

DISCUSSION.

RÖSCH. — Before opening the discussion, I have something to add which I forgot, or rather which I did not say. When I talked about the study of correlations between observing conditions and the meteorological situation, I had in mind the synoptic meteorological situation in the sense of situation of the air masses and this reminds me that we have been concerned mainly with effects near the ground. We have also been speaking of the jet-streams (not enough, indeed, but we mentioned them). But we have said almost nothing about the effects of the intermediate layers which are widely affected by the synoptic situation. What we can say about these effects will be largely supplemented by observations made in existing observatories as a function of the meteorological situation. I think that is what Prof. Kiepenheuer wanted to mention.

KIEPENHEUER. — You mentioned the Russian experiments in the wide plains of Western Asia, and the fact that even there conditions were not good, or were variable; this might be due not to the plain but to the tropospheric synoptic situation there, which of course I do not know at all.

RÖSCH. — This is the answer to the question I raised a moment ago. It is clear that it is not a matter of *microclimate*, it is a matter of *climate* in the common sense. If there are no geographical differences over a large region, the only factor is the general situation. Just one more word : Dr. Cesco mentioned to me during the break that it would not be enough to recommend to the existing observatories that they should undertake some observations and experiments. We must draft some instructions, suggest types of experiments and explain how to conduct them if we want them to have some meaning.

CESCO. — I have in mind the amateur astronomers of whom there are many in Argentina, Chile, and Peru; most of them have quite large telescopes, and maybe it would be of some importance to get this kind of people interested, for they are very careful and they love astronomy. I think that we could find easily, at least in that part of the world, about five hundred telescopes available for such work.

DOMMANGET. — I think it will be probably a matter for this group you proposed in the paper you read this morning, Dr. Rösch, between meteorologists and astronomers, to standardize the techniques, the methods of observations, size of instruments and so on.

VAN ISACKER. — I just want to say a few words about what Dr. Rösch said a moment ago, concerning the wave-front. One thing which is quite clear, but has not been said, is that this wave-front is in fact the link between meteorologists and astronomers. Corrugation is due to purely atmospheric causes, but its effects are a problem only for astronomers; they do not concern meteorologists at all. But, I think we must define a bit more precisely what we want to know about the wave-front. A complete description of it is quite impossible and perhaps not useful. But what we can say is this. A wave may be represented by an electric field, with an amplitude, and a phase ($\omega t - \varphi$). This is what interests us, because amplitude just gives us scintillation, which I understand is not very important for astronomers. So the important thing is the phase, which is a function of x, y, t , azimuth, elevation, and so on. What we must know is some statistical value of this phase, and we can write for instance that the mean value of the square of the phase may be expanded in a spectrum $S(k)$ where k is a wave-number, or $k = \frac{2\pi}{\lambda}$ where λ is a wave-length. But what we must know, and what I suppose is sufficient for our use, is the spectrum in a certain range of wave-lengths which must be about 10 m for very large apertures down to 10 cm, because I suppose that the amplitude of corrugation of the small wave-lengths is also very small. Now, the form of this spectrum can, in fact, be deduced from observations of the temperature and humidity of the air; in simple cases, mainly on a flat surface, I think that meteorologists can give some indications about the general form of it. That is one point.

Another point is about a question of Dr. Rösch, why are observations made on flat plains generally bad? I think it is very simple. On clear nights we have cooling of the ground, causing an inversion of temperature which may give an angle variation or motion of about 1". Therefore we cannot expect to have very good seeing in normal conditions. So you try to find some abnormal conditions, on a mountain for instance, where this layer must be very thin because cold air does not accumulate. There may be turbulence above this thin layer, but this will be dynamical turbulence, with very small temperature fluctuations which cannot give any optical effect.

SIEDENTOPF. — Dr. Rösch, in his excellent summary of what we have been doing during this week, has also raised some points that are necessary for the future work on site testing and seeing in general. I would like to suggest that the most essential of these points be put down in the form of recommendations, and given to the I. A. U. I have tried to formulate again what should be done but it is the same as Dr. Rösch said in his first points :

a. further experimental and theoretical research on the relation between atmospheric phenomena and seeing should be encouraged in cooperative

studies by astronomers and meteorologists; this means the study of the seeing effects of the middle and higher atmosphere;

b. study of the dependance of seeing on height above the ground, on topography and on ground cover, is necessary and should be done by direct and indirect methods; this emphasizes the importance of the ground layer and also goes somewhat in the direction I mentioned on the first day (site improvement).

c. the relation between the aperture average of ray deviation and the time average needs further study; this means the relation between the seeing in large instruments and the seeing in the site testing equipment.

I do not know if it will be necessary to add still something about the need for standard site testing equipment; perhaps it could be included in the point *c* of these recommendations.

RÖSCH. — It would certainly be good to put this before the I. A. U. I hope you let me arrange this by combining what I have said and your more condensed wording, in such a form that I. A. U. could take it into consideration.

RICHARDSON. — It has been noted that it is desirable to place the site testing telescope above the ground layer by means of the construction of a very steady tower. I wonder if it might be possible to eliminate the effect of the ground layer instead of raising the telescope above it. This might be done by isolating the column of air above the site testing telescope by means of a flexible plastic sleeve hoisted to seal a hollow tower. One wonders if such a sleeve placed around Dr. Lynds' tower would eliminate the temperature fluctuations and the associated optical turbulence within the tower.

FOURNIER D'ALBE. — Before this meeting comes to an end, I think that Professors Scorer, Pollak and Queney, if they had been there, would have liked to join in thanking the Chairman, Dr. Rösch, for his invitation to this meeting. I am certainly very glad we were able to come here, though I do not know how much the meteorologists helped. I am afraid the tendency of meteorologists has always been, when somebody asks them about some meteorological phenomenon, to say : " Well of course, this is much more complicated than you think ", and finally to leave the inquirer much more confused than he was in the beginning. This arises partly because the atmosphere is the seat of phenomena which never reproduce themselves entirely. It is not cyclic, and nothing repeats itself exactly. That is why it is difficult to make any sort of prediction, as the public knows only too well. This brings me to one point which, I think, was not mentioned by Dr. Rösch this morning. It concerns the frequency and duration of site testing observations. How long should you go on making measurements at a site in order to be satisfied that you

are obtaining data which will be characteristic of future conditions? This is an extremely difficult question to answer, due to the fact that the pattern of optical turbulence at a site will never repeat itself exactly. So, I think it is almost impossible to fix any definite time period. One may say that observations should go until some physical insight is obtained of what is happening and what is determining the seeing conditions. By physical insight, I mean a sort of picture of the airflow and its fluctuation at the site with as much quantitative detail as possible. I do not think that one can go much further than that, and one certainly should not lay down any sort of norm of frequency and time period. I think one should try to understand in general terms what is happening and to arrive at a fairly precise physical description of it.

My second point is of a different order. I think that it would be useful if in the future the proceedings of the Commission on Site Testing of I. A. U. were communicated to the World Meteorological Organization, for the information of its Commission on Aerology and Climatology. The W. M. O. might be able to act as a link with meteorological services in the world and help the astronomers to call officially upon the Meteorological Service in the country which is being surveyed. Thus astronomers would not only be able to meet meteorologists on a personal basis, but the Service might officially take an interest in the problem and put its resources at their disposal.

RÖSCH. — I am very grateful to Dr. Fournier d'Albe for recalling to us this point, which I mentioned in my first introductory paper and which I completely dropped this morning, about how long we must observe in order to make sure that our results are really significant. I think we cannot discuss this here; it is one of the points we must add to the problems to be treated with the meteorologists.

The second point of Dr. Fournier d'Albe is a very useful one also. May I recall to you that Dr. Fournier d'Albe is here for two reasons; first, he is an observer of U.N.E.S.C.O., and second (or first) he is a distinguished meteorologist. So he will be able not only to contribute to our meteorological studies, but also to tell U.N.E.S.C.O. to what extent these problems involve international cooperation, work in various countries, and so on. He suggested a cooperation between the International Astronomical Union and meteorological organizations. But, just as Prof. Oort said a moment ago, I do not think it would be wise to set up a formal committee, since an informal group of people would probably work more efficiently. Thus, we shall take steps to contact two other bodies: the International Association of Meteorology (I. A. M.), one of the Associations forming the I. U. G. G., and the W. M. O. As far as I know there is an agreement between I. A. M. and W. M. O. to divide the work in basic research and applied research, but of course some of the prominent

persons in the field of meteorology are interested both in I. A. M. and W. M. O. I am thinking of Prof. Van Mieghem among others, who now happens to be the Secretary General of I. C. S. U. So we are in an excellent situation to have very important officials interested in our problems.

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After the discussion, Dr. Bowen, on behalf of the participants, expressed their thanks to Drs. Rösch and Righini for their work in the preparation and conduct of the Symposium, as well as for the organisation of social events outside the scientific sessions.

Dr. Rösch, in turn, said how grateful he was for the help received from the Consiglio Nazionale delle Ricerche (especially from Mrs. F. Veglianti and Mr. G. Grazioli.), for the extremely efficient work accomplished by the Secretaries, and, finally, for the contributions of the participants themselves, which made up the interest of the meeting.

