Finally, and probably of most relevance for this meeting, what is the "best" technique of long-baseline interferometry in the visible region? I would like to suggest that there are *three* classes of long-baseline interferometers:

i) Small aperture Michelson, in which wavefront tilts are actively controlled,

ii) large aperture pupil plane (Michelson) as proposed by Dr. Currie and others,

iii) large aperture image plane (speckle) as being undertaken currently by Dr. Labeyrie and his colleagues.

Professor Hanbury Brown suggested that the main advantage of the small aperture Michelson was its *accuracy*. A large collector technique must surely give you a fainter limiting magnitude (for equal optical bandwidths) - but can it *also* give you good accuracy? The speckle decalibration results appear to suggest that this is so. So which technique is "best"?

DISCUSSION

R.Q. Twiss: There seems to be a fundamental difference of opinion as to just how important the effects of atmospheric turbulence are for very long baselines. I would say that for practical purposes at baselines of several hundreds of meters the bandwidth set by turbulence will be 2.5×10^{11} Hz. On conventional Kolmogorov theory, you expect path fluctuations of the order of 10^{-6} of the baseline, but that is undoubtedly too high, because the large scale turbulence is overestimated. There is a lot of evidence in radio astronomy which would imply that maybe you aren't as badly off as that, but you are getting very considerable differential path lengths at baselines of up to kilometers and beyond. This is a very important point.

D.L. Fried: I think you are right. The Kolmogorov theory - and the outer scale - may be misleading here. I have tried to indicate some of that in my papers. If you simply assume the Kolmogorov theory then you come up with the conclusion that you must restrict the fractional spectral bandwidth to something of the order of r_o/D. But then when you start going to large baselines, you start exceeding the outer scale. Now the outer scale is of the order of a few meters to about ten meters or so near the ground. This may apply for the first several hundred meters above the ground. That's where most of the turbulence which causes a loss in resolution is to be found. At higher altitudes you have a lot less turbulence but the outer scale gets to be in excess of a kilometer. Now a proper theory would have to take account of the fact that most of the turbulence doesn't keep growing in mean square phase difference as you increase the baseline. Some of it does and clearly the r_o/D estimate is a limit - an extreme limit. We don't know the distribution of the outer scale very well but the data I have seen suggest that the contribution to r_o from high altitudes is about 20-30%, and this implies that maybe we could increase the allowed bandwidth by a factor equal to the 3/5ths root of 20%.

D.G. Currie: If you take what limited information we have about the vertical profile and some of the hypotheses about the change of the outer scale with altitude, and perform the integrals, you get reasonable numbers for the bandwidth, similar to the ones I've talked about. Now that is based on very weak data. Quite obviously, the outer scale of turbulence has not been measured at these altitudes. However, I would suggest that turbulence alone would allow a bandwidth of around 10 nm.

There is another effect which is not a turbulence phenomenon, which I believe will be important. If you look at the interface between the upper atmosphere and space, you see "gravity waves", similar to waves on the surface of the ocean, and these gravity waves have been measured using arrays of microbarometers. They typically have wavelengths of kilometers, i.e. most of them are long compared to the baselines that we are talking about. They have periods which typically peak at around 6-7 minutes, and have a spectrum which extends from a few minutes to around 15 minutes. According to the microbarographs the spectrum falls off at both sides of these limits. The magnitude of the waves is quite variable, depending upon weather conditions. In fact for a long time our worry has been that you can partly see them: in many areas you can see a striated cloud structure. It's similar to a mountain lee wave which you can also see around Washington. These indicate a spatial variation of *something* in the air. Microbarographic data are needed to confirm this. However our feeling based on very crude data has been that for more than 75% of days you could ignore this effect, as it was small. For a small percentage of days the effect was large, so that it could be treated as a phenomenon similar to clouds. Now this is based on observations near Boston; what it would be like somewhere else is difficult to say, since we have very meager data. However, there is also some information at a ten minute period. Our equipment is designed with the idea that the resultant path differences could be tracked using a triple delay. Ten minutes is long compared to our time for getting a useful signal to noise, when we are at the centroid of the fringes. So one does have the option of tracking not only with millisecond time constants, as talked about by Hardy (see p.10-1), but also with a time constant of the order of minutes. The longer time constant should correspond to the peak in the power spectrum of the "gravity waves".

C.H. Townes: I want to summarize briefly what I have picked up from people here. I think that the question of coherence and the outer scale, etc., is very important for what can ultimately be done with an interferometer. As I understood from Dr. Currie, he has numbers of about a meter for the outer scale, while Dr. Fried says one to five or ten meters near the ground. But it's clear that we don't know very much for distances much larger than that. Another piece of information: Dr. Currie is quoting about seven visible fringes, or 3.5 μm roughly, as the total excursion for any of these distances, and the CERGA group (Dr. Koechlin) tells me that the total excursion they find at distances up to 20 meters is about ten fringes, or about 5 μm, under the worst conditions. Sutton and Storey have talked about the infrared situation where they have quantitative information on the fixed 5.5 meter baseline. Their results are not inconsistent with the numbers that have been given by others. One finds in their data also a suggestion of sidebands with periods of minutes. Now in the infrared the situation is numerically different simply because the 7 - 10 optical wavelengths become a fraction of a wavelength in the infrared. If that's really the upper limit then it's very hopeful for longer baselines in the infrared. But you certainly need measurements. You need some preliminary measurements to see how far one can go in building a much more advanced system at longer baselines, and to see how to design them.

E.P. Wallner: It would seem to me that what we're saying is that at least for the long baselines we are probably beyond the outer scale and to consider the effects on the apertures to be independent is pessimistic, but not very.

D.L. Fried: Maybe I didn't make one point clear. At high altitudes we do know what the outer scale is, and there is evidence that at least in some cases it will exceed a few kilometers. I've seen data by Ritter in which he flew a plane looking for turbulence and was integrating as necessary to get the spectrum, and he has a number of spectra in which the -8/3 power law continues out to well over a kilometer. But that is at aircraft flight altitudes.

D.G. Currie: Some information on this subject should be available over the next year. We are close to completing an instrument for a different purpose which measures the atmospheric wedges down to the tenth or hundredth of an arcsecond level, and it is to be used in the site survey for the long baseline interferometer. Its real purpose is for other types of astrometry but there hopefully will be some data relevant to interferometry.

R.Q. Twiss: I would like to remind you of the radio data. Using the one mile baseline at Cambridge there was evidence of path fluctuations during the night of the order of 400 μm. However, when you go to the Five Mile Telescope you get these very much larger delays, which as far as one can tell increase at something like a millimeter of path difference per kilometer. Since these are big things, of course they are slow so that the prospects of servoing them out are real, but all I would like to say is that you must servo them out. You've got to have a good enough signal to noise to do it. Once you are well down on the transform curve, the fringes aren't there to be servoed. You will have a problem. And that is why we want a narrow bandwidth on a big baseline instrument when looking at a partially resolved star.

D.L. Fried: Take advantage of the bandwidth of the servo! You know, if you designed the servo properly there would be no reason why it shouldn't work on very faint stars and servo on the fringes with a one minute servo time constant. That's kind of a funny servo bandwidth but 1/60 Hz bandwidths are possible and it implies that you can work with 1/60th the photons you normally would think of.

R.Q. Twiss: This is true, but of course when you are looking at interferometry on stars you are really interested when the star is resolved, not when it is not resolved! And when you are trying to pick out the limb darkening law from the fringes it is quite a different matter.

D.G. Currie: In some ways I think the radio data may be unduly pessimistic. I've looked at some from Westerbork, and the situation was that large excursions were often due to the fact that the radio astronomers could observe when a front was passing through. At a time, that is, when you never would do optical observations.

R.Q. Twiss: Yes. The figure of 400 μm was the average for good conditions. It went up to 4 mm under bad conditions. But even if you allow that much of this is due to water vapor, there is still the possibility of getting delays corresponding to hundreds of microns per kilometer of baseline.

W.J. Tango: If you go to some kind of speckle procedure in which you analyze extended images, either in pupil space or image space, won't there be problems in trying to do white light fringe tracking?

D.G. Currie: I think that envelope tracking rather than fringe tracking could be done in pupil plane interferometry.

E.P. Wallner: I'd also like to comment on the fringe tracker For the very long baseline system you must have some kind of slow fringe tracking (i.e. envelope tracking). You will then be primarily concerned with the atmospheric effects on the high frequency terms. For the high frequency terms considering the distant apertures to be independent will give an upper bound on these effects.

R. Hanbury Brown: Well, you've also got to fringe track haven't you, because Dr. Bender has got the Earth moving around so much.

E.P. Wallner: Again that's the envelope type of tracker rather than the fringe tracker. That will be slow tracking.

J.B. Breckinridge: I'd like to ask how important it is to make a measurement of the outer scale of turbulence before we invest money in a large path difference Michelson, or whether it really doesn't make any difference.

R. Hanbury Brown: I would ask you how you are going to measure the outer scale without making the Michelson first?

J.B. Breckinridge: The one thing you could do is use a shearing interferometer like the one I have described on a large telescope; you get at least five meters of path difference on the 200" Hale telescope and you would expect to see the visibility rolling off.

D.G. Currie: We've done that and we find it does not roll off. The only roll-off you see is what is attributed to guiding - as low as 5-10%. The magnitude of the guiding errors is consistent with this loss.

C.H. Townes: Well, certainly one needs a pretty good instrument to measure these long range correlations, something like an interferometer. On the other hand, one can make many compromises as to quality and sensitivity, and so on, in building an initial instrument, which could be designed with some emphasis on the basic measurements to explore what can be done. This may well have a big influence on future designs.

R. Hanbury Brown: One simplification would be to work on the brightest stars.

C.H. Townes: Right.

A.J. Greenaway: I'd like to comment on what Hanbury Brown and Chris Dainty said. The question is whether one should work in pupil space or in image space. One thing I would like to ask people to think about is that if you do the interference in image space and you have a lot of path error, this results in a loss in visibility of the fringes, which is very bad from the point of view of precision in the measurement you want to make. If you have the same lack of path length correction in pupil space the fringes you look at now move. You change the position of the zero order fringe, but it may still be well within your aperture. In other words, the visibility hasn't changed but the position has, and so one should seriously think whether one can (a) track fringes and (b) tolerate a lack of path correction using the pupil space interferometer.

C.J. Dainty: But it lowers your signal to noise ratio because you have to integrate over a bigger bandwidth. But you do get precision.

R. Hanbury Brown: I would like to point out that the signal to noise ratio question is very much complicated vis-a-vis the small Michelson by the difficulty of multiplexing spectra in a large aperture instrument. In other words, your large aperture instrument has got to be that much larger than the small Michelson interferometer because how are you going to multiplex it spectrally? It is easy to multiplex, say to put in several hundred channels, with a Michelson by dispersing and then using pairs of photodetectors or detector arrays, but you tell me how to do that in a large aperture speckle interferometer.

A.H. Greenaway: In either pupil space or image space, the idea of multiplexing is presumably to get more photons in different channels, so obviously if one can do this using white light fringes one is bolting up the number of photons and again increasing the signal to noise. There are two reasons why you want to use narrow bandwidths. The first is that the object does not look the same over the range of baselines that that range of wavelengths implies, but surely this also goes for multiplexing, except that you have the possibility of sorting the information out.

R. Hanbury Brown: Each result can be normalized by its appropriate wavelength.

A.H. Greenaway: The other reason comes back to the problems in keeping the interferometer arms the same length. And then you choose the narrow bandwidth just to help you with the actual mechanical correction. And therefore if one can find some other way around this .....

D.G. Currie: Even for a white light interferometer you still have the path difference problem which would need to be corrected by an active servo device. This is necessary for both pupil and image space interferometry.

C.J. Dainty: Is there any interest in phase retrieval? Nisensen gave a paper on this, and there were three papers in the last session but not too much discussion. Did I get the impression that it is just too pie in the sky for the long baseline people? Don't you *really* want images like the radio astronomers get?

G. Weigelt: We do want images. There are ways to retrieve phase but usually it requires at least three telescopes in order to know the phase difference between two different spatial frequencies. If you observe only one spatial frequency at a time, which is usually the case with a two telescope system, you cannot derive the phase difference between one spatial frequency and another. So you cannot retrieve the phase.

C.J. Dainty: Yes, but it does affect your basic initial designing on a North-South or East-West baseline, doesn't it?

G. Weigelt: It doesn't change things.

R.Q. Twiss: If one can use an active servo to get wide bandwidths in a Michelson, there is a chance that one could build a three aperture synthesis instrument. I've done some signal to noise calculations on this.

J. Davis: It's a case of learning to walk before learning to run.

W.J. Tango: Yes. The optics are very much more complicated.

L. Koechlin: I'd like to say that it is possible to observe three spatial frequencies at the same time with a two aperture interferometer using several different wavelengths simultaneously. If you have a two telescope interferometer and disperse the beam with a prism, you observe fringes in different wavelengths at the same time. And those fringes correspond to different spatial frequencies. Thus you can derive the phase shift between two close spatial frequencies assuming the atmosphere affects adjacent channels similarly. By integrating step by step the phase shift from zero spatial frequency to the highest spatial frequency we can slowly retrieve the phase.

C.H. Townes: Of course, in phase determination, in the infrared it is perhaps a little more akin to radio than it is to the visual case; as you get to longer wavelengths it *is* practical to correct the measured phase. Just how practical it is or what baseline or how long a wavelength in the infrared you are apt to need before this can be done well is something we don't understand very well yet, but it appears that at 10 μm one can do some substantial phase recovery.