# DISCUSSION.

DOMMANGET. — Je crains que si une opinion ou une autre étaient émises pour ou contre les points de vue exposés, nous ne nous égarions. Il n'y a peut-être que le plan du Symposium à discuter, mais je crois que là-dessus il faut faire confiance au Président.

RÖSCH. — Dans mon idée, la discussion sur cette introduction générale ne doit pas constituer une préfiguration des discussions futures. Je suis bien d'accord avec Dommanget : il ne faut pas commencer à discuter telle ou telle question et empiéter sur les séances suivantes. Mais ce que je souhaiterais, c'est que l'équilibre entre les différentes parties soit bien établi. Et surtout, j'aimerais savoir si tout le monde est bien d'accord sur l'esprit du Symposium et sur les buts que nous devons nous proposer; c'est cela que je considère comme le point important de la discussion d'aujourd'hui. J'aimerais savoir si nous devons effectivement nous proposer, par exemple, de parvenir à des normalisations de méthodes, ou si certains collègues considèrent que c'est aller beaucoup trop loin dans ce que nous pouvons faire. C'est là le genre de questions générales que je voudrais voir soulever, plus que le détail de ce qui sera dit dans les séances suivantes.

BOWEN. — In his opening address, Dr. Rösch presented an excellent survey of the history of the formation and early operation of the Committee on the " Choice of Sites ". He also presetend a clear statement of the problem which, as he very aptly pointed out, is one of getting around the limitations imposed by our enemy the atmosphere. I am very happy that Dr. Rösch emphasized that, in spite of recent great advances in rocketry, we must still look forward to many years of ground based observations, for most astronomical problems.

It was pointed out that these atmospheric limitations are of two types. The first is caused by direct meteorological factors such as clouds and wind. Here we may expect great assistance from the meteorologists and their records. The second limitation is the effect of air turbulence on the sharpness of the image. As, Dr. Rösch has indicated, the second problem is very complicated and it is here that most of the differences of opinion will appear.

Two general approaches to the problem of the effect of turbulence on image size have been suggested and used.

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1. The direct approach by the measurement of image size, or what corresponds to it in an instrument of small aperture, the amplitude of image excursion.

2. The indirect approach through study of airflow over the site by measuring the height distribution of the turbulent layers by thermal or optical observations. This is essentially an attempt to understand the physical basis of the problem.

Dr. Rösch has presented both approaches to the problem very objectively but has indicated his preference for the first approach. I have been one of the more outspoken proponents of carrying out certain studies along the second line of attack although not to the exclusion of the first approach. Since these questions of approach have a bearing on the fields to be covered in later sessions it may not be inappropriate for me to outline the reasons for this view point at this time.

As it happened I was associated with two major site surveys, which used the direct approach, namely the ones that selected sites for the Palomar and Kitt Peak Observatories. From a study of these data I have come to doubt very seriously the validity of these measurements.

If we look at the thermodynamics of air flow over a mountain top we see the reason for this doubt. Thus during the day the sun beats down on the rock surface and raises the temperature many degrees. At night back radiation in turn lowers the temperature through a large range. On the other hand the air flowing over on isolated peak has not been in contact with the ground for a long period of time and consequently has a nearly constant temperature. Of necessity there must therefore be a large temperature difference between the air and ground for most hours of the day and night. The temperature gradient in the air increases rapidly as the ground is approached. Furthermore the movement of the air over the ground results in turbulence which causes mixing of the various layers of air of different temperature. The layers of air near the ground would therefore be expected to play a major role in enlarging telescope images.

This result has been confirmed by thermal measurements, some of which will be reported later and which show that rapid temperature changes increase as the ground is approached. Direct optical measurements made by two crossing beams from double stars also indicate that much of the turbulence causing image enlargement is located near the ground, that is to say withing a few tens of metres. We would therefore expect the seeing to improve rapidly with height above the ground.

Unfortunately, in the early surveys it has been customary to use small portable instruments that receive the light 2 or 3 m above the ground. We may therefore raise the question as to how valid these

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measurements are likely to be in predicting the performance of a large telescope located 10 to 30 m high.

One may then ask why not place the test telescope on a tower. This was tried in the Kitt Peak survey using towers 18 m high. The difficulty is that poor image quality in a small telescope manifests itself as image motion. Likewise any light movable tower is swayed by the wind and also causes image motion and it is very difficult to separate the two effects.

The study of image quality as a function of elevation above the ground is important from another standpoint. Thus it is easy to show that for both direct photography and most spectroscopic observations the effectiveness of the telescope varies as the first power of the aperture and inversely as the angular diameter of the " seeing " image. In other words halving the diameter of the image is just as effective as doubling the aperture. This may very well justify pushing the height of telescopes much above present practice. For example if the seeing image were reduced to 3/4 of its normal size by increasing the height 25 m it would in general be economically justified. The effectiveness of such an instrument would be equal to that of one a third larger at the lower level and in general the cost of raising the telescope would be much less than the doubling of the cost required for an instrument of a third larger aperture.

At present time one very major telescope in the United States which is in the preliminary design stage is being planned with its declination axis 45 m above the ground. Much more definite information about variation of seeing with height is very desirable to determine whether this is justified.

Finally I would like to emphasize again that I am not opposed to any direct approach. However no direct measurement seems to exactly duplicate exactly the final telescope conditions. Because of the great complication of the problem it seems to me that we should take advantage of any experiment either direct or indirect that will help us to obtain a correct solution to the problem.

Rösch. — I agree completely with what Dr. Bowen has said, especially about the importance of the elevation of the instruments (either the testing instruments or the final telescope) above the ground. However, I think that the problem is not only that of dealing with the Z coordinate, but also with all the surroundings of the instrument. I have some slides, which I may show in another session, of wind-tunnel experiments on domes of various types, which demonstrate very definitely the difference in the eddies around the dome according to the shape of the dome and shutters. I think that it is extremely important not to use a testing instrument quite close to the ground and thus that we

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must try something to get rid of support vibrations. Our discussions during the Symposium can lead to some idea as to which type of instrument could be best. But I think that for the telescope which is to be installed, there is also the problem of the dome itself. Finally, I am sure that we have to reach an agreement later on about the middle point between direct and non-direct measurements and about establishing correlations between direct and non-direct measurements. This is the heart of our Symposium, at least for the turbulence problem.

BOWEN. — I might add too that my interest in these somewhat indirect approaches is not so much that I do not agree with Dr. Rösch that the direct one is important, but a feeling of hopelessness about the difficulty of making the direct readings for a large number of points. We can probably improve the towers but still they are complicated things; one cannot move them around at a hundred different points.

UNDERHILL. — I am wondering if during the course of the conference we could contrast the advantages and disadvantages of a site that is within a flat area with mountains not very close (let us define later what very close is) and of one in a more restricted mountainous region.

HOGG. — I would like to suggest that the importance of elevating the telescope depends on the locality. We find at Mount Stromlo, in many instances, in fact most of the time, that an eye applied to the focal point of the telescope sees the disc of the primary mirror filled with light, and crossed by a pattern of quickly moving lines colloquially described as " telegraph wires ". On very few occasions do we see the shadow pattern with a slow lazy motion of turbulence, presumably produced nearby the instrument; and colloquially called " worms ". The " telegraph wires " fairly clearly indicate by their direction a correlation of seeing with upper atmosphere wind which is unlikely to be affected by conditions in the vicinity of the telescope. On the other hand, one feels that the slower moving patterns are clearly related to conditions at low level, in the surroundings of the telescope.

Thus the influence of the elevation of the telescope can vary very much from site to site.

WILSON. — I would like to reiterate one of the points brought out by previous speakers and put it in a slightly different context. The problem of a site survey is essentially a problem of a hierarchy of decisions that have to be made. Initially, these decisions are made on the basis of climatic conditions and of general meteorological theorems which are fairly well understood because there exists a great deal of meteorological data; for example, we would locate preferably an observatory in the south-west part of the United States, rather than in some other part,

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because there are a great deal of weather data showing cloud coverage and wind conditions favourable to optical observations. However, when we go to finer structure, what Dr. Rösch calls microclimate, there are very few general observations. There are no theorems in existence for example which tell us how to select a site in one location as against a location 2 km away. A portion of meteorological science that needs developing is the question of topographic effects on the microclimate, especially with regard to local seeing. To my knowledge, no answers have been given definitively. Many astronomers have a certain amount of experience but this is more an art than a science at the present time, and what we are faced with here in a need that has not yet been developed by the meteorologists to the extent that is required for astronomical purposes. As was said a minute ago, to answer this question we would have to test locally in a hundred spots. This is prohibitive, so the problem of where to test requires theorems relating topography to atmospheric turbulence which do not exist in a fully developed scientific form yet.

Scorer. — Dr. Wilson is asking for a general theorem about topography. This is really far too much, because it is impossible to express the shape of topography in general terms. There is quite a lot known about what will happen over certain kinds of topography but the absence of general theorems by meteorologists is due to the absence of any sort of general kind of topography rather than due to a lack of knowledge on the part of the meteorologists. If the meteorologist were to advise on whether the instrument should be set up at one place or 2 km away, he would have to visit the site and take into account a whole host of considerations which only a few meteorologists understand at the moment. But there are those few, I think.

Wilson. — That is precisely my question. If there exist such specialists, and they can be taken to a given site and can tell us on the basis of looking at the site, or by some tests, how to predict the local seeing, then we will have solved this problem.

Bowen. — May I add just one other comment here. One of the things which is rather simpler than you have indicated is this : the telescope is going to be located on a mountain top, for instance; should it be put near the leading edge of the cliff toward the prevailing wind, or should it be put back at a certain distance ? An answer to that question would be very helpful. I think there are a few general situations that would take care of a great many problems, rather than being quite so completely undefined.

Scorer. — I do not think the meteorologists know enough about what the problems are. I have learnt a terrific amount already this morning

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about your problems, but I think the answer to your last question would be different according to whether you wanted to observe the Sun or the stars.

BOWEN. — That might very well be. But I think you can see that you probably would be able to give some kind of an answer to the question. " Is the turbulence likely to be greater near the leading edge or back from it ? ", for instance.

STOCK. — I would like to make two comments on the subject. I have had frequent contacts with meteorologists and the situation seems to be like this. We are generally talking about night-time conditions which at most sites under consideration now are very different from day-time conditions, while meteorologists have paid very little attention to what is happening at night. Also a number of the questions that have been raised here are merely concerning the local topography and its effective advantages or disadvantages; we may actually have the answer already, from experiences gained in a number of site surveys and studies that have been made over the past years. Of course these answers are in the hands of the astronomers, not in the hands of meteorologists, and for the most part not available in the literature. That is why many of you are asking these questions, simply because the answer is still not published.

HOAG. — I think that when we discuss our problems with meteorologists we should admit at the beginning that they will see a great heterogeneity in the kind of information that we have available. In fact, for a given mountain, the meteorologists might find that there are as many ideas about conditions at this site as the number of astronomers that they talk to. I think this is an unfortunate situation, and my primary interest in this meeting is finding out how we can describe uniformly the conditions at a given observatory site, whether we already occupy one or whether we are in the process of testing to locate one.

SIEDENTOPF. — I should like to point out certain difficulties that lie between the site testing and the actual observatory that is to be built at the site. When the site testing is done, the territory is always in a very rough state and the final observatory will look entirely different; the ground will be covered with trees and bushes, artificial lakes may have been made, or something similar. So it seems to me that we could add to our discussion about site testing, another discussion about site improvement. It does not seem impossible to improve a certain site by changing the microclimate of the place.

FOURNIER D'ALBE. — Drs. Bowen and Hogg have both made remarks which I think are of general importance, concerning the height at which the disturbances which affect your seeing take place. Dr. Bowen said

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that he thought most of the trouble came from the layer below 200 m from the ground, but Dr. Hogg mentioned two types of optical trouble; one which he thinks comes from the lower atmosphere and one from the upper atmosphere above a height which he did not specify. These two phenomena are really quite distinct problems from the meteorological point of view. The turbulence in the upper atmosphere is a function of the general circulation and the general topography, whereas the lower atmosphere turbulence is above all determined by the local topography. Now, the astronomers have quite a lot of experience already of this and the meteorologists would be very grateful for any information that they can bring to this discussion. At what height in the atmosphere does this phenomenon occur which bothers you? I think the way the meteorologists will approach the problem depends to a great extent on the answer to this question.

BOWEN. — I think most astronomers recognize these two different height phenomena, the high level one due to jet-streams and things of that sort, and the low level one due to air of different temperature coming over the mountain. I was emphasizing particularly the low level one because that is what causes trouble in the site survey, due to the fact that one gets different effects depending on the height of the observations. The high level effects, we would agree, present no problem as far as site surveys are concerned, because they originate so very high that even a small telescope will give you roughly the same information as a larger one. My emphasis on the lower level does not mean that we do not recognize the other.

RANDIĆ. — In recent times there have been changes of site for several instruments, and I think it would be very useful if from now on people who are using these newly located instruments give all the details about the topography and the other conditions of the new site, so that some comparison can be made with the observations at the old one. For instance, we have had an opportunity to operate with the same instrument, with the same observers, in two different places, which differ in distance by about 7 km, but in elevation by about 800 m. We were measuring the geographic latitude, therefore it was very important to know how much the image was moving. The observing places were practically the same, there being a clearing in the forest in both cases. The only difference was that the instrument at the lower site was located about 2 m above the ground, whereas at the higher site the instrument was practically on the ground. The image was much better at the site of higher altitude and the results were accurate, so it is clear that only the difference in the elevation produced much better images and much less motion. It would be very useful if, in the case of changing the site of

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big instruments, we could compare the results. This is possible only if you know the corresponding conditions, which usually are not published.

POLLAK. — Somebody mentioned that the meteorologists had accumulated much material for day-time and very little for night observations. May I mention here that in the German literature there exists a book, by Prof. Geiger, from Münich, *Microclimatology in the neighbourhood of the earth surface.* The first problems about turbulent bodies were developed, discussed and observed in Vienna in 1915 and in Germany about at the same time. There are general principles which could be of use for astronomers and I think it would be a good idea if some meteorologist would find time to select all these observations which extend over the night and to put observations and general principles together. That would be a *Microclimatology for astronomers.* In England, in Germany, and also in America, there are many observations done on high towers with recording instruments. So it should not be so difficult to compile data for any time of the day. I myself made 30 years ago observations near to a lake with wind pendulums; we made cinema pictures and evaluated the photographs with stereocomparators. It would be possible to put together a number of very valuable observations which are available in the literature.

KIEPENHEUER. — I quite agree with you. We have made a lot of use of this book of Geiger, but it applies of course only to the very low levels, up to some 30 or 50 m. This book is to be highly recommended to all astronomers working in this field. But I would also add a few words about our questions to meteorologists. I think there are four different layers in our atmosphere which affect seeing in different ways. The first layer is the very low one, up to 30-40 m, which some people call the layer of forced convection; then comes the sub-cloud region which might go up to 4 000 m; above is the troposphere, which is affected by the large-scale motions of air and which will bring good seeing and bad seeing for longer periods; then the stratosphere with jet-streams, which might affect seeing or not, we do not yet know very well. These four layers have to be discussed somehow in a different way and our questions to the meteorologists must refer to these different layers, otherwise there will be much confusion. In addition to this, I think we have to discuss the situation above mountains and above plains separately, because all that can be said about these layers, especially about the convective one close to the ground, is that the phenomena are very different at day and at night in the mountains; therefore we must be very careful in considering these differences between plain and mountain.

SCORER. — I would like to ask a question now, rather than after I have had my say in session number four in the programme. It is much

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better that I should be forewarned and I now issue an open invitation to any astronomer who thinks he has ideas on meteorology and what he thinks meteorologists ought to tell them, to tackle me if he can, some time before Wednesday morning, because I know now what I would say if I were invited to speak now, but it might be modified very considerably in the light of what any astronomers might tell me.

BOWEN. — I am sure our people accept your invitation.

PROTHEROE. — I think we are being somewhat unfair to the meteorologists and I am afraid they are going to have still more trouble with us because I realize that we do not define precisely what we mean when we talk about seeing. One thing, for example : Arthur Hogg mentioned looking at shadowband patterns. I am not sure that you get a real measure of the seeing from shadowband patterns. I have made direct measures of the velocity of these patterns and, if my memory serves me right, never did I find the velocity below something like 15 m/s, and quite frequently the velocity is over 50 m/s; I know fully well that, as I was standing at the telescope, I was not subjected to a wind of 30 to 100 knots. One also knows that it is possible to introduce artificial turbulence in the vicinity of the telescope which completely changes the character of the seeing or the appearance of the image, at least as measured in terms of scintillation, without at all affecting the appearance of the shadowband pattern. So it appears that we do have two very different problems, whether one talks about the appearance of the image, or about the appearance of the shadowband pattern. It is an actual dichotomy, not an artificial thing, and one has to be careful to specify that which he wishes to talk about, before making too many wild statements.

STOCK. — I just want to make one remark. I think that we have created quite a bit of confusion already for the meteorologists who are with us, and I would propose that we defer any further discussion on the relation between our subject and meteorology until we have exposed the phenomena that we are concerned with, so that the meteorologists may have some idea of what we are talking about.

BOWEN. — I think this is a good point, and I propose to bring this session to a close.



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