DISCUSSION.

COURTÈS. — I would like to add several details to my introductory paper.

J. Texereau has made a lot of experiments with the new 76-inch reflector of the Haute Provence Observatory, on the distribution of light intensity in stellar images. He has never had a chance to get a very good quality image, but they were of average quality. The size of the image obtained by an exposure of a few seconds or a few tenths of a second is of course always much greater than the theoretical diameter of the star, which is shown as a black dot at the bottom of each photograph presented here. In fact the image diameter measures a little less than 1 ″. One realizes, by this comparizon, the efficiency of big telescopes.

The star (*fig.* 36) is γ *Virginis* with an exposure of 1/500th of a second (2 ms). For each component the group of grains lies within a diameter of 0.95″, a little less than 1 second of arc. The next plate (*fig.* 37) shows the same star but with an exposure of 1 s. This exposures is made, of course, after reflection by a plane parallel plate, in order to reduce the intensity, since we are integrating the light during 1 s.

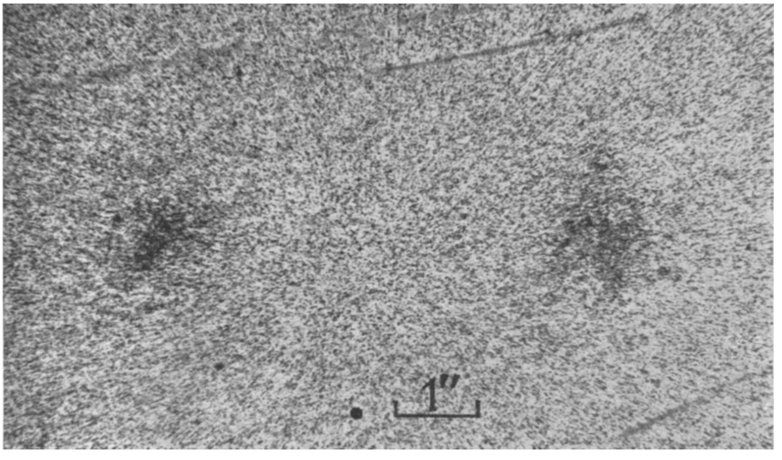

Fig. 36. — γ *Virginis;* pose : 1/500e de seconde (30 mai 1962) (cliché obtenu ainsi que les quatre suivants au télescope de 193 cm de l'Observatoire de Haute Provence par J. Texereau).

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The distribution of the granulation is, of course, much more regular than on the preceeding slide, but the average diameter is almost the same; that means that the accidental refractions exist only for a relatively short time during the exposures. The average effect of the accidental refractions would increase by only 10 % the image diameter.

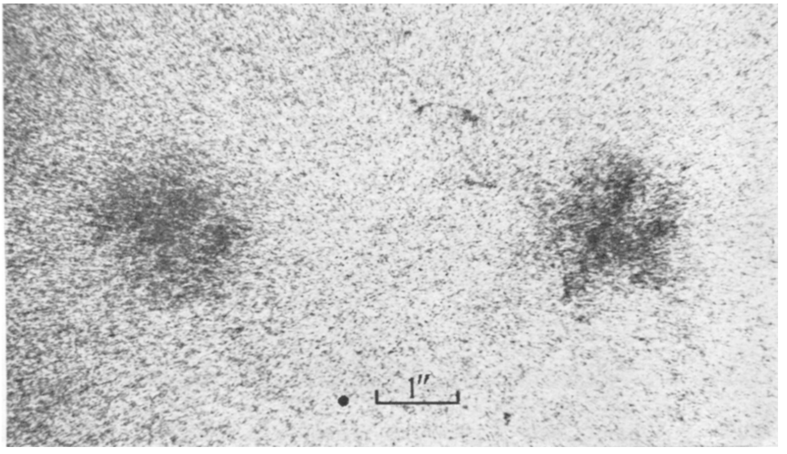

Fig. 37. — γ *Virginis;* pose : 1 s, après affaiblissement (30 mai 1962).

Lastly, we see (*fig.* 38) γ *Virginis* taken with an exposure of 1/200th of a second. The distribution of light in the two stars looks the same as in the pictures that Rösch showed yesterday. That means that

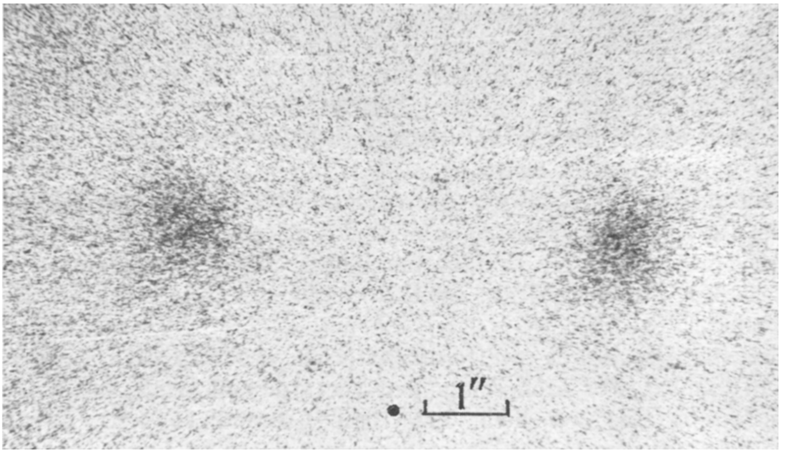

Fig. 38. — γ *Virginis;* pose : 1/200e de seconde (30 mai 1962).

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during this short exposure the two wave surfaces coming from the two components have been similarly affected. This is to be expected with a 76-inch telescope, since the two components are separated by 3″ which is equivalent to only 15 cm at 10 000 m, the assumed maximum altitude of the perturbing layer. ε *Bootis* (*fig.* 39) has been obtained with 1/100th of a second and we see some light reaching the emulsion very far away from the centre owing to some strong disturbance. It is interesting to note that some spots have a diameter near to the theoretical diffraction figure. This does not necessarily mean that the conditions of flatness

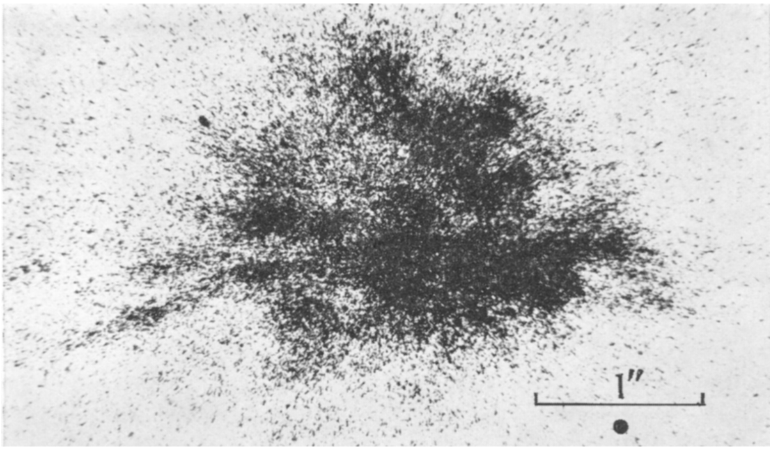

Fig. 39. — ε *Bootis;* pose : 1/100e de seconde (30 mai 1962).

of the wave surface are well satisfied for the whole aperture for some very short time intervals, since these spots could be interpreted as accidental effects of superposition of the diffraction figures given by a network of relatively small Γ elements situated in a random distribution on the wave surface.

With three circular holes the diffraction figure is very complicated; one realizes that the Γ elements can play the same role with a much more complex and unsteady figure. γ *Andromedæ* (*fig.* 40) was taken during a very strong change of image quality during an observation when the " seeing " suddenly became bad. As you see, there are very strong disturbances everywhere in the picture.

I have also some preliminary data on the profile obtained by J. Texereau from some of the pictures which you have seen. In the photographic profile (*fig.* 35), the point A corresponds to an intensity of 1/10th of the peak density; the point B to 1/100th intensity.

The parts B and C correspond of course to a very small Γ element; flat and large elements correspond to the top of this profile. The two profiles are for two different days. The scale shows that the star image diameter should be 1″ in that case, and we see that, in order to get good photometry, it would be necessary to go at least to point B, which means a hole of 6 or 4 seconds of arc. For spectrographic work, it should be 2 or 3 seconds of arc in normal conditions of image quality. At the

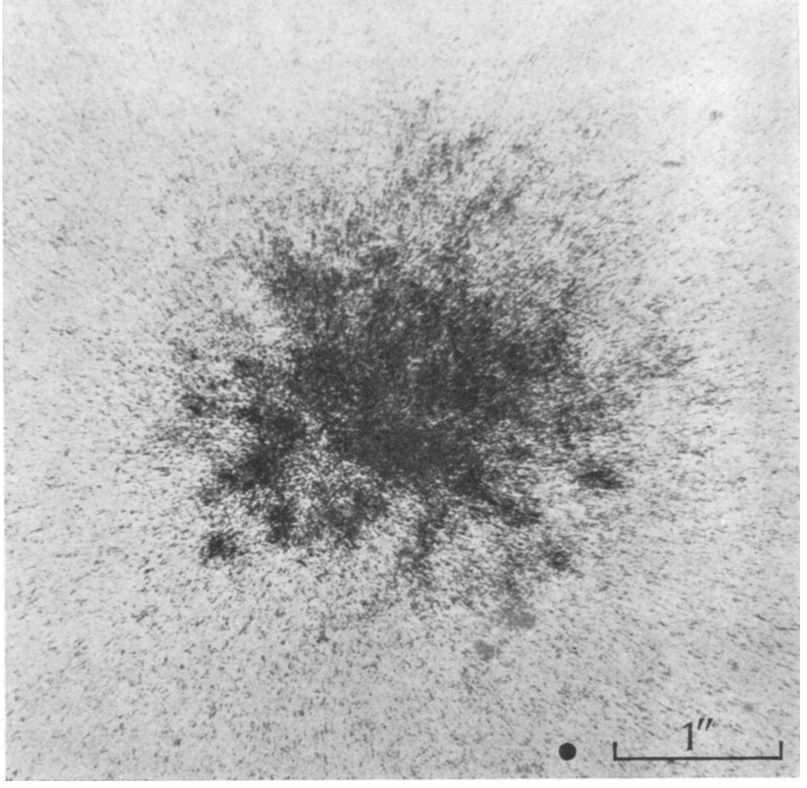

Fig. 40. — γ *Andromedæ;* pose : 1/100e de seconde (octobre 1962).

limit we saw yesterday, on the electron camera photographs presented by Rösch, an example of the best resolving power obtained so far. The first diffraction ring gives a proof that 0.2″ was reached.

May I add just a few remarks about scintillation. I think scintillation ought to be considered when trying to study the figure of the wave surface by the knife edge test. If we use the knife edge test, as Dr. Bowen showed us two days ago, it would be a good idea, as Dr. Protheroe pointed out, to obtain the effects of scintillation at the

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same time. I think it should be possible to have a camera observing the Foucault test just behind the knife edge, and a semi-aluminised glass plate in front of the knife edge, directing part of the flux, free of geometric limitation, to another camera which integrates the scintillation during the time interval of the knife edge test.

Now, concerning the deformations of the wave-front, may I suggest to the Chairman that he ask Dr. Rösch to describe his experiments using the Hartmann technique and the occultation of a star image by a knife edge ?

Rösch. — For the Hartmann-like technique, we used the 60 cm diameter, 18 m focal length, refractor of the Pic du Midi. The screen covering the objective has twelve 8-centimetre holes, four on an inner ring and eight on an outer ring. The film is out of focus. Pointing the instrument at a bright star, we get the twelve-dot patterns shown on figure 41 (*a*). Now, in front of the four central holes we place a flat mirror about 35 cm in diameter, and a point source at the focus of the instrument. The light coming from this source and falling onto the mirror through the four central holes is reflected by autocollimation and converges back onto the film. On figure 41 (*b*) the changes from one pattern to the next mean changes in the distribution of the refractive index inside the tube only. This is with the dome open. The last series (*fig.* 41, *c*) corresponds to the same experiment but

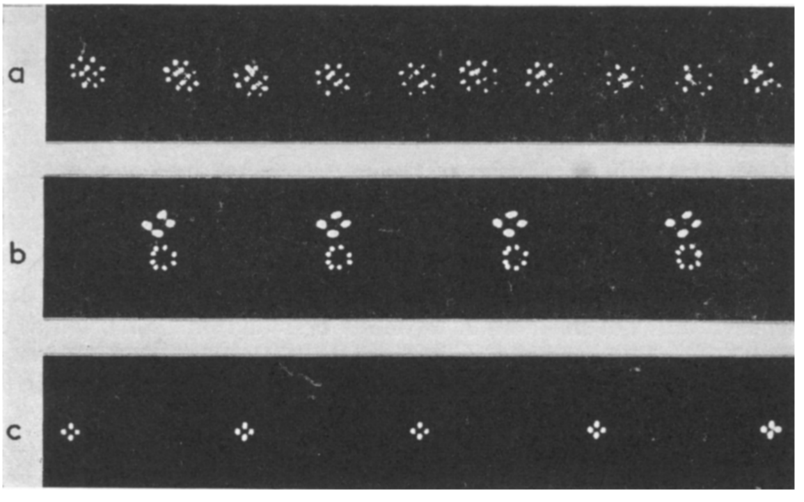

Fig. 41. — Essais de Hartmann (réfracteur de 60 cm).
***a*, écran à 12 trous, lunette visant une étoile; *b*, les quatre trous centraux couverts par un miroir plan, autocollimation sur une source ponctuelle au foyer de l'objectif; *c*, source au foyer, coupole fermée.**

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without the star, the dome having been kept closed for several hours so that the temperature should be quite uniform inside. The four dots are quite regularly spaced and they do not move; but as soon as the dome is opened, then the front end of the tube begins to cool, the cool air flows down and inhomogeneities appear inside the tube.

For the occultation technique, the device is quite simple. A star image is focused onto a knife edge through a parallel glass plate. This plate is tilted about an axis parallel to the knife-edge, by means of a synchronous motor and a cam, so as to cause the image to move normally to the knife edge. In 2 s, the image travels from a completely occulted position to a completely unocculted one, then moves back quickly and starts again. A photomultiplier collects the light behind the knife-edge, and the photo-current is displayed on a cathode-ray oscilloscope having its horizontal scanning triggered by the same cam at the start of the plate. On plate XI some oscillograms show conspicuously the increase of scintillation with decreasing diameter of the objective. The average slope of the diagram gives the spreading of the image, while the horizontal widening of the central portion gives the mean image motion (see *Proceedings of the Symposium on Solar Seeing*).

MEINEL. — First, two general remarks. Although the title of this Symposium is Site Testing, it has been very obvious that we cannot do effective site testing until we settle the question of the physics of seeing itself. In addition, most of the methods that have been discussed involve the gathering of observation that require a large expenditure of time and effort in reduction. Quite obviously the problem of testing a primitive site for its suitability is one of the principal problems that we are concerned with, but this is made difficult because we know so little about the basic physics of seeing. I prefer, by the way, to use the term " seeing ". Large telescopes, which are our central concern since the expenditure of millions of dollars is involved, suffer principally from image blur. Image motion is only the consequence of limiting the aperture size. While this may be done to gain image sharpness, it is not meaningful to define blur and image motion as two separate quantities, since they depend upon the size of the telescope. I would prefer to use the term " seeing " for the integrated effect that one has with a large telescope.

The second subject that I would like to discuss is site testing. In general, we have a primitive site to be considered. The problems of site testing under these conditions can be generally grouped under four headings. First, the lack of skilled scientific personnel to man these sites during a long period of time. This fact is something one must face when designing equipment for their use. Visual methods suffer from the tendencies often shown by these observers. Usually they can be classified into the type that does not really have the diligence required, and at the other extreme, the person who feels that this site is the best of all. In either case one cannot trust the data. Second, the lack of electrical power for extensive use. This limitation helps to determine the type of impersonal instrument that you would design for primitive site testing. Third, testing at a primitive site usually involves ground effects. These can be serious since when you finally build the telescope its optical system will be 10 to 20 m above the ground so that the test made from the ground is not necessarily definitive. At least a ground test is limiting in the sense that the seeing can be no worse at a higher elevation, and it should be better. Testing from the ground is therefore limited since you really do not know how good a site is : you only know how poor it can be. If one attempts to avoid ground effects by using a tower, or even if one does not, one encounters the fourth principal problem : that of wind shake of the instrument.

The two general topics that we are discussing, the physics of seeing and site testing, remind me of a problem of similar nature in photometry. Dr. Stebbins always tells his students that they must decide that they will either study extinction or study stars since one cannot study both in the same night. I would like to talk about an instrument to measure seeing.

It would be very nice to talk about an instrument that would meet all the above conditions, but it does not yet exist.

I would like to describe the instrument developed for use in the site selection program for the observatory which was later located on Kitt Peak. These telescopes were to be mounted atop 60-foot towers, but practical problems prevented their extensive use in the high towers. Regular observations however were made from 10-foot towers. The installation of towers on Kitt Peak is shown in figure 42. Before I describe the instrument I would like to make a comment about the towers. Each tower consisted of three concentric towers, the innermost carrying the instrument and the outer two providing protection from wind. It was found, however, that the air-coupling between the towers caused the inner structure to develop objectionable motion when strong winds were incident upon the outer tower. Each of the three towers had a separate foundation. I have some misgivings about Dr. Kiepenheuer's towers since ours used similar construction and the parallelogram idea failed apparently due to cross-coupling terms. On the other hand, the Polaris towers are perhaps more sensitive to seeing, since tower motion of 1 second of arc was deemed serious.

The Polaris telescope was designed to operate without the assistance of personnel for a period of one week under the most severe environ-

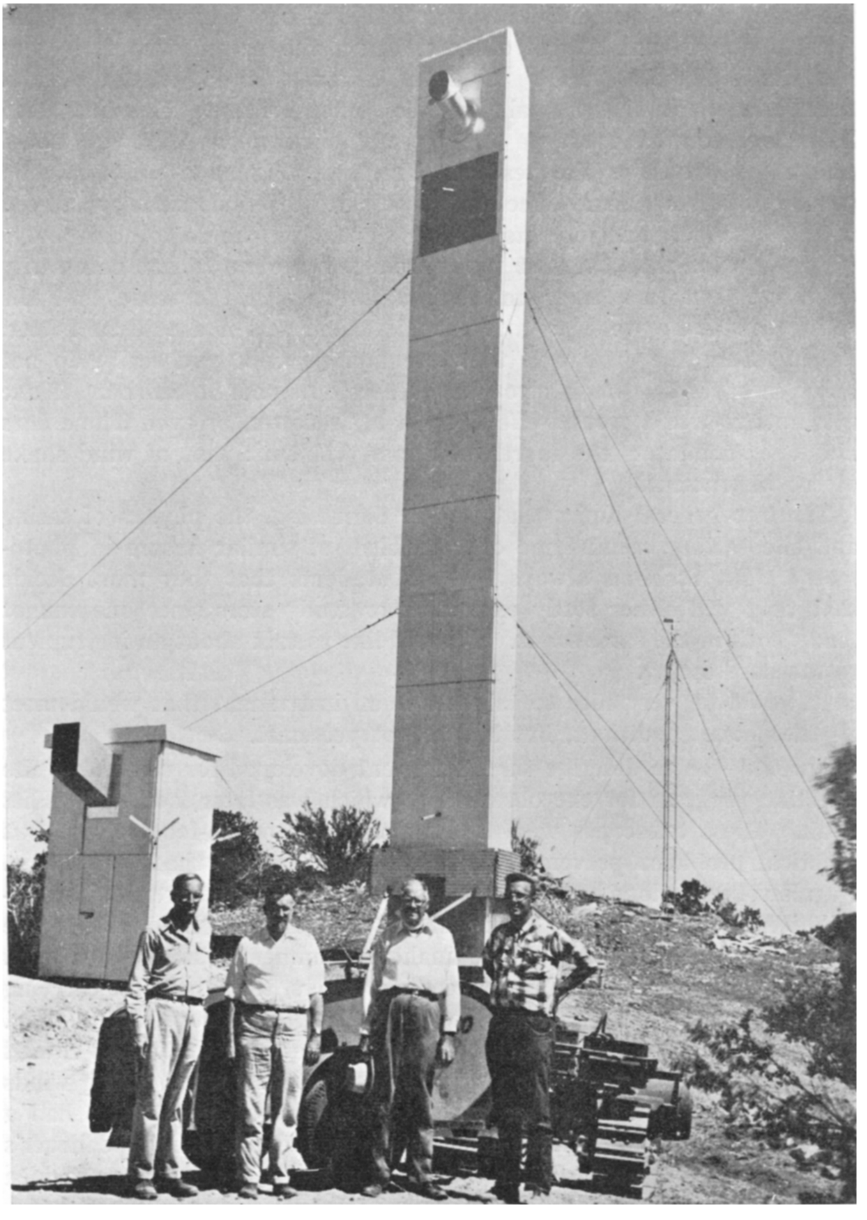

Fig. 42. — Photograph of the seeing test towers located on Kitt Peak. Polaris monitor telescopes are located in both the 10-foot and 60-foot towers. Microthermal probes project out the north face of 60-foot tower near the top and bottom. Note the outriggers supporting the outer tower shell. Shown are Drs. E. F. Carpenter, F. K. Edmondson, J. C. Duncan and A. B. Meinel.

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mental conditions of high mountain sites. Power limitations made it necessary to avoid the use of motors of similar devices. Instead we designed the telescope to employ the diurnal motion of the earth to scan the image of Polaris. The telescopes were Cassegrain catadioptric systems of 6-inch aperture and 216 inches focal length. This focal length produces the image of Polaris outside the periphery of the primary mirror when the optical axis of the telescope is pointed at the north celestail pole. The image profile of Polaris was measured by means of a reticle placed around the primary mirror. This reticle had occulting bars photographically reproduced on a glass plate. The widths of these bars varied in steps of two ranging from 0.5″ to 8″ with a clear and a dark space of 20 ″ width separated each cycle of bars.

The position of Polaris outside the primary mirror resulted in significant aberrations even though the system was well corrected for spherical aberration and coma through the use of the meniscus corrector. The residual astigmatism produced an image approximately 5″ long but whose sagittal width was entirely diffraction limited. The exact diffraction image of Polaris combining both geometrical and physical optical aberrations is given in chapter 10 of *Telescopes* (Ed. Kuiper, Chicago, 1960, p. 164). The image of Polaris is transferred by means of a driven parallelogram prism to a 1 P 21 photomultiplier located on the optical axis. The drive for the pickup prism was provided by a flexible shaft connected to the spring-driven Esterline-Angus chart recorder. It was found that the clock rate of the recorder was accurate enough to permit tracking Polaris for an entire week, barring unforeseen difficulties, which incidentally seemed to occur rather frequently, such as freezing during winter or nesting owls during summer.

The diurnal rate scanns the image of Polaris at a rate of 1/4 second of arc in 1 s of time. With electronic integration of the order of 1 s one therefore both integrates the motion of the image during that time and has sufficient resolution of the image to detect blurring. Reduction of the occultation was simplified in actual practice by measuring only the depth of the occultation by the 1 second of arc bar, assuming that the image intensity distribution during the integration time was approximated by a Gaussian distribution. A more complete analysis was out of the question since an automatic instrument of this type generates kilometres of data during a year. Perhaps modern data handling techniques now would permit fuller utilization of data from an automatic instrument. The results obtained with this instrument are summarized in the reference cited above.

There were numerous problems associated with breakdown of the equipment and the severe remote environmental conditions. Our principal problem, however, resulted from what we hoped would be our principal gain on this survey, that of putting instruments on top of 60-foot towers. Operation from these towers proved exceedingly difficult because of wind shake. Even though the towers were protected by two outer towers there was still enough dynamic air coupling between the outer towers and the inner structure to cause motion of the telescope which was of the same order of magnitude as the seeing that we hoped to measure. A long series of observations was accomplished at Kitt Peak from a 10-foot tower during a total of 150 nights in 15 months.

There are various electronic means of different degrees of sophistication with which you can separate image motion from image blur in a small telescope. Incidentally, image blur with a small telescope such as the Polaris telescope is always negligible at a good site. One can only see appreciable broadening of the image of this diffraction pattern when the seeing is bad. It is principally the motion that one must integrate in observations with small telescopes. The aspect that was perhaps most encouraging about this Polaris instrument is that it did not confirm that any of the sites had exceptionally good seeing (0.2″-0.6″) of the type that is so often reported by visual observations. The observations with the instrument show a maximum at about 1.4″ which does agree reasonably well with what has been obtained later with the large telescopes that have since been built on Kitt Peak. It was interesting to note that this same instrument was adapted for a survey at Cloudcroft, New Mexico, in which the visual reports again indicated better seeing than was confirmed by this instrument. Most of the people who have studied these two survey programmes have tended to give greater weight to the answer of an impersonal instrument than to the answer of a visual observer. In some manner, this symposium should attempt to define a type of instrument suitable for use at a primitive site which will yield reliable estimates of the seeing. We hope that this will be possible as a by-product of some of the current work on the physics of seeing.

Hogg. — Thank you Dr. Meinel. Before calling on any other speakers, I immediately hoist a flag " Unfair to southern observers ".

Rösch. — First, I think I have been happy in choosing a Chairman coming from the southern hemisphere, so that he could draw our attention to this problem. Second, I agree completely with Dr. Meinel that, up to now, we have been dealing widely with " physics of seeing " and not exactly with site testing; but I am sure that this was necessary and that we have not even yet said enough about the physics of seeing. My conclusion will be that after this meeting I must tell Dr. Sadler, General Secretary of I. A. U., that we need another Symposium, either on the physics of seeing or again on site testing.

It seems that everybody agrees on the importance of having instruments high above the ground. We are therefore faced with the problem

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of mounting a somewhat elaborate instrument (giving as much information as possible) on a tower at a certain elevation above the ground and at the same time being able to move this equipment from one place to another in the field. Dr. Courtès has suggested that this could be done in several steps; this may be the only practical solution. My proposal of using Hartmann tests with a mosaic of small mirrors, I think, can be made practical by reducing the number of mirrors; at the limit, we come to the telemeter-like system used by Dr. Stock. With more than two mirrors, say three or four, we may already get some useful information about the low spatial frequencies on the wave front. I think there is a line of research to be followed toward a compromise : by reducing the number of mirrors, we can obtain equipment light and compact enough to be mounted, if not at 75 feet above the ground, at least at such a height as to avoid most of the ground-layer effects.

One point also which I want to make is the importance, which Dr. Courtès emphasized, of the studies made with large instruments. I am sure Dr. Bowen agrees also that much is to be learnt by using the large existing instruments, maybe not directly for site testing, but on the physics of seeing.

Hoag. — I would like to speak very briefly on some points in Dr. Courtès paper. Taking these points in order of the instruments suggested, I think that the all-sky camera which is proposed is an excellent method of determining the sky coverage at any time, even at night, but I think that the amount of reduction that is required in analysing these photographs is excessive.

Courtès. — I would remark that this reduction needs just a look through a few pictures a night !

Hoag. — I think that all observatories have a responsibility for assessing the quality of their own sites in order that we may know something about the physics of seeing and in order that a direct comparison may be made between existing sites and sites that are being tested. At existing sites an instrument of moderate or large size is often being used by a small group of people, who cannot devote much time to observations and reductions of this sort, so that we must take use of automatic devices or the by-products of normal research.

It is quite clear that uniform and quantitative methods of evaluating the quality of existing observatory sites would be of value in future site selection surveys. The need for this material and suggestions for acquiring it have been presented in the Report of the Working Group on Site Testing (Rösch *et al.*, 1961). It is my purpose to endorse the need for this kind of activity and, at the same time, plead that these methods of evaluation take a minimum amount of time from observers' normal efforts.

There are many methods for site evaluation which have been employed or suggested in addition to those cited above (for summaries *see* Stock and Keller, 1960; Meinel, 1960; Nettleblad, 1953; Wimbush, 1961). A complete application of these techniques at each existing observatory would be impractical in most cases. We should therefore look to automatic devices and by-products of normal research activities as means of obtaining the desired information. The examples which follow are illustrations of these two approaches taken from the work of the Lowell Observatory, Atmospheric Research Observatory, and the Flagstaff Station of the U. S. Naval Observatory, all in the vicinity of Flagstaff, Arizona.

Transparency. — An automatic photoelectric Polaris monitor (Mikesell, 1955) is used to record the relative transparency of the atmosphere at the Flagstaff Station. Any Polaris monitor is an instrument with built-in bias being restricted to one part of the sky in a limited range of latitude. It is, however, a kind of instrument which can easily be made to operate without attention. Our present monitor observes Polaris, an adjacent area of sky, and an internal standard light source in a repeating cycle throughout the darkness interval each day. From the records of these observations we summarize, each month, the elapsed time and percentage amount of the darkness interval for each of the following conditions : photometric transparency greater than 90 %; transparency 90-50 %; and transparency less than 50 %. We define the photometric condition as that continuous part of an interval of greater than 90 % transparency

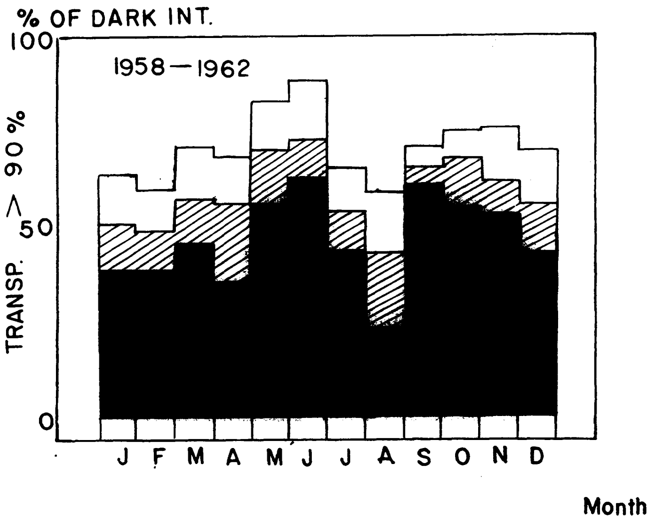

Fig. 43. — Average amount of the darkness interval which is more than 90 % transparent at the pole (hatched) with maxima and minima.

showing variations in transparency of less than 2 %. On the average, 70 % of the time reported as greater than 90 % transparent is photometric.

Mean monthly values for the condition, transparency greater than 90 %, for a four year period, together with extremes, are shown in figure 43. The reduction time now required for these records is about 1 h per month and we are planning to replace the present mode of recording with elapsed time dials which will directly display the desired information, thus further simplifying the reduction process.

A further evaluation of the sky transparency at the Flagstaff observatories is provided by records of photoelectrically determined extinction coefficients. Values of these quantities and their seasonal variations have been discussed by Serkowski (1961) as a by-product of the Lowell Solar Variations Program. A summary of these results for B-magnitude extinction coefficients with 42 additional determinations for the 1961-1962 season is presented in table I.

TABLE I.

B-*magnitude extinction coefficients* 1955-1962 *Lowell Observatory*.

Month.	Q_{bl}.	No.	
January-February................	0.245	22	±0.025
March-April.....................	0.282	42	±0.043
May-June........................	0.295	35	±0.045
October-November-December.....	0.223	9	±0.064

Individual values of 108 determinations of the B-magnitude extinction coefficients are shown in figure 44.

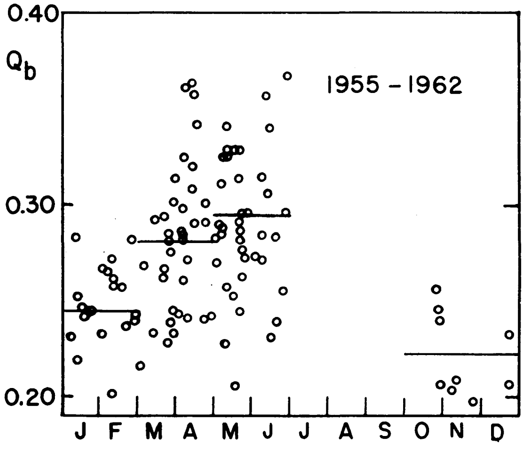

Fig. 44. — Seasonal variation of the extinction coefficient for B-magnitudes. Lowell Observatory.

The mean value of the extinction for B-V colors for all seasons represented by this work is 0.115 magnitude with a dispersion of $\pm$ 0.015 magnitude.

Still another observational quantity having an important bearing on transparency is the amount of precipitable water vapor in the atmosphere. This factor is regularly determined by Adel at the Atmospheric Research Observatory as a by-product of his infrared investigations of the constituents of the earth's atmosphere. Mean monthly values of the precipitable water vapor above Flagstaff are shown in figure 45

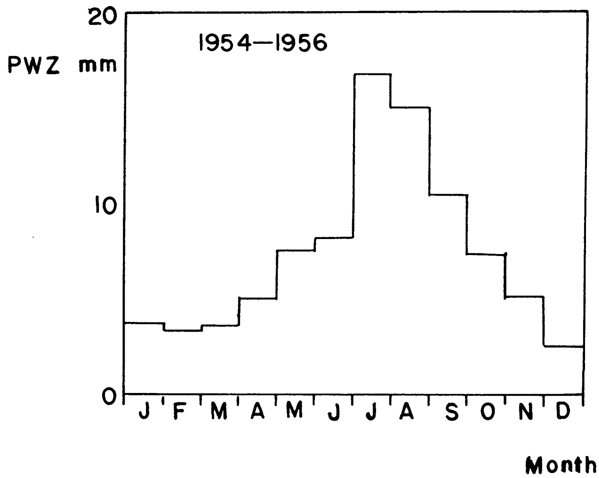

Fig. 45. — Precipitable water vapor over Flagstaff (adapted from Adel, 1958).

which has been adapted from one of Adel's reports (1958). It is evident from figures 44 and 45 that some correlation exists between seasona values of the B-magnitude extinction coefficient and the average amount of precipitable water vapor.

Integrated image quality. — One useful quantitative feature of image quality is the diameter of the integrated image. This is often estimated visually but may be measured objectively as was done by Meinel (1958) during the site selection survey for the National Astronomical Observatory. It has been my practice to measure the average size of the image by what I call a knife edge cut (Hoag, 1959). In the process of focussing by means of the Foucault test, I measure the lateral shift of the knife edge from the position where the first shadows appear on the mirror to the position where the last bright spots vanish. These measures, when made slowly, show small dispersion over a large range of aperture and magnitude if other conditions remain constant. Photoelectric calibrations indicate that the knife edge cut measures approximate the diameter of a hole which will admit 90 % of the light of a star image. I have also found that these measures are approximately linearly related to air mass so that knife edge cut values can be reduced to the zenith. This relationship also indicates that photoelectric determinations of extinction coefficients are systematically affected when small apertures are used.

A frequency distribution of knife edge cut measures for 200 nights over a six year interval is shown in figure 46. The large values shown

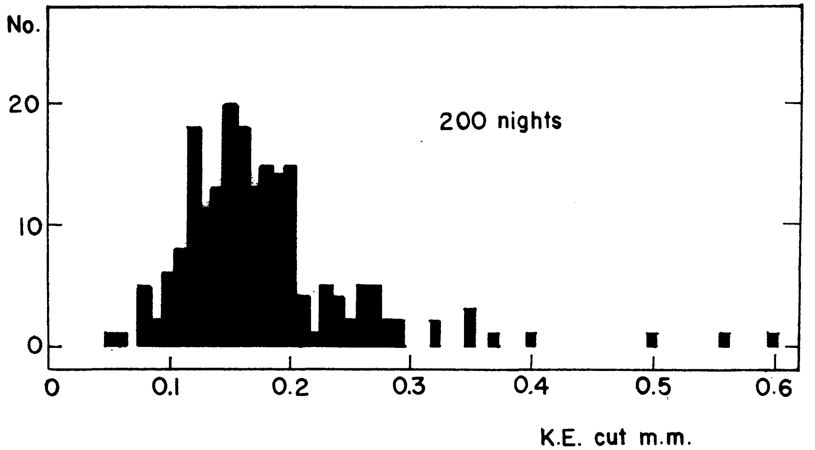

Fig. 46. — Frequency distribution of integrated image quality (knife-edge test) measures over a six-year period.

in the distribution were observed during clearing following storm front passages. The knife edge cut scale of figure 46 can be converted to seconds of arc by applying the telescope scale, 30 seconds of arc/mm. Photoelectric measures of the distribution of light in images allow conversions to other scales. For the median knife edge cut for the illustrated distribution, 0.16 mm, a 4.8″ aperture will admit 90 % of the light of an integrated image, and 68 % will be admitted by a 1.4″ slit.

Time resolution of image quality. — If the " seeing " is characterized by optical turbulence of small, uniformly distributed elements, the image formed by a large aperture instrument will always be poor. In many cases, however, the turbulence is such that instants of good image quality occur. It has been a common practice of observers to visually monitor image quality and expose only during selected intervale. This technique is useful to some degree but it has been demonstrated by Gaviola (1949) and many others that the intervals of best image quality are too brief to be taken advantages of in this way.

Leighton (1957) has perhaps published the best review of motion picture sampling as a method of selecting instants of good image quality. He has found by experimental cinematography of solar granulations under the best conditions that 19 of 6 650 frames were sharp over approximately a 4′ square field for a 12-inch aperture. Photography of this sort brings up the problem of post-selection-finding the one good frame in 300 in the case of the example cited. However, an even more arduous task of post-selection is involved in culling single photographs as, for example, the selection of plates for Kuiper's lunar atlas (1960). Perhaps the outstanding example of post-selection is Jannsen's (1885) photograph of solar granulation [Jannsen (1896), or Kiepenheuer (1953)].

That short intervals of good image quality do occur is further illustrated by current work at the Lowell Observatory. The advent of the image tube has made cinematography of double stars possible. Hall, Baum, Ford and Fredrick have used the 24-inch refractor with a cascaded image tube for double star photography (Fredrick, 1960). In the classical method of double star photography, initiated by Hertzsprung and further developed by Strand and others, stars separated by about 1.5″ can be measured. The Lowell investigators have found that, with the shorter exposures made possible by image tubes, the limit can be pushed to about 0.5″. Confirming evidence has been secured by Rösch, Wlérick and Dupré (1961). Section A of plate XII shows successive frames of a 24 frame per second run on 51 *Aquarii* = ADS 15902, with a 24-inch aperture. The exposures were approximately 1/30th of a second. In 30 feet of film so exposed, 3 frames (about 0.25 %) show images of good quality. The separation is about 0.6″.

Another example of short exposure post-selection is shown in part B of plate XII. In this case the separation is 0.8″ and the magnitude difference is about 3. The pair is 85 *Peg* = ADS 17175.

The Aeronautical Chart and Information Center, U. S. A. F., is currently engaged in lunar mapping. A group headed by Cannell is now using the 24-inch refractor at Lowell as an aid in interpreting Kuiper's lunar atlas charts. In addition to visual work, cinematography has been employed. The phenomenon of short intervals of good image quality is well illustrated by a sample of their work. Parts C and D of plate XII show greatly enlarged sections (negative prints) of adjacent frames of a motion picture taken of the *Fra Mauro* region. The rate was 8 frames per second with exposures of 1/15th of a second on Shellburst film. These two examples are representative of the difference in quality of the contiguous frames over a 7′ field photographed with a 24-inch aperture.

Automatic recording of image quality. — We are installing an additional Polaris monitor at the Flagstaff Station which is designed to answer what I consider to be two vital questions concerning the suitability of a stellar observing installation as follows :

1. What fraction of the darkness interval does an amount of light in a star image X percent enter an aperture of d'' (say 80 % in $3''$).

2. How many intervals of duration t occur per unit time when an amount of light in a star image X_1 percent enters an aperture of d_1 (say $t = 1/25$ s, $X_1 = 90$ %, $d_1 = 1''$).

The instrument has an aperture of 6 inches and will employ a guiding system similar to that developed by Leighton (1956). The measuring unit will resemble a conventional photoelectric exposure meter in some respects but will also incorporate a " pre-selection " device similar to that proposed by Platt (1957). The time constant of the guider will be adjusted to make the response of the measuring unit equivalent to that for a large aperture instrument.

Lynds. — As Dr. Rösch mentioned, the double-beam telescope Dr. Stock used in Chile is in fact equivalent to a large telescope having a two-aperture Hartmann diaphragm with the slight difference that the images produced are in focus. Also, these images can be adjusted in relative position at one's convenience. It has occurred to us that the instrument should be fitted with a photoelectric device to measure automatically the relative image motion and yield a quantitative observation of the seeing parameter now being estimated visually. One such device, which has worked well in the laboratory, consists of a symmetrical squarewave light chopper operating in the focal plane of the double beam telescope and a photomultiplier, to detect the transmitted light. The chopper is a rotating radial bar-pattern reticle having a spatial period corresponding to 40 seconds of arc on the sky. When the chopper is rotated the integrated A. C. signal present at the output of the photomultiplier is an inverse function of the star image separation over a range of 0 to 20 seconds of arc separation. It is of course the change of separation or relative motion of the images which is recorded. We hope to have this instrument in regular operation in the near future.

Rösch. — I would just like to mention in connection with this type of equipment a device which, I know, has been used by our Russian colleagues. To measure image motion, they use a cylindrical lens just in front of the focus so as to project, not a point-image, but a focal, onto a V-shaped aperture; then, if there is a motion in the direction of the axis of the V, the record of the changes in flux gives a measure of the image motion, provided you have corrected for scintillation, indeed. I do not know how they manage for this correction.

Courtès. — Perhaps, there are two positions, one in the large part of the V where the scintillation can be appreciated, and, after that, the image diameter is measured in the small part of the V.

Hogg. — I would like at this point to take a somewhat mean advantage of my position as Chairman to call on myself to describe an instrument that has been developed in Sydney. This is an instrument which has been devised by Ramsay and Kobler of the National Standards Laboratory in Sydney and it is intended to record simultaneously the variations in scintillation, in blurring, and in image motion. I do not propose to enter into a detailed description of this instrument because an account will shortly be published in the *Observatory Magazine.* An occulting disc, somewhat similar to the type described by Dr. Lynds, is inserted at the focal plane of a 6-inch telescope. The light from the telescope passes through a photocell and the D. C. output from this is of course proportional to the scintillation. The D. C. output is measured, not in the normal fashion, but by beeing maintained constant with the aid of a feed-back loop to the L. H. T. supply and a mechanism to control the led high tension source. The fluctuations in intensity are recorded in the form of the voltage applied to the photomultiplier. There is an alternating component which is brought about by the image blurring, and the amplitude of this is measured. The occulting disc carries around its periphery similar fixed segments to those of the central portion and a light, fixed in position, passes through the disc to a phototransistor, the A. C. output of which establishes a fixed reference phase. Image motion of the star brings about an alteration in the phase of the output current and this alteration of phase is compared with the reference signal. The instrument itself is very compact and has been designed to work off a 12-volt battery so that it can be used in the field. Record have been obtained by Ramsay and Kobler and by Wood at the Sydney Observatory, using a 6-inch reflector with which they can quite readily measure stars of second magnitude. I refer those interested to a forthcoming article in the *Observatory Magazine* by Ramsay and Kobler.

Rösch. — I think, Mr. Chairman, that this is extremely interesting, but I wish to point out that in a sense the philosophy of this equipment is almost exactly the same as the method I have used, except that in my equipment the edge of the screen is fixed and I move the image by tilting a parallel plate; whereas here the image is fixed and you turn an occulting disc. I have, so far, only recorded the photocurrents, but I am sure I could transform these, since I have the A. C. component, the D. C. component, the mean slope and so on, and get similar results.

Dommanget. — Il me semble nécessaire de faire une remarque générale concernant la plupart des exposés et interventions de cet après-midi. Le sujet proposé pour la présente session consiste en la discussion critique d'instruments permettant l'observation des effets de la turbulence atmosphérique, mais nécessairement utilisables en campagne puisque leur emploi doit permettre le choix de sites d'observatoires. Or, à aucun moment, il ne fut question des facilités ou des difficultés d'utilisation des instruments proposés au cours d'une prospection. On n'a pas examiné non plus les incidences des méthodes d'observation correspondantes en ce qui concerne le dépouillement et les réductions des données d'observation, ni, en particulier, en ce qui concerne le personnel nécessaire pour mener à bien les opérations. L'un des orateurs a insisté d'ailleurs sur la difficulté de recruter ce personnel, spécialement pour des raisons financières. Nous avons nous-mêmes rencontré de telles difficultés en Afrique du Sud. C'est pour ces raisons entre autres, que je reste convaincu du grand avantage de la méthode des anneaux de diffraction telle que l'a proposée A. Danjon. Les observations sont rapides (15 mn par séance de 10 étoiles), les réductions immédiates (moins de 10 mn par séance) et le personnel nécessaire extrêmement réduit. De telles qualités devraient entrer en ligne de compte pour le choix de toute autre méthode d'observation en campagne.

Rösch. — Je pense comme vous que la méthode des anneaux de diffraction est une méthode de base, mais je crois qu'il faudrait la compléter par une méthode donnant quelque chose sur la forme de la surface d'onde sur une étendue plus grande, c'est-à-dire en fait la méthode des anneaux de diffraction, plus une évaluation du déplacement d'ensemble de l'image pour un instrument de 20 ou 25 cm de diamètre, et par conséquent une mesure du mouvement de l'image dans ce télescope. Reste à savoir quelle est la méthode à utiliser pour évaluer le déplacement de l'image, qu'on n'évalue pas à l'oculaire directement.

Dommanget. — Je ne voulais pas présenter la méthode des anneaux de diffraction comme la seule utilisable : je la considère simplement comme un modèle. Bien entendu, si l'on pouvait mettre en œuvre des procédés aussi simples et aussi rapides pour la mesure d'autres paramètres de la qualité des images, comme par exemple l'agitation, il faudrait en envisager l'emploi au cours des prospections.

Stock. — I think we should apply the same principle as we heard is applied at the Royal Meteorological Society : that is, no criticism should be withheld. Some of you may have got the impression that a new component or a new term has entered the discussion, namely something that is called " accidental refraction ". I believe that any erratic deviation of the position of the star, or the path of its light, with a time constant of several seconds or less, we will have to consider as part of our problem. I still propose that we call the whole problem "seeing" and that we should not call part of it "accidental refraction". I noticed with interest that in most of the photographs obtained by Dr. Rösch, and showing very short-exposure stellar images, the size of these images was, in almost all cases, of the order of 1 or 2 seconds of arc, that is of the order which we all are accustomed to when we use large instruments. In one case an image profile was shown and this was also an image of something between 1.2″ and 1.5″, as far as I could tell. On the other hand, when estimates of the seeing are made on the Danjon scale with, let us say, a 10-inch reflector, the figures that come out, for good seeing, are between one and 2/10th of a second of arc; for extremely poor seeing, they may be of the order of half a second of arc. We have a scale difference here of only half an order of magnitude, which makes the interpretation somewhat difficult. Another thing that I want to emphasise again; our goal must be to predict the conditions that a large instrument, a 200-inch or even larger, will experience. We all know that we shall get a blurred image, and this blur will be the principal thing that we shall have to predict. When we use a smaller instrument we may have blur and we may have image motion, and we must take both these effects into account. It was proposed that the first survey of a site should be made with a 10-inch telescope and that it should be based on estimates of the resolution of the diffraction pattern or on the Danjon method. Now, I just want to point out again why we have developed a different system. A number of years ago we made a long series of observations with a 10-inch reflector and a 60-inch reflector, estimating in the 60-inch telescope the diameter of the image, and determining with the 10-inch telescope, placed on the side of the large instrument, the diffraction according to the Danjon scale. We plotted as ordinates the estimates of the diffraction pattern, the abscissae being the image diameters observed in the 60-inch telescope. We observed that the whole graph was covered with points almost uniformly, except that they seemed to funnel into the origin. This is quite easy to understand : when a large instrument registers perfect seeing, this means that there is no turbulence whatsoever on any scale, so that very good conditions will be observed with any kind of instrument. However the reverse is not necessarily true. If we are concerned only with extremely good seeing, which as all of us know is very rare, then of course a 10-inch telescope is a very good and simple device and probably the one to be recommended because of its handiness. However we must not expect that we will find a site where we have such good seeing for any large fraction of the time. We will have to take other kinds of seeing into account also, and for this we must have a method which gives us with sufficient accuracy the seeing to be expected with a large instrument.

COURTÈS. — In answer to the first part of your question, I think the diffraction ring method is not too accurate, because, in some cases, the big telescopes give much better images than I have shown; the photographs I showed (*fig.* 36 à 40) were of no more than average image quality; they had not been taken in particularly good conditions. But I agree with Dr. Bowen, that it is possible to reach a power of about 0.3″. If you want to be certain of measuring it properly, you need a method which assures you of the flatness of the pseudoflat elements of the wave surface, to about 0.20″ or 0.25″; this is in the middle of the Danjon scale. With regard to the necessity of knowing, for larger telescopes, if there is any deviation from the general tilt of the different parts of the wave surface, I think the Danjon method is accurate enough, particularly when the seeing is exceptionally good (the most interesting cases). But I would like to ask Dr. Bowen what are the best images they get in California or Palomar during one hour of exposure, for instance.

BOWEN. — The best I have seen is between a quarter and a third of a second, and this was on just one night. Normal size is a second to a second and a half, as measured visually, not on photographs (5).

COURTÈS. — But I think that if you have deviations of more than 0.3″ with a small diameter of 25 cm, you cannot hope to obtain a better image in a larger telescope. This is the reason why it is necessary to have so accurate a method in order to know whether the pseudo flat elements are, in fact, sufficiently flat.

(5) *See* Dr. Bowen's complementary information, foot-note, p. 166.

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