spaced points; we don't know what the random relative rate of time arrival of light rays is so we don't even know what the fringe tracking thing has got to do, and it looks rather difficult to measure it without first building the instrument itself. The second thing of course is that we don't know how to do it technically. We don't know whether we have to track the actual fringes or whether we can track the envelope of the fringes. We don't know how large the mirror can be in order to gather the light, relative to r_o, for fringe tracking. In fact we don't know much about it yet, and that is a very important thing which has to be solved. I personally believe that if we think about two things in interferometry, accuracy and limiting magnitude, it will turn out that the precision of 1 - 2% which was talked about at this meeting will eventually be achieved, perhaps, by Michelson interferometry; but the limiting magnitude which takes you down to objects like Seyfert galaxies and things like that will be, perhaps, achieved by speckle interferometry. I think maybe that is what will happen, and therefore both fields must be pursued actively because both are important astronomically.

Now another problem in the Michelson interferometer is the problem of tilt correction. Tilt correction is a vital problem in connection with achieving the accuracy we want. We don't really know too much about it; there is a lot of mathematics. We don't really know how fast it is necessary to tilt correct yet, we don't know how accurately we can measure $|\Gamma|^2$, given tilt correction. We don't yet know whether the results that we shall get, which are going to be lower than they should be due to all these various effects, can be accurately corrected back to the value they should have. That is the important thing we have to resolve.

I think this meeting has value in bringing together people who are connected with theory and with practice, and people who are connected with speckle and with Michelson type interferometry. I hope they have exchanged ideas. I certainly have benefited by it.

C.H. Townes: The discussions about astrometry have reminded me of the period about two decades ago when the measurements of time and of length were in a rather similar state. They had been measured for years, and were well-measured with good accuracy, and there had been very little improvement over some period of time. Rather suddenly many new ideas and techniques began to be discussed. There were plenty of uncertainties as to systematic errors, and no one knew just how much could be done, or whether there could be substantial improvements. Of course, clocks were already amazingly good, with an accuracy of 10^{-8} or sometimes 10^{-9}. But we now have, two decades later, improvement of about 10^5 over those accuracies - not by a single technique but by several alternatives, including radically improved quartz crystal clocks which had been the standard before but which had seemed stuck at an accuracy of about 10^{-9}.

I suspect that stellar position measurements are also going to be subject to substantial improvement, although probably not by a similar factor of 10^5. As some of the new techniques we have discussed get tried out and improved somewhat, we'll be more demanding about finding out the nature of systematic errors, and discover ways of substantially improving precision. Several varieties of new approaches will likely produce marked improvement. Some of them may subsequently fade away, but in another one or two decades I expect the situation on precision will look quite different. And what a revolution in astronomy only one order of magnitude improvement in angular precision can make!

One of the other areas discussed in which we have already recently made real progress that has been impressive to me is the understanding of atmospheric fluctuations, particularly the coherence length. For the first time, it seems to me, there is at this meeting a substantial coming together of a number of different real measurements of coherence length and a better understanding of the general behavior of path length variations through the atmosphere. Fortunately, the measurements are consonant with an already well developed and reasonable theoretical treatment, so that certain theoretical extrapolations can now be made with confidence.

There are other aspects of our knowledge of atmospheric propagation, however, which are still rather blank. One, of course, is the outer scale of turbulence and the extent of coherence at quite long distances. As we project further our techniques to long distances, such parameters are going to be quite important. The coherence at long distances may also be very

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specific to a site, and hence troublesome to codify and systematize so that various observers at different sites can compare notes. On the shorter scale distances, clearly we now have substantial and solid information, yet there is still something of a problem in intercomparing coherence lengths.

As we measure coherence lengths, what do they mean in terms of another observer's experience? For them to mean anything, we have to specify in some detail the nature of the atmosphere at which that particular coherence length was measured. Perhaps the only reasonable gauge at this point is some measure of seeing. Unfortunately it is not easy to construct a clear-cut definition of seeing in order to calibrate reproducibly the conditions under which coherence is measured. But some definition of seeing is probably the best that can be used at present in order to intercompare coherence lengths, and certainly better than reports of a particular coherence length at a particular site without some measure of atmospheric characteristics at the time.

A second rather blank area is knowledge of time fluctuations of propagation through the atmosphere. In so far as anyone has tried to construct simple theories of time fluctuations, they don't seem to fit what little experimental information we now have.

I am impressed by how hard people are working towards getting excellent results at rather poor sites. Siting is important to any astronomer, but it is perhaps most important of all in interferometry, for measuring small sizes and precise positions. The difference in quality of interferometric results for seeing of, say, 1/2 second, and those for 3 seconds seeing is enormous. Yet many of us have to struggle with rather poor sites. Perhaps after working out techniques at convenient sites which compromise atmospheric stability, we will somehow manage to use the really best sites. We should also know more about what and where those best sites are. Perhaps our developments of various interferometers just haven't gotten far enough yet to demand the best possible site because we have been working on techniques and are for the moment willing to accept what we have.

Finally, I would like to emphasize the substantial differences that exist between the games of interferometry in the infrared and in the visible region.

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Perhaps those of us in infrared work have made a mistake in not differentiating more clearly for some of our colleagues outside of the infrared field the differences between these two as well as the similarities, which have been rather more commonly discussed. Each of these wavelength regions of course has different technical characteristics, but also substantially different astronomical goals. Professor Hanbury Brown has emphasized the great importance of precise measurements of stellar discs. In the case of infrared, particularly for those stars which have dust around them, there is no clear-cut outer surface of emission. Our information about these regions is also still very primitive, so that rather imprecise measurements can be valuable. Furthermore, there is no one size that is characteristic; there is a distribution of "stellar" radiation of varying intensity and of characteristics which vary markedly with wavelength. For example, there is a distribution of radiating dust - perhaps more than one kind of dust, each with its own distribution of gas - and at least several different molecular species. Each of these components can have its own spectrum, and hence be studied separately. Further than that, as Professor Low and his associates have discussed, these distributions are not necessarily close to being spherically symmetric. While only elliptical distributions have been modeled so far, the actual distributions may be still more complex. Thus, the intensity structure in the infrared tends to have much less symmetry than that in the visible. Rather gross information is still of importance both because of our present lack of knowledge and also because there is unlikely to be any tidy theory which will reduce the size of the radiation field to small variations on a predictable pattern. Finally, many stars which will be studied with infrared interferometry are so completely surrounded by dust that they cannot be studied in the optical region. In these cases we will be able to measure what would normally be considered the optical discs only by infrared techniques.

Infrared is of course intermediate between radio and visual wavelengths, and the field can profit from techniques in both domains. Techniques from both are of course being adopted in the infrared. However, we are perhaps still missing a vigorous application of the more recent techniques used in the visible region to take advantage of what telescopes already exist in order to measure stellar sizes. We heard this morning one nice report of work at

Tenerife which would fall in such a category. We probably haven't yet gone very far in really adopting for the infrared some of the most recent techniques of the visible region for size measurements.

J.C. Dainty: My brief is to say a few words about the *problems* in speckle interferometry, but I feel that first of all I should point out some of the *merits* of the technique. For observations on simple objects such as binary stars using single dish telescopes, there is absolutely no doubt that speckle interferometry is a worthwhile technique; I think that's amply illustrated by the work of McAlister and others, who between them have taken thousands of accurate measurements on binary stars. The second favourable comment I'd like to make about speckle interferometry is that it is inherently capable of observing very faint objects, in contrast to some other techniques; we are still waiting for technology to realize the predicted limiting magnitudes, but I'm sure that these will be attained in the next few years.

But what are the problems? I think that these can be grouped into three areas. First of all, can you accurately decalibrate the atmosphere in speckle? I've always been very sceptical that this is possible, but at this meeting we've had several contributions which appear to indicate that yes, you can accurately decalibrate the atmosphere; measurements by the Avco group, Roddier et al and Selby and Wade (in the infra-red) all support this conclusion. And perhaps Worden's cross-correlation technique can also help us obtain accurate, seeing - independent measurements. Thus speckle might give *accurate* results on *faint* objects and be superior to "small telescope" interferometry on both counts.

Phase retrieval is another problem. I suppose it's pie in the sky for long base line interferometry at the moment - we would be quite happy to have accurate measurements of $|\Gamma|^2$ - so I'll restrict my comments to single dish interferometry There are two fundamentally different interferometric techniques that are being used to obtain images: one is the pupil plane (amplitude) interferometry of Breckinridge, Currie or Roddier and the other is the image plane (speckle) interferometry as suggested by Labeyrie and modified by Lynds et al, Knox and Thompson, Nisenson et al, and others. Which is "better" - pupil plane or image plane?

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