DISCUSSION.

RÖSCH. — I just want to mention a few points after the paper by Dr. Stock and show two or three slides. The first point is about this terrific word " *seeing* ". You know that there has been a lot of discussion about the use of this word. I am definitely against " seeing ", but I must recognize that it is so widely used that it is difficult now to do without it; but perhaps we may use this word provided it is given a definite meaning. I am afraid that it has been used in so many different ways in the literature that the problem has become very confused. As an example, I just want to take the paper by Dr. Stock. He spoke quite often of " various components of the seeing ". I do not think this is a good solution. Why not use one name for each of these components ? We could use the word "seeing " to express the fact that the distribution of light in the image differs from that predicted by the theory for a given diameter of the objective (an Airy pattern with a central peak and concentric rings). This is what you call " one of the components of the seeing ". Apart from this component, there is the other one which is merely the motion of the image as a whole. As for scintillation, I think that agreement is already almost general to separate it definitely from all the other components. Thus my suggestion is to restrict " seeing " to the first component : the fact that the image does not appear as it would in the case of a perfect atmosphere. It seems to me that this would make the explanations much easier than dividing the " components of the seeing " according to their effectiveness either in small instruments or in large instruments; because for each size of instrument you know what the theoretical pattern is and the departure from it is what I propose to call " seeing ", if the use of this word is to be continued. This will be a matter for further discussion.

Now I would like to present some slides, at least to show to the meteorologists what a stellar image looks like when observed through the terrestrial atmosphere (*fig.* 8). These are photographs of a double star taken with the electron camera of Lallemand shown here with a very high magnification, the angular distance between the two components being 2.1 seconds of arc. There is a difference of brightness of one magnitude between the two components; all these exposures (duration 1/64th to 1/16th of a second with a 60 cm objective) have been taken at intervals of a few seconds. You see how much the " seeing " differs from one to the next, and you will notice that in any case the " seeing " is the same on both components; this is because the angular separation is very small so that the rays from both stars travel through the same air masses. Advantage can be taken of the fact that the shape of the image is always the same for both components, to improve the accuracy in measuring their angular distance. In one of these pictures, the central peak of the theoretical diffraction pattern is in evidence : this is the situation called " broken rings " in the Danjon scale; the first bright ring of the Airy pattern appears broken in parts. The dark ring between the central peak and the first bright ring also is well defined, and the radii of these rings have just the theoretical values corresponding to

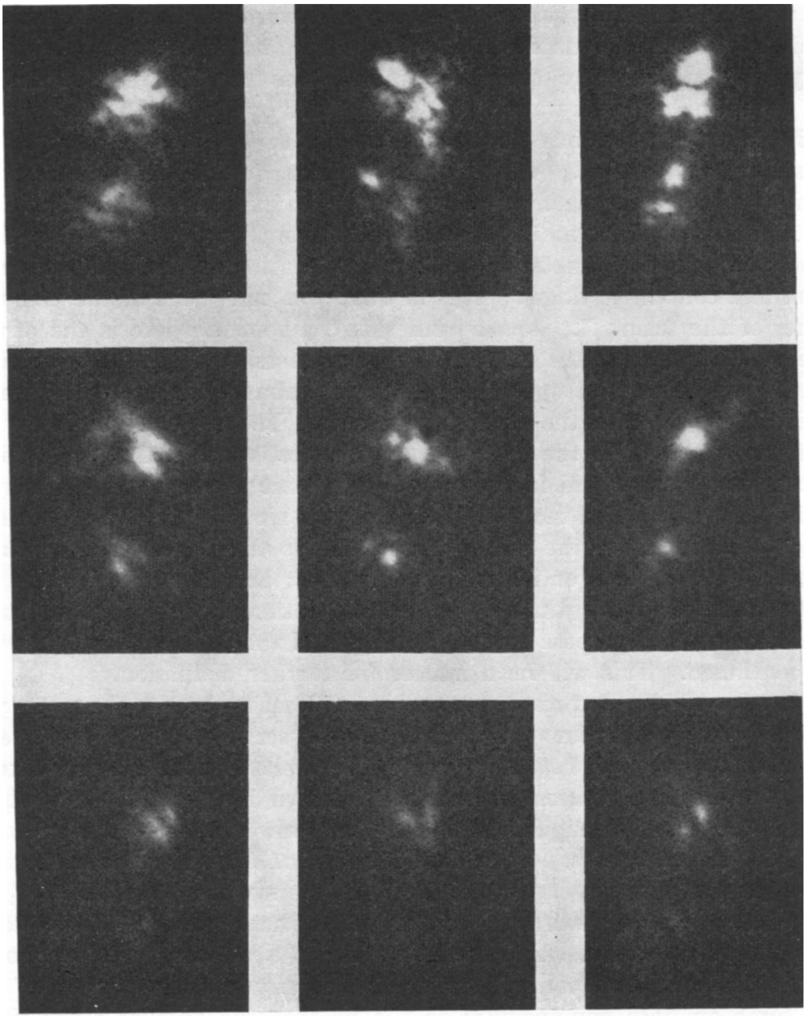

Fig. 8. — Aspects de l'image focale d'une étoile : α *Geminorum*, magnitude des composantes : 1,99 et 2,85; écart angulaire : 2″,13; temps de pose, *de haut en bas*, 1/16, 1/32 et 1/64 de seconde; caméra électronique Lallemand, Observatoire du Pic du Midi.

the diameter of the objective used (0.23″ for the first dark ring). Perhaps this is one of the sharpest stellar photographs ever obtained and I wanted to present it as an example.

Siedentopf. — I would like to add a few theoretical remarks to the results that Dr. Rösch has just shown us. At the Astronomical Institute in Tübingen, Dr. Scheffler has studied the intensity distribution in a stellar image under the influence of seeing and of the deformation of the telescopic mirror. Figure 9 shows the distribution function that has

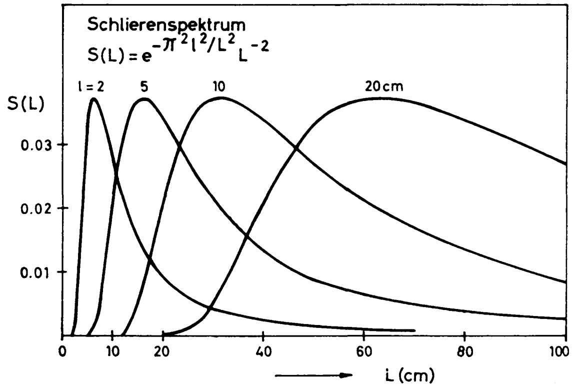

Fig. 9. — Distribution functions of the diameters L of elements disturbing the wave-front for the formation of a telescopic image.

been assumed for the diameters of the disturbing elements, either eddies in the atmosphere or deviations from the ideal mirror surface. The form of this distribution function was chosen for reasons of mathematical simplicity. Each curve is characterized by the value of the parameter l which gives nearly the diameter of the smallest elements. The elements of maximum frequency have diameter about three times larger. Empirically, the smallest turbulence elements in the atmosphere are near 5 cm diameter, so the curve used for the following calculations is that for $l = 5$ cm, where the most frequent elements are between 10 and 30 cm. The part of the curve towards the large diameters is not very important for the theory. The effect of a turbulence element is inversely proportional to the diameter if the differences of the refraction index are the same for all elements. The same holds for deformations of the optical surface, if the amplitude of the deformations are the same for all diameters.

Figure 10 shows the effect of the wavefront deformation on the intensity distribution in a telescopic image for a mirror of 100 cm diameter and $l = 5$ cm. Up to mean ray deviations of $\pm$ 1.5″ there remains a central part corresponding to the diffraction disc, the outer part of the

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intensity distribution being determined by the mean amplitude of the ray deviations, which can also be expressed by an equivalent value of the deformation of the incoming wavefront. The form of these outer parts is of course determined by the form of the distribution curve of the diameters of the turbulence elements and the amplitude of the mean temperature fluctuations corresponding to each diameter. So figure 10 only gives one example from a wide variety of possibilities.

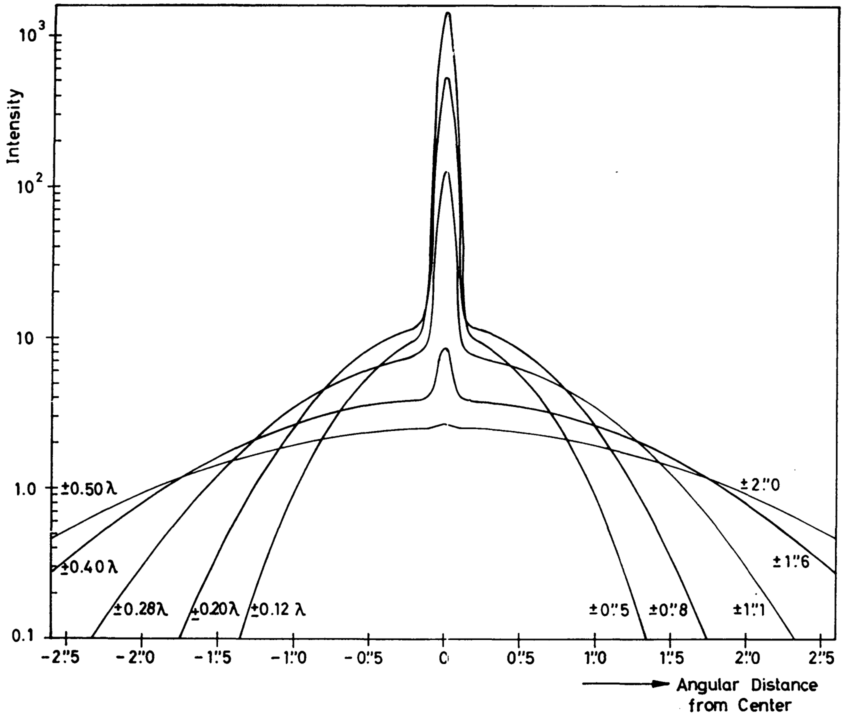

Fig. 10. — Intensity distribution in the focal image of a 100 cm parabolic mirror for $l = 5$ cm and for different mean amplitudes of the seeing in seconds of arc. On the left side of the curves are the equivalent values for the mean deformation of the wavefront in units of the wavelength. The secondary maxima in the diffraction pattern have been neglected.

If the deformation of the wavefront is caused by deformation of the mirror surface the mean value of the wavefront deformation is twice the value of the mirror deformation. This relation shows the necessity of having extremely good mirror surfaces in order to get the best possible resolution under good seeing conditions.

These calculations, the details of which can be seen in the papers of Dr. Scheffler (1962 *a* and *b*) give an explanation of the empirical facts just demonstrated by Dr. Rösch. They show that it should be possible to see the small diffraction disc inside the disturbed stellar image under certain conditions.

CIALDEA. — What is the meaning of " deformation " ?

SIEDENTOPF. — When you have a plane wavefront with parallel light rays normal to it entering a turbulent layer, this wavefront suffers deformations, and since the rays, by definition, remain perpendicular to it, they are no longer parallel. The scattering in the direction of the rays gives the mean amount of the seeing effect and this of course is connected with the wavefront deformation. The greater the deformation of the wavefront, the greater the mean amplitude of the seeing.

RÖSCH. — This is just a point I have forgotten to mention in my preceding intervention, about seeing and image motion. Let us consider a corrugated wavefront arriving on the objective, and a portion of plane averaging it over the surface of this objective. The change with time of the tilt from the normal of this average plane is in fact the image motion, whereas deviations of the actual wavefront from this plane cause the differences between the real image pattern and the theoretical pattern — let us call it " seeing " —. Scintillation, then, results from long wave-length ondulations which become important when the wavefront is considered over an area much larger than the objective.

PROTHEROE. — I would like to ask Drs. Rösch and Siedentopf if they consider the intensity to be equal across the wavefronts ? Here it seems to me that one is talking about the direction of the wavefront; but if one thinks about the scintillation, one is interested in the intensity at each point on the wavefront rather than the direction. Am I confused ?

RÖSCH. — I do not think one has to consider the difference of intensity from one point to another on the wavefront within the limits of the objective. The scintillation is the change with time of the integrated energy collected through the objective and not the difference of intensity from one point to another over the same.

PROTHEROE. — But what I am trying to say is that one has to consider not only the shape of the wavefront but the intensity of the wave at each point.

RÖSCH. — Well, this gives the details of the diffraction pattern.

HOAG. — In connection with the use of the term seeing, which Dr. Rösch has used just recently, perhaps one of the difficulties is that " quality of images " makes a very good term, I think, for the focal surface of the instrument, but when you get outside the objective, " quality of images " is not so satisfactory. This is where many astronomers like to use the word " seeing " which perhaps could be substituted by the laborious

" quality of atmosphere ". What I am suggesting is that the difficulty is resolved if you are willing to say quality of images for the focal surface but quality of atmosphere for that region above the objective.

Rösch. — The confusion arises from the use of " seeing " both for something which happens in the atmosphere and for something which happens in the focal plane.

Hoag. — What I am objecting to is the use of the term quality of images to refer to the atmosphere.

Rösch. — No, it should not be used to refer to the atmosphere.

Hoag. — No, but the word " seeing " has included....

Rösch. — ...too many things !

Hoag. — Right. Because of this very fact I do not think " quality of images " replaces the term " seeing " as that has been used.

Rösch. — " Quality of images " and " quality of atmosphere ", both are required.

Kiepenheuer. — I think at the end of this meeting we have to lay down some dogma about " quality of images ", " seeing ", and so on.

Scorer. — Could I come back to a general impression I am gathering from this? I am beginning to feel that once the best site in the world for a telescope has been discovered, you would not be anymore interested and that is where the only telescope will be. When I first came I thought the objective was to find a great many sites in different parts of the world. This is quite a different meteorological problem. Some of the problems can be eliminated immediately by going to a particular part of the world. Could we meteorologists have some guidance as to whether you want us to think on a world-wide scale, or whether the choice has to be made between two sites in Chile ?

Kiepenheuer. — I think the question is not quite as simple as you have put it. I would think that there are two different problems. We are all looking for one " ideal " site which has not yet been found and there might be only one such site. But we need many other sites which are " good " and they have to be everywhere, because astronomy has to be done not only in the States, or in Russia, or in Japan. We shall have new astronomers, and they will have to see the skies somehow at many different places. So the meteorological problem is quite manifold, I think.

Courtès. — I think the kind of question we could ask the meteorologists is, for instance, the following : we need good weather at night, but that means good weather in general, and I guess the condition for that is either wind or high pressure — but I am not a meteorologist —.

It would be interesting to know what is the general behaviour of the atmospheric turbulence in such cases.

Scorer. — Could I come back to the studies made in Chile which have been mentioned ? There has been quite a long period of observations, somewhere between one and two years. I wonder if one of the things on which meteorologists ought to be able to advise you, is whether these years are representative of a longer period. Because, for example, one of the wettest and cloudiest years that we had in Southern England was one of the sunniest in Scotland, although the distance is not very large; but this is not a normal feature of the weather at all, and it may be that the two or three years over which you have measurements are not typical of 10 or 20 years.

Stock. — Well, I can only say that actually along the coast of South America we have only two weather situations. We have either a highly stable situation which prevails practically throughout the entire summer and a long section of the winter, or, occasionally, a low pressure system that moves up the coast of Chile and lasts only a few days. Only these two situations occur and their features repeat every time, so the only difference you may get from one year to another is the frequency of bad weather periods.

Scorer. — One final question. You have some figures of the temperature range and you take this as the difference between the average maximum and the average minimum. Now it might be that at one site there is a large temperature range on some days and a small range on other days, whereas at the other site it might be very near to the average every day.

Stock. — No; this actually is not the case; the data repeat themselves very well. You may have gradual rise or fall of the temperature depending on the large scale conditions, while the diurnal range remains the same. There are actually very small deviations from the average, except when it just happens that a bad weather system is coming in or moving out, but during the stable periods which cover certainly 70 or 80 % of the year, the diurnal range is a constant feature and practically fully determined by the local topography rather than by anything else.

Meinel. — I would like to make a comment on the question of whether or not the period of test can be deemed " typical ". It is well recognized that in any semi-arid area there are very large variations in rainfall and cloudiness from year to year. This behaviour characterizes from a meteorologist's stand point a " desert region ". This could well happen, for instance, in the case of the Kitt Peak site survey. We made an effort to look at this question and see whether we had sampled typical years, by looking at the tree ring growth records that the Dendrochronology Laboratory of the University of Arizona had measured. These records did in fact show that approximately the past 50 years, including those of the survey, had been abnormally dry. In Arizona, as far as this evidence goes, we cannot say with any degree of assurance that the weather that prevailed during the site survey will be typical of the weather over the next 25 years.

HOGG. — I would like to speak to the same point, as to how one can be sure that a short period of sampling is representative of a longer period of time. We find in Australia, and I expect one does in other places too, extreme difficulty in getting night cloud observations; but on the other hand we have been able to find relatively long periods

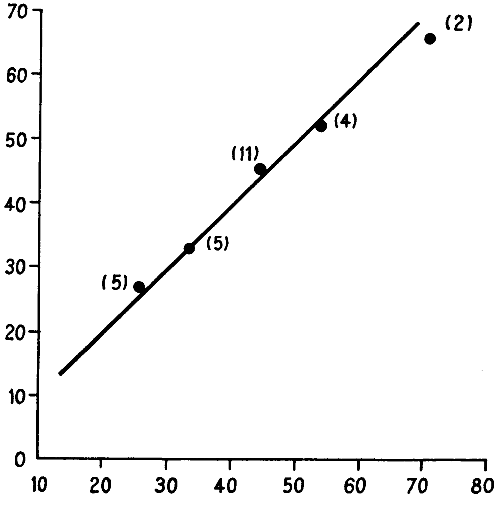

Fig. 11.
Abscissae : percentage of nights clear at 9 p. m.;
Ordinates : percentage of photoelectric observing hours.

of rainfall observations, because it is this quantity that is of interest to the agriculturists and to the pastoralists. We have therefore tried to use rainfall as an indication of cloud. The first slide (*fig.* 11) is an attempt to justify the use of cloud observations at 9 p. m. as a practical index of the astronomical usefulness of the night. The percentage of hours that a telescope at Mount Bingar observing station could be used photoelectrically is plotted vertically, i.e. if there were 10 h of darkness in the night and 5 h could be used photoelectrically, then we record 50 %. Horizontally is given the average (for monthly periods) percentage of nights clear at 9 p. m. There is a fairly strict relation between the cloudiness at this single hour and the percentage efficiency of the site.

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The next slide (*fig.* 12) shows how one might attempt to use the cloud observations over a short period of time as indicative of what might be expected over a longer period of time. The figures that are plotted are the grouped means of rain days per year against cloudiness at three o'clock in the morning, using a total of 30 years of observation. For a certain number of rain days in the year, one can predict the amount of cloudlessness at night, e.g. 80 % cloudless nights in the top lefthand corner at three o'clock in the morning is associated with approximately 25-28 rain days per year. Thus a short period of cloud observations may be distinguished as belonging to an abnormally clear or abnormally cloudy spell by reference to the long period rain day data.

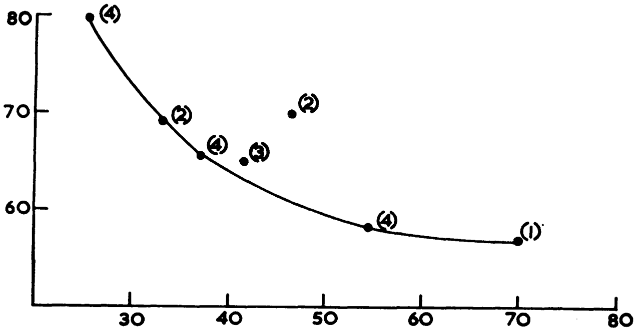

Fig. 12.
Abscissae : rain days per year;
Ordinates : percentage frequency of zero cloud at 3 a. m. (Broken Hill).

COURTÈS. — I would like to put a question to Dr. Stock : when you tried to use a diaphragm in front of the Blœmfontein Baker-Schmidt telescope, at what distance between the two holes did you obtain the non-correlation of the image motions ?

STOCK. — The linear distance necessary to produce a practically independent motion was, if I recall, about 30 or 40 cm, indicating that most of the optically effective turbulent elements were of that order or smaller. The angular diameter, I do not recall exactly but it was well within the plate which, I believe, covered 3.5 by 3.5 degrees, which then led to a distance of the order of 100, 200 and 300 m above the ground.

COURTÈS. — I think your result agrees very well with those of Danjon about the maximum distance of correlated fluctuations which he found to lie between 20 and 25 cm. You remember his system : by using a Mach interferometer he observed the fringes caused by the interference of two images of the wavefront. At the beginning, the fringes are steady; for 1 or 2 cm separation of the images, there is a motion of the fringes. For larger separations, the amplitude of this motion increases, up to 20 cm separation; then, it becomes constant. There seems to be a good agreement with your observations and I see no reason to criticize this study by interference fringes.

Stock. — I do not recall any criticism on my part of the interference fringes method. However I believe that we do have to obtain absolute assurance that turbulent elements which are optically effective are not larger than the aperture of the optics we are using, and so far we have very few examples of this kind of investigation : essentially they have been made only at two places where there is no theoretical reason whatsoever that would limit the size of the turbulent elements. We cannot conclude from two examples that they have world-wide validity.

Courtès. — Yet I think it is one of the best experiments that can be done, and one should continue with your method because it could provide the real proof that the size of the turbulent elements is always within certain limits; that is very important, because it could give the upper value of the tilts that you were talking about. By means of the parallel fringes of a Mach interferometer you can find the difference in the optical path between two separated images of the same wave; the result is about one wavelength for 20 cm separation; that means a maximum general tilt of approximately half a second of arc. Are the deflections of the stars of the same order in your observations ?

Stock. — They are. However, I recall one thing which has actually led to the design of the instrument which we are using now. Six years ago, the diffraction image seeing was being estimated at the Boyden Observatory while at the same time I was observing with the 60-inch Boyden reflector. Those who have been working at Boyden know very well the kind of seeing that you get there : good seeing until midnight, then a sharp break, and the images, from well under 1″, go up to 10 or 20 seconds of arc. We have tried to correlate the effects observed with a 60-inch with those estimated through a 10-inch aperture. The major effects observed with the 60-inch aperture were seen also with the smaller one, but the relation was not such that one could predict with safety from the small instrument what the 60-inch was observing. However, as soon as we began to observe instead image motion, we had a perfect relation, even to the smallest details, between the image motion measured in the smaller refractor and the image diameter in the 60-inch. This, interpreted in terms of the model I have expounded here, means that there were turbulent elements exceeding the size of the 10-inch telescope which contributed to the seeing of the 60-inch, and that is why I feel we are safer if we use larger apertures. Of course, we cannot very well carry a 20-inch reflector around; the 10-inch is already pretty big to handle, as you know, so we were forced to use the system we are trying now, although I agree that in many places the 10-inch aperture may be sufficient, because large turbulent elements do not occur. It appears that this is the case in Chile. However we have no direct means of predicting this, at least not so far.

Bowen. — There is some evidence regarding the size of these elements from knife-edge pictures taken by a telescope. If one puts a knife-edge at the focus, then one gets a dark patch if the deflection is in one direction

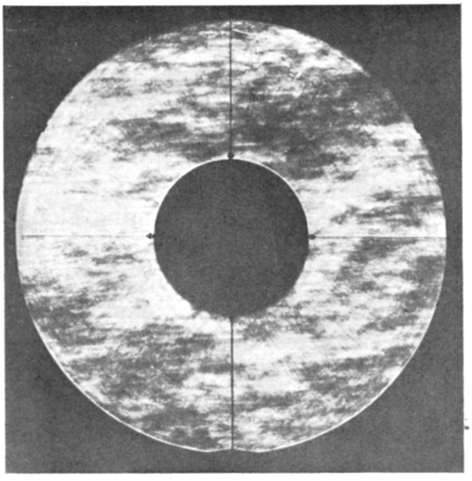

Fig. 13. — Foucault patterns, Palomar 200-inch reflector.

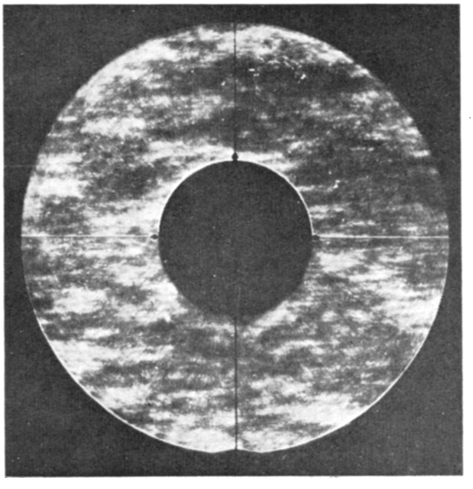

Fig. 14. — Foucault patterns, Palomar 200-inch reflector.

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as the turbulence passes over, and a light patch if it is in the other direction. Behind the knife-edge, one puts a Leica camera which is focused on the main mirror. I happen to have here a group of pictures taken in that way with exposures as short as 1/25th of a second on the 200-inch telescope (*fig.* 13 and 14). The scale is given by the diameter of the image of the mirror. These are just samples that were taken on one night. The seeing was average or a little better, probably seeing 3. If you take a long exposure, of course, then all the details are wiped out because this pattern is travelling across with the velocity of the local wind; usually it will cross the aperture in the order of a second or two. Some of the very sharp and regular details on these patterns are due to small residual defects in the polishing of the mirror.

Courtès. — If I remember some experiments that I did with the 76-inch reflector in the Haute Provence Observatory, when a strong wind is blowing you can see the focal image becoming elongated in a general direction oriented 90° from the average direction of the strips seen with Foucault test; that is a proof that the strips given by the wind behave like parallel slits in front of the telescope. May I come back to another point ? If the average diameter of the small pseudo-flat areas of the wave-front is no more than, say, 20 inches (varying from night to night), diffraction theory tells you that you cannot get better than the resolving power corresponding to an objective diameter of 20 inches. This may be the real limitation of the big telescopes, the reason why we cannot get better than 1 second of arc or a little less. Do you think so ?

Bowen. — Well, we get much better than 1″. I saw one night, with the 200-inch, images with a diameter between a third and a quarter of a second all night. I should say that it was a visual observation, during spectroscopic work using a slit which was 1/12th of a second wide and the stellar image was, I estimated, about three times the width of this slit. I have seen it only once, but what I want to point out is that it does occur.

Courtès. — That happens from time to time in our observatory too, but it is only in visual observation. On photographs we have never got less than 0.7″, for a one-hour exposure, for instance, even under the best conditions. Could I put the same question to Dr. Rösch ? The problem in astrophysics is not of the instantaneous image as you have shown, but a one-hour exposure, for instance.

Rösch. — We have no experience in using long exposures. We are people of short exposures. However, I remember a 10-seconds exposure taken by Camichel on a close double star, where the diameter of the images was less than one third of a second of arc, just the limit which could be expected with the 24-inch objective used.

HOGG. — Would not scattering of light in the emulsion contribute to the larger size of the photographic as against the visual image ?

COURTÈS. — Yes, but not much. Of course after a very long exposure the image is over-exposed, you are absolutely right, but at medium density you do not miss so much. I think the other observers agree with me.

PROTHEROE. — Dr. Bowen, one question with regard to your photographs. These look so much like shadow-band photographs taken without a knife-edge that I wonder how much you have separated out the ray deviation and the density variations.

BOWEN. — That is somewhat of a problem. I would guess that scintillation shadow-bands tend to move rather faster, due to their high elevation. I doubt if one stops the shadow-bands with the 1/25th of a second exposure.

PROTHEROE. — I have seen some shadow-band photographs taken at 1/20th, by A. Hoag using a 40-inch telescope, which show a pattern not unlike these; so that you can get some stopping. Of course the pattern becomes more elongated or streaked, as some of yours do, and I would anticipate that possibly you were getting an actual integration of that. One of your patterns, for example, had nice lines running through it; I am almost sure it is due to the fact that the pattern was moving rapidly and you actually smeared it out, as opposed to some of the others which were more blotchy in appearance and do not have these lines. I have also some measures on the scintillation pattern which indicate very large fluctuations in the density of the pattern, so I would not be surprised.

BOWEN. — I will not argue with you on that point because there may very well be something of that kind involved. On the other hand, I am quite sure that most of the effect is local, because as you watch with the eye it moves rather slowly, in the order of a second or two across a 200-inch aperture; that is, of the order of 5 to 10 miles/h which, I would guess, would be too slow for most of the high level winds. So I think the local wind plays the major part, although there may very well be some other effect in it.

COURTÈS. — If one is worried about the blending of shadow-bands with the true knife-edge pattern, it is very easy to separate the two effects, By inserting a small half-silvered mirror in front of the knife-edge you get the whole flux of light and in that case you photograph the shadow-bands alone, whereas behind the knife-edge you photograph the sum of both effects and you can note the difference.

Van Isacker. — One small remark concerning the lecture of Dr. Stock about the height of the level responsible for scintillation. There are different methods to estimate it which are coherent as long as they are not very precise. The difficult point is to know what is the thickness of this layer. It is very easy for the mathematician to assume a thin layer, and very difficult to determine its thickness by observation, because we need at least two telescopes to measure simultaneously the scintillation. There are some measurements of this type done by Barnhart, Mikesell, and Keller in 1959. I used their data to compute the thickness and I found numbers between 5 and 10 km. That is certainly not a thin layer. More than that, it seems that this result is compatible with the hypothesis of a completely homogeneous turbulent atmosphere. It is only the effect of height and pressure that gives this maximum effectiveness at about 12 km, but all the atmosphere must contribute to scintillation.