition, it is difficult to model the structure of C when one includes a point source of flux and position required by the interpretation of your third VLBI component G' as the third image. We suspect, as you suggested as one possibility in your paper in "Science", that G' is too close to the optical nucleus of the lens galaxy G1 for it to be the third image (it is observed to be too bright, relative to the lens models of Young et al., 1981 and Greenfield, PH.D. Thesis 1981). The intrinsic radio properties of the G radio source are within a factor of two of those of M 87, for scales between arcminute and milliarsec.

DISCUSSION ON THE PAPER BY MARCAIDE (p. 247)

ROBERTS : Could you give the separation of the S and X band brightness peaks in the A quasar in milliarcsec?

MARCAIDE : 0.7 $\pm$ 0.1 mas.

BURKE : In a formal way, you can derive an upper limit for the mass of the foreground quasar. What do you get from your present limit on a 3rd image ?

MARCAIDE : The present observational limit on the third image places a poor limit of 10E14 to 10E15 M$\odot$. The apparent lack of gravitational distortion on B places a lower limit of several 10E13 M$\odot$ as given by Dyer and Roeder(1980, Ap.J. 238, L67). I would bring down, perhaps to 10E13 M$\odot$, the above limit using their method, although I am skeptical of the basic assumption of the method.

PORCAS : Could you tell us what the story is on possible flux density variations in the two quasars ?

MARCAIDE : We do not have evidence of any total flux density variation in any of those two quasars. The structure of A may have changed from november 1979 to march 1981, but without clear associated flux density change to approximately 10 % level.

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DISCUSSION ON THE PAPER BY SUBRAMANIAN (p. 249)

PORCAS : Moore at Jodrell Bank has also done modelling of the 0957+561 system, including use of the same VLBI data. His time delay is much longer than your estimate of 1 year. How unique are these models ?

SUBRAMANIAN : The time delay that we calculate does come out to be about 1 year for a variety of models, which differ basically in where one locates the center of the cluster, provided we fit all the observed constraints. Since I have not seen Moore's calculations it would be difficult for me to say exactly why they differ.

GORENSTEIN : Refsdal has shown that the bending law of the lens determines the time delay and it is unnecessary to decompose the delay into geometrical and potential terms. If one does decompose the delay for each image into these two terms, then the sign of the terms will usually be opposite and therefore the contribution will cancell.

SUBRAMANIAN : I agree that it might be unnecessary to split the time delay into geometrical and potential terms. However our reason for doing this was to understand exactly why we were getting a time delay of about 1 year while Young et al. got a time delay of 5 - 6 years. The reason turned out to be that there was a sign mistake in Young's et al. potential time delay (they got 3 + 2 years rather than 3 - 2 years).

ROBERTS : Fluctuations in the B quasar which are due to mini-lensing by stars in the galaxy G1 can be separated from intrinsic variations in the object quasar by their wavelength independence. Our monitoring of 0957+561 does not show the same kind of variation in A and B as are seen in the optical. In addition, Young showed that the timescale for mini-lensing changes in B is very long (about 100 years) at least for the kind of stars to be considered to be important.

SUBRAMANIAN : I agree that flux variations which are due to mini-lensing can be distingushed from those due to intrinsic variation. However the situation is complicated by the fact that mini-lensing need not affect the radio emission and the optical emission in the same way, because of the differences in size of the radio and optical emitting regions. Thus you can have optical variation due to minilensing without corresponding variation in the radio. Finally the time scale for mini lensing changes depends on the mass of the lens. For a star of mass M, the time scale for variation is roughly $50x(M/M_\odot)^{0.5}$ years. So if low mass stars are present, say in the halo of the galaxy, one can have variation in flux over time scales of a few years (Gott 1981, Ap.J.

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243, 140).

DISCUSSION ON THE PAPER BY CRANE (p. 259)

BARTEL : You associated the large circular component in the central region of M81 with the NL emission region. What are your arguments against this component being a SNR?

CRANE : Component 4 coincides with the known optical NL emission region and at least 10% of the observed flux density at 6 cm is thermal. Also the suggestion of a shell-like structure is probably an artifact of fitting a continuous range of structure with two gaussian components.

DISCUSSION ON THE PAPER BY ANANTHAKRISHNAN (p. 261)

SIMON : In 1975, 3C84 was observed at 92 cm, 3-station VLBI. Model fitting to this data indicates a compact component less than 20 mas which has a more extended component (about 50 mas) to the north.

ANANTHAKRISHNAN : I think that is probably what we are seeing.

DISCUSSION ON THE PAPER BY NEFF (p. 263)

BRIDLE : It was not obvious to me which feature in your map was the core and which was the "jet". There is a real problem of whether to call all trains of knots "jets". They might be the brightest features of blobby jets (with fainter connecting emission), or they might not. I feel we should be cautious about applying the jet terminology too quickly, as it carries such strong prejudices about the underlying physics.

NEFF : I am in complete agreement with this comment. We hope to justify our presumptuousness in higher frequency, higher resolution and higher dynamic range observations. I wish to point out that for Seyfert galaxies the two contending explanations for radio emission are star-bursts and 'mini-jets'. We feel that this observation supports the latter hypothesis, and therefore we refer to it as a jet.

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WEISTROP : Are large HI envelopes observed in other Seyfert galaxies or galaxies with active nuclei?

NEFF : Hawarden et al. (A.A. 74, 230) report large HI densities envelopes around early type galaxies, two of which (NGC 1512 and NGC 5921) are not known to be active galaxies. Bergeron et al. (preprint) have detected a very large envelope around 2251-178, a low z quasar, which may be the result of an HI envelope interacting with a strong continuum source.

JAUNCEY : Mk348, with its rotation axis perpendicular to the plane of the sky, would perhaps be expected to show super-relativistic motions on the simple beaming model.

NEFF : We hope to search for bulk motions with higher resolution observations. Even if the motion of material in the jet is "slow", we can measure such motion in a reasonable time because of the galaxy's proximity.

MARCAIDE : I find risky to deduce the core nature only from its compactness without spectral information.

NEFF : I agree. We hope to determine the location of the core in this galaxy by combining our (future) 6 cm observations with those reported here at 21 cm.

DISCUSSION ON THE PAPER BY LO (p. 265)

TAYLOR : With your accretion model, don't you have some difficulty in explaining the constancy over nine years of the flux density of the compact source?

LO : The infall time scale is about 10E4 years which refers to infalling from one parsec to near the center (0.1pc). What happens close in is not clear. To fall into a black-hole, the matter would have to lose its angular momentum and the time scale would depend on detailed physical conditions close in. The relative constancy of the compact source may just reflect the fact that there is not much material near the blak hole to be accreted. Presummambly, the matter seen to be falling now will be accreted eventually, but over a long time scale.

MUTEL : Compared with most other galactic radio cores, the Galactic

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Center source is remarkably bare (no jets or multiple components). How wide a field of view have you searched for other non-thermal components which may be associated with this source?

LO : The lower resolution maps of Sag A by Ekers et al., over at least 10'x10', do not show much bright features except Sag A East and Sag A West.

KELLERMANN : Is an elliptical brightness distribution consistent with interpreting the size at 3.8 cm as the result of scattering?

LO : It is still possible to do so; one would have to appeal to elongated structures in the scattering medium. The experts tell me that there may be evidence for this.

MARCAIDE : From the data in the Goldstone-Greenbank interferometer can you place a new limit to the detection of the very compact component once detected by Kellermann et al.? In view of your limit can you comment on the nature or existence of that component?

LO : The upper limit on the 0".001 component is less than 0.01 Jy.

WRIGHT : I'd like to comment that our galaxy is, of course, an interacting system. And most models of the Magellanic Stream imply, as a by-product, quite large amounts of gas falling directly into the Galactic Centre. This may be relevant to Galactic Centre models.

SCHILIZZI : Can you be sure that the compact radio source is at the dynamical centre of the galaxy? Could you invent a model in which the compact source is not at the dynamical centre?

LO : I am reasonably certain that the compact radio source is very close to the dynamic center of the Galaxy, if not defining it. The dynamic center is, of course, not well defined observationally. IRS16, the best candidate for the central star cluster, is not completely understood, and its absolute position is uncertain by about 1". As mentioned in my talk, the modest radio luminosity of the compact source does not preclude it as a possible stellar object. Reynolds and McKee (1980, Astrophys. J, 239, 893) suggested that it is a pulsar moving through the center. On the other hand, circumstantial evidence is overwhelming that the compact radio source is a unique object in the Galaxy.

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DISCUSSION ON THE PAPER BY BARTEL (p. 275)

ROBERTS : Could you clarify for me exactly what it is that you measure, i.e. what geometrical properties of the pulsar emission region are determined? I am thinking of resolution across the emission region versus resolution along the line of sight.

BARTEL : If the pulse structure is due to a temporal intensity modulation of the emission emanating from a single region at the pulsar, then we would not expect any significant differences between the interferometer phase-delays of the three components. However, if the pulse structure is due to emission from regions which are spatially distributed in a plane perpendicular to the line of sight - as is conceivable if scattering effects are involved or if several rotating beams of radiation are pointing towards the observer from slightly different locations - then differences between the interferometer phase delays of the components may become observable. We determined upper limits on any such phase-delay differences and hence on any position offsets.

DISCUSSION ON THE PAPER BY MUTEL (p. 277)

HUTCHINGS : One of your objects, LSI + 61 303, has a 26-day radio cycle and is a unique X-ray source. Any information on the radio structure would be of great interest.

PORCAS : How rapidly does HR 1099 vary? In particular, if it changes during the course of the VLBI observations, how do you get over the problem of interpreting the visibility function?

MUTEL : The shortest time scales we have seen are several minutes. For the VLBI data reported here, the sources had nearly constant flux levels. In any case, we measure the correlated flux one each minute for sources with sufficient signal-to-noise ratio

NEFF : Using the VLA, Brown, Broderick and Neff have detected (with eclipse data) a position offset between the two senses of circular polarization. Do you see any evidence for such a separation in your dual polarization work?

MUTEL : We have not yet observed in the dual polarization VLBI mode.

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DENNISON : Is there any possibility that the observed gaussian brightness distribution is caused by interstellar scattering, and have interstellar scintillation been searched for in this object?

MUTEL : Interstellar scattering and scintillation effects are very unlikely because the stars are, in general, less than a few hundred parsecs away.

DISCUSSION ON THE PAPER BY GELDZAHLER (p. 283)

KONIGL : Could you comment on the interpretation of the SCO X-1 lobes in terms of magnetic pinches, as proposed by Achterberg, Blandford, and Goldreich?

GELDZAHLER : This is an interesting idea. The authors make two predictions: 1) that the shape of the lobes should be conical with the apex pointed away from the central component, and 2) deals with Faraday roation in the lobes. Our present maps really aren't sufficient to confirm or deny (1), but our improving 2 cm VLA observations may be able to do so, at least for the NE lobe. We have data in hand to check the Faraday rotation and are currently doing so.

RUSK : Has there been a search for circumstellar material around Sco X-1 which the "beam" might be impinging upon? Old outbursts may have increased the density of the interstellar medium about Sco X-1 so that it may not be the "typical" $n = 0.1 - 1\ cm^{-3}$.

GELDZAHLER : Yes, there has been such a search. For example, the 408 MHz Jodrell Bank - MPI survey of Haslam et al. reveals no evidence for any low level shell structures near the outer lobes. Also, deep optical plates taken by Dave Malin at the AAT show no such structures.

BRIDLE : Lower-resolution VLA observations of Sco X-1, particularly at 20 cm, could be valuable as they might detect diffuse emission connecting the "lobes" to the central source. This could confirm the physical association of the three emitting regions.

GELDZAHLER : Hjellming and Wade have already made such observations with the C-array of the VLA and no such connecting emission was found.

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DISCUSSION ON THE PAPER BY NICOLSON (p. 285)

ALLER : Did the triple flares exhibit a characteristic modulation frequency?

NICOLSON : The triple flares appear to be a superposition of consecutive outbursts. This is supported by 2 cm measurements at Parkes which show distinct outbursts.

BORIAKOFF : Do you have any polarization information on the flares?

NICOLSON : Measurements at Parkes for one flare at 6 cm failed to detect polarization.

HUTCHINGS : A comparison of the radio properties of Circinus X-1 and LSI+61303 (0236+61) may be interesting.

NICOLSON : A comparison would require data at optical, infrared and X-ray wavelengths as well as radio. Another similar objects may be AO538-66 in the LMC. We have some limited data for this object. The upper limit for radio flares is 50-100 mJy at 6 cm.

WEILER : Do your VLBI observations tell you anything about the size of the radio outbursting region as opposed to the size of the binary ystem?

NICOLSON : At a distance of 10 Kpc the linear size is greater than 25 AU whereas the binary dimension is about 1 AU. However the longest baseline data is probably limited by interstellar scattering.

DISCUSSION ON THE PAPER BY WRIGHT (p. 287)

HUTEL : How many stars are known like HI-36 which have steeper than expected optically thick radio spectra?

WRIGHT : About 10.

KONIGL : 1)What is the spectral index in the steep portion of your spectrum? 2) Is it possible to invoke acceleration at large radii due to the formation of grains in the flow?

WRIGHT : 1)The spectral index varies in different objects from about +0.8 up to about +1.5 in Vy 2-2. 2) Yes it's possible: see Marsh

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(1976) Astrophys. J., 201, 190.

DISCUSSION ON THE PAPER BY SCHILIZZI (p. 289)

PORCAS : Is it true that, in addition to an arbitary registration on the sky, there is also a 180 deg orientation ambiguity in your December 1982 sequence of maps?

SCHILIZZI : There were only 3 hours of usable closure phase data on one epoch (JD2445310). This constrained the model to the orientation shown. We assumed the same orientation for the following epochs.

HUTCHINGS : In addition to the direction jitter seen here, there is jitter in the moving line radial velocity. If, as seems likely, they are connected, it should be possible to determine the place of origin of the optical lines and a 3 dimensional picture of the jitter.

SCHILIZZI : We have not yet checked the detailed correspondence of jitter in the radio position angles and that in the optical radial velocities.

JOHNSTON : A comment and then a question. Hjellming and I have been observing SS433 at 6 and 2 cm for over three years now. At 6 cm we can see radio radiation out to a distance of three turns in the corkscrew. Over this range, we do not see any deviation from the predicted positions of the observed values. At 2 cm we also see at least 2 turns of the corkscrew and also see no deviation. My question is how often do you see emission that is in a different position from the prediction?

SCHILIZZI : Only on 9 December 1981 do we see emission in a very different position angle (ΔPA about 20 deg.) to that predicted by the kinematic model. At other epochs we see deviations of a few degrees only.

SPENCER : Similarly there is only one outburst seen on Merlin maps (occuring in April/May 1982) which is in an anomalous position angle.

DISCUSSION ON THE PAPER BY SPENCER (p. 297)

BEGELMAN : To avoid kinetic energy fluxes in excess of 10E42 - 10E43

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erg/sec it is necessary to assume that the line-emitting gas has a very small filling factor. It may be the interstitial gas that is responsible for the radio emission and, if this is the case, then it would not be too surprising if flares at the central engine could sometimes eject this gas at a higher velocity than the line emitting gas. Therefore, the anomalous ejection may not show up in optical observations. Could you put a lower limit on the amount of energy contained in relativistic particles plus magnetic fields in one of these outbursts?

SPENCER : The power required to replace the radio jets in a period of 164 days is about 10E39 ergs/sec, assuming equipartition between high energy electrons and magnetic field in the jets. This is comparable with some of the lower estimates of the kinetic energy flux in the optical clouds, suggesting that the kinetic energy fluxes are indeed higher.

DISCUSSION ON THE PAPER BY KONIGL (p. 299)

MORIMOTO : Molecular line observation of IR sources in the bipolar flow molecular clouds showed existence of rotating disks of dense molecular gas of several thousand AU in diameter.

VAN BREUGEL : Regarding the similarities between galactic bipolar-flows (i.e. Herbig-Haro objects) and extra-galactic jets I must mention that, while Herbig-Haro objects are probably shock excited, the only unambiguous result from optical line emission associated with radio jets (Coma A=3C 277.3) shows that there is photo-ionization (presumably by non-thermal emission from shocks in the jet).

KONIGL : I agree with you that unambiguous data on shock excitation in extragalactic jet sources is still lacking, although the work of Brodie et al. (1983), referenced in my talk, suggest that the spectroscopic properties of the radio and line-emitting knots in CenA are similar to those of Herbig-Haro objects. The analogy between these objects and optical emission clumps in extra galactic jets could, however, hold even if the latter are not directly associated with shocked material. In this connection I would like to mention recent photo-inization models (e.g., Ferland and Netzer, 1983, Astrophys.J.264, 105) which showed that the emission in a partially ionized medium with a low ionization

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parameter could be very similar to the emission from a shock. The photo-ionization source could be the low shock attached to an accelerating clump in the jet. Such shocks are invoked also in accounting for the continuum emission from Herbig-Haro objects (e.g., Schwartz, 1981, Astrophys.J., 243, 197).

LO : What is the source of energy for the jet? And is there a problem with accounting for the energy and momentum of the outflowing molecular gas?

KONIGL : The source of the energy is most likely the gravitational binding or rotational energy of the central star, or even energy liberated in accretion onto the star. As I mentioned, there is now direct evidence for outflows from the immediate vicinity of some embedded sources, but the exact mechanism which transfers the energy and momentum to the wind is not yet clear. One of the difficulties is that very large momentum discharge rates are required to power the outer lobes - much larger than available from radiatively driven optically thin winds accelerated by the observed photon fluxes from these sources, and in fact also larger than those inferred from the direct measurements in the immediate vicinity of the sources. I would like to emphasize, however, that the jet formation and collimation mechanisms that I mentioned do not depend on the precise nature of the "central engine" which powers the wind, but only on the presence of a wind and of a flattened ambient mass distribution.

DISCUSSION ON THE PAPER BY CORDES (p. 303)

SHAFFER : The sources NRAO150 ($|b|=2$ degrees) is probably affected by interstellar scattering at 18 and 13 cm, in a non-symmetric fashion.

CORDES : The way to test whether ellipticities in visibility functions are due to interstellar scattering is to check that the axial ratio is independent of wavelength. Ellipticities would be caused by non spherically symmetric irregularities in electron density if there is some preferred alignment of irregularities somewhere along the line of sight. I recall that interplanetary scintillations indicate that asymmetric blobs exist in the solar wind plasma with alignment in the direction of the local wind velocity.

SIMON : What is the minimum scattering size expected at 329 MHz? For 3C147 an upper limit to the size of the core of 8 mas is observed.

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CORDES : The pulsar observations suggest that θ_{min} is about 7 mas at 327 MHz for b=90 deg but the error on this prediction is at least a factor of two. I would not be surprised if there are holes in the large scale height component of the scattering medium but to date there are no measurements that show such holes.

KONIGL : Could you comment on the possible physical nature of the two components that you used in your model-fitting?

CORDES : I suspect that there are several kinds of regions where electron density "turbulence" is produced. The Crab and Vela pulsars both show enhanced scattering, presumambly related to their associated supernova remnants. The low-scale height component has a filling factor compatible with an extreme population i class of objects, such as HII regions or stellar winds associated with O and B stars. Gesansky (1980, An. Rev. Astron. Astrophys., 18, 289) has argued that the maintenance of the turbulence may be important for the energy balance of the interstellar medium. The large-scale height component might be maintained by supernovae or infalling material.

ANANTHAKRISHNAN : Pramesh and I had presented evidence, at the 1982 IAU General Assembly in Patras, that between latitudes 0° and 4° and longitudes 0° and 40° there is a total absence of IPS indicating substantial enhancement in scattering towards the galactic centre. In the same paper we had also stated that such enhancement is not seen in the anticentre direction. Therefore it is clear that Simon's observation of 3C147 should not be seriously affected by interstellar scattering. Thus scattering is not only latitude dependent but also longitude dependent; i.e., one must look at it in terms of galactocentric radius.

DISCUSSION ON THE PAPER BY R. NORRIS (p. 323)

REID : It is very unlikely that circumstellar OH masers are saturated. The brightness temperature needed to saturate the 1612 MHz transition is T_{sat} = 10E11 (0.01/Ω) K where Ω is the beam solid angle of the emission. This temperature is several orders of magnitude greater than typical brightness temperatures. Also, the extremely bright features you observe in the blue shifted OH peak cannot be saturated if they amplify the stellar continuum, since the beam pattern would be the solid angle of the star at the distance of the OH maser which is very small (e.g., Ω less than 10E-8).

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NORRIS : In reply to your first point, I would say that T_{sat} is rather uncertain because it depends on a poorly known quantity, the thermalisation rate. Empirically, the smoothness and linearity of the OH intensity variations in response to the varying IR flux suggest that the masers are saturated. Regarding your second point, I don't agree that this is the correct solid angle to use. The central star may be very small, but the appropriate beaming solid angle is determined by the geometry of the shell and observer. I think you have to be rather careful, as the results of my model are rather non-intuitives, and really require careful calculations, which are in progress.

JOHNSTON : Bowers, Spencer and I (this volume) have mapped about twenty late type stars in 1612 OH using the VLA. The spatial structure of this emission is consistent with central outflows in almost all cases although there are probably small deviations from this simple model in all cases. For the supergiant stars and some others such as IRC 10420 more complex models are needed to fit the data.

NORRIS : Yes, we find the same result. Most sources are generally consistent with a simple expanding shell model, although small perturbations from this model are nearly always required. Sources such as IRC10420 and the supergiants represent a small class of interestig exceptions.

MUTEL : The source OH17.7-2.0 has been mapped previously by VLBI. Would you comment on why the continuum source has not been previously seen?

NORRIS : Two possible reasons suggest themselves. One, which is more exciting, is that we are witnessing a short lived transient event. We shall, of course, be closely monitoring the source to investigate this possibility. The other possible reason, which is more mundane, is that only the channels containing the strong maser lines were mapped. Furthermore, it should be emphasized that since the broadband source is only a fraction of a Jy in intensity, the previous observations may not have had sufficient sensitivity.

BOOTH : The fact that OH17.7 was not detected in a previous VLBI experiment suggests that we are indeed observing a transient phenomenon. If the high internal motions suggested by the new spectrum are confirmed and if OH17.7 corresponds to an M-supergiant it is conceivable that we are beginning to witness a pre-supernova build up in the star.

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DISCUSSION ON THE PAPER BY LANE (p. 329)

NORRIS : Do you have any information on the structure of the SiO masers that are resolved out by your measurements? Could you perhaps use the Hat Creek interferometer for this as your measurements on VX Sgr show the resolution effects to be the same in the two SiO transitions?

LANE : Roughly half of the observed total-power flux in both lines at 43 GHz is resolved out with the 75 Km baseline. Presumably this flux originates either from extended halo components surrounding the hot spots detected by the interferometer or from an ensemble of many weak maser components, small in size, but spread over an area larger than the fringe spacing. Clearly, shorter baselines, preferably with aperture synthesis capabilities, are needed to locate this emission. An effort to map stellar sources in the V=1, J=2-1 SiO line at 86 GHz with the Hat Creek interferometer would be very useful.

BOOTH : Do your observations suggest that the SiO masers may be at the same radial distance as the H_2O masers and what are the implications for pump process?

LANE : VLBI measurements toward several Mira's and semi-regular variables suggest the H_2O masers occur at somewhat greater radial distances than the SiO masers, as is expected from the lower excitation requirements of H_2O. Toward VX Sgr, for example, the radius of the H_2O maser shell is about 2 x 10E15 cm, compared to 8 x 10E14 cm for the SiO J = 1-0 masers. The implications for SiO pump processes are not easy to specify since pump models must also take account of the fact that maser lines from higher J may be formed in different regions (as comparison of J=1-0 and J=2-1 profiles suggests).

DISCUSSION ON THE PAPER BY SCHNEPS (p. 335)

GENZEL : Can you say anything about the distance to the galactic center?

SCHNEPS : We just processed a portion of the SGR B2 data for two epochs almost a year apart. This source is too variable to permit a quick distance determination based on so little data: misidentification of features is a problem with only two epochs processed. We can already see from the data that the source must be at least 5 Kpc distant. A better number will have to wait for further processing.

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DIAMOND : How can you be sure that the observed proper motions are indeed motions and not just a misidentification of various features due to their intrinsic variability?

SCHNEPS : This was demonstrated well by the Orion KL data (Genzel et al. 1981a). Observations over five epochs showed the maser spots to move linearly with time, which demonstrate that the kinematic motions are being observed, rather than a "Christmas Tree" effect. Misidentification of features is a serious problem if only 2 epochs are observed. However the new experiment observes five epochs over 2 years, enabling us to follow variable features over several epochs.

DE BRUYN : How certain are you that the masers are distributed spherically, which is a necessary condition to derive distances from a comparison of radial velocity and proper motion distributions?

SCHNEPS : Departures from isotropy can be measured to a large extent since the distribution of masers on the plane of the sky, as well as the distribution of motions in three dimensions, are observed. Only the spatial coordinate along the line of sight is indeterminate. Allowance can generally be made for observed asymmetries by modelling the errors in distribution, as was done for W51 by Genzel et al. (1981).

MARCAIDE : The very accurate distance to Orion was determined using an isotropic flow model. How does Erickson et al. finding of the bipolar flow in that source (1982, Ap.J., 261, L103) affect the determination?

SCHNEPS : Were the masers aligned in a highly collimated jet an ambiguity could arise, but we know that this is not the case. The distribution of masers on the sky is not simply jet-like. In such a poorly collimated flow no errors are introduced by assuming that the masers are irregularly distributed on an expanding shell, as opposed to an expanding cone.

DISCUSSION ON THE PAPER BY JOHNSTON (p. 339)

CAPORALI : You mentioned that in giving astrometric coordinates of radio sources one should specify the frequency of observation, because the source structure may change with frequency. Is there any indication that not only structure, but also the position of the center of emission changes with the observing frequency?

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JOHNSTON : Yes, the apparent position of many quasars may change by up to 2 mas or more in measuring the peak emission over frequencies from 5 to 22 GHz because the majority of the cores of the sources are self absorbed.

COHEN : The "core" in 3C273 is strongly self absorbed at 2.3 GHz and the centroid has a one or two mas difference in right ascention between 2 and 5 GHz. It would be better to us a BL Lac for a right ascension reference source.

JOHNSTON : I agree with your suggestion. My basic point was that only one source located near the equator should be used as the right ascension zero point.

DISCUSSION ON THE PAPER BY TAYLOR (p. 347)

SHAFFER : What are the ambiguity spacings on the PSR0950+08 proper motion/parallax determination and what effects might they have on the results?

TAYLOR : Between 15 and 20 mas. Incorrect identification of fringe numberings is unlikely in our data for PSR 0950+08.

DISCUSSION ON THE PAPER BY BAUDRY (p. 355)

JAUNCEY : Because of the problems with galactic radio sources, proper motions, structures, position co-incidence etc., it is important to compare directly the Hipparcos and EGRF. Just what is the magnitude limit of Hipparcos, since there are several southern BL Lacs within one magnitude of 3C273?

BAUDRY : The nominal Hipparcos precision, about 2 mas, is expected for objects brighter than about 9-10 magnitude. The magnitude limit is somewhere between 12 and 13 magnitude. Thus 3C273 should be accessible to Hipparcos.

JOHNSTON : The relative positions of maser sources to the optical may be difficult. VLA observations at H_2O masers show emission spread over 0.2" to 0.3". The location of stellar optical emission to this maser emission to a relative accuracy less than 0.01" will require that the

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structure of the H_2O emission be modeled in some detail.

NORRIS : Regarding the positional coincidence of the masers and stars, the model I discussed in my talk may well apply to SiO and H_2O masers. If so, then when observing with the longest baselines all emission may be resolved except that which represents the amplified stellar thermal emission, which is of course coincident with the star. We find, with VLBI observations of OH masers, that this one unresolved spot, which on the shorter baselines is not significantly different from the other components, is all that is seen on the longest baselines.

BAUDRY : VLBI observations of stars in H_2O and SiO must be made in order to find the unresolved spots similar to those observed in OH. They must also be observed more or less regularly in order to find out how they behave with respect to the stellar position.

MUTEL : It is not obvious that there are any OH masers for which the optical stellar position and any individual maser feature coincide within 10 mas. Only in those stars for which a simple spherical shell geometry is applicable can one expect the line edges to be coincident with the star's line of sight, but these models are clearly not viable for supergiant stellar masers (as mapped by VLBI) and even the Mira variables show significant deviations from spherical symmetry (as mapped at the VLA). Some OH/IR stars (e.g. OH127.8 0.0) appear to have spherical symmetry, but they, of course, have not optical counterparts.

BAUDRY : Geometry in SiO stars (Miras) has to be investigated. For this we intend to use the IRAM interferometer, at 86 GHz.

DISCUSSION ON THE PAPER BY DE VEGT (p. 357)

CAMPBELL : How far did you make sure that the precession/rotation models used in establishing the radio and optical catalogues are the same? The sinusoidal signature in the residuals of your comparison reminds one of a systematic difference between these models.

DE VEGT : The influence of the model is about 10 mas.

JAUNCEY : Which optical catalogue should we now be using in the South?

DE VEGT : The final catalogue for southern hemisphere is SRS (available

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at the end of 1983, probably). An intermediate frame is provided by WL50 (Washington-Leoncito)

DISCUSSION ON THE PAPER BY MARCAIDE (p. 361)

SHAFFER : How did you place a Gpc distance limit on the quasar? Low tranverse velocities are possible for local quasar models.

MARCAIDE : I said under reasonable assumptions, i.e. that at least for one quasar its transverse velocity be a fraction of its radial velocity if the red-shift were due to local doppler-shift. Possibly a better way to use the astrometric limit would be to assume transverse speeds of about 300 Km/sec and then the distance to the A quasar would be in the Mpc range.

BARTEL : You determined the separation between the two quasars with nearly microarcsecond precision. Suppose you detect a significant difference in the separation of the two quasars of, say, 50 microarcsec in the next epoch observation. Would you be inclined to believe to have detected proper motion?

MARCAIDE : I would try to understand a bit better possible systematics. Using the maps deduced from the second epoch with the phase-delay observable from the first epoch, and vice versa, should help in the research. I encourage you to read our paper which will appear in the A.J. August '83 Issue, where we wrote several related comments.

DISCUSSION ON THE PAPER BY NIELL (p. 363)

CAPORALI : You mentioned that the nutation model used in the astrometric reduction of VLBI data is probably not enough accurate for the precision of the VLBI measurements. Have enough VLBI data been accumulated to permit that the nutation constant is included in the least squares solution?

NIELL : Our dual frequency data covers only the period 1978-1982. When we solve for a nutation-like departure from the 1984 system, any change is not significant.

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DISCUSSION ON THE PAPER BY SHAFFER (p. 365)

BARTEL : You selected sources with simple brightness distributions. Do you nevertheless make attempts to correct for structure phase?

SHAFFER : We do not normally correct for structure, but reductions which do correct for structure show that there is only a small effect much less than 1 cm, presumably because the effect of structure shows up on only a few data points out of the 100 or more data points per baseline in a given experiment.

KELLERMANN : Your results show little, if any, improvement when MKIII was used instead of MKII. Indeed, even without increased sensitivity one would expect some improvement over the past 10 years due to more refined techniques.

NIELL : The sensitivity of MKIII is required to make the mobile VLBI geodetic system possible, since the antennas are small.

KELLERMANN : Yes, but my question was directed to Shaffer and his results using large fixed antennas.

GORENSTEIN : As comment to Kellermann and Shaffer, I believe the sensitivity of the Mark III system as compared to Mark II is needed to study the systematic effects that limited geodetic accuracy.

SHAFFER : Yes, systematic errors show up quite obviously in the residuals of each baseline solution. Each source tends to show its own systematic trends. Unfortunately, we have not had time to properly analyze these residuals.

KELLERMANN : You are both avoiding an answer to my question: How has the improved sensitivity of MKIII improved the result?

DISCUSSION ON THE PAPER BY LEGG (p. 383)

SAUNDERS : This is a question directed to you and Kellermann. Why don't the U.S. and Canada cooperate fully on a VLB array? It's a waste of money to have separate arrays - shouldn't you as scientists be persuading your governments of this? After all, even the English and French manage to cooperate with each other in the European Space Agency!

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LEGG : In principle, yes, I would agree that a well-planned joint array would be superior scientifically. There seem to be difficulties, however, in getting agreement on how to build such an instrument, and in how to operate it. One difficulty is possibly the success of the U.S. effort to have their array funded. There may be an understandable reluctance to delay, or even de-rail, their project by considering collaboration.

DE BRUYN : You mentioned the possibility of improving the UV-coverage by observing over an 8% relative bandwith. Have you investigated whether large spectral index changes in the sources (known to be present) would not deteriorate rather than improve your dynamic range?

LEGG : One could make sure that the dynamic range was not deteriorated by measuring spectral effects. This could be done by covering exactly the same U-V track (in λ/d) at different wavelengths. I.e., imagine a particular track at wavelength λo produced by two telescopes. Another two telescopes, producing the next longest track at λo, could also produce the original track at wavelength $\lambda o + \Delta\lambda$. The spectral differences could therefore be measured, independently of visibility structure.

WRIGHT : Do you think that the STARLAB project will be in funding conflict with the CLBA?

LEGG : We are told that there should be no direct interference because these funds would come from different sources. At some level, however, there would presumably be only one pot of money, and possibly some conflict, though we hope that there might be enough difference in timing to avoid this.

DISCUSSION ON THE PAPER BY BHONSLE (p. 391)

SHAFFER : I think this is a good frequency and array for monitoring sources. Do you have additional plans for mapping or other uses?

BHONSLE : Yes, I do have additional plans to utilize our three IPS telescopes, which are separated by | 200 Km. First, I would like to incorporate Mark II VLBI terminal at each of the three stations and calibrate angular size measurements of radio sources made with the IPS technique. Next, I shall incorporate antenna tracking of radio sources and attempt mapping using "closure phase and amplitude" techniques.

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DISCUSSION ON THE PAPER BY BURKE (p. 397)

WEISTROP : 1) Are there plans for a VLBI experiment on the successor to VOIR (VOIR has not been funded)? 2) When do you think the first OVLBI experiment will be done?

BURKE : 1) Not likely; 2) 1990 or shortly thereafter.

DISCUSSION ON THE PAPER BY LEVY (p. 405)

SHAFFER : What baseline length (40,000 Km?) was used for the S-Band VLBI sensitivity calculations? TDRSS is at geosynchronous.

LEVY : The standard list of sources was used. By adjusting the angle of observation the baseline can be varied down to much less than an earths diameter.

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