DISCUSSION.

Rösch. — Something was missing in the paper by Prof. Kiepenheuer, but I would like to say that I do not criticize him at all, because I realize how difficult it is to treat the problem otherwise. The point is the standard of quality of a solar image. In the preceding sessions the discussion went in various directions, but we always had a fixed reference point, which is the size of the stellar image; because dealing with a point source, it is easy to say what is the practical size of the image in such conditions, with such a telescope, and so on. Things become much more difficult when you are dealing with the Sun : all along your paper you mentioned good and bad images, sharp or fuzzy, but you never gave us anything in seconds of arc, and from my own experience I understand where the trouble lies. I happen to have looked at nearly a hundred thousand images of the solar granulation on 35 mm film, I can say that after you have found what you would call excellent images in looking at some fifty thousand, you examine the next film and find a new one which is definitely better than any you have yet seen; then you decide that all the first fifty thousand ones are bad. So it is very disappointing to try to set up a standard. However, we are fortunate enough to have high-resolution observations of the Sun with an instrument high enough in the atmosphere to avoid most of the deterioration due to the atmosphere; I mean the photographs obtained by Schwarzschild in the Stratoscope Project. This can provide a standard of definition of sharpness of the solar image to which we can compare the results of ground-based observations. I just want to show you one example, which is a photograph of solar granules obtained at the Pic du Midi (*fig.* 58). It is one of the best we have (not the only one of this quality). I will show you in which way it compares with those obtained in the Stratoscope Project and in which way it is different. The smallest details recorded have a size of approximately one third of a second of arc, which is practically the theoretical resolving power of the objective used (38 cm in diameter). It happens that these photographs ressemble so much those obtained by Schwarzschild that he himself agreed that it is difficult to discriminate which one has been obtained by us and which one by the Stratoscope Project. But, if you make up moving pictures with a number of such photographs obtained in a short interval of time, as we did, you see that the granules are distorted by the atmosphere. This means again that the wave front is approximately flat, but with a tilt changing with time differently from one part to another of the field, and that gives a very spectacular distortion on the film. So we can say that under good conditions we can reach the resolving power of a 38-cm objective. May I mention that our photographs have always been taken early in the morning with the Sun not very high above the horizon; we hope to improve things and work later in the day. Anyhow this, I think, will show you what can be expected from ground-based observations and give you an idea of a standard of resolution for solar images.

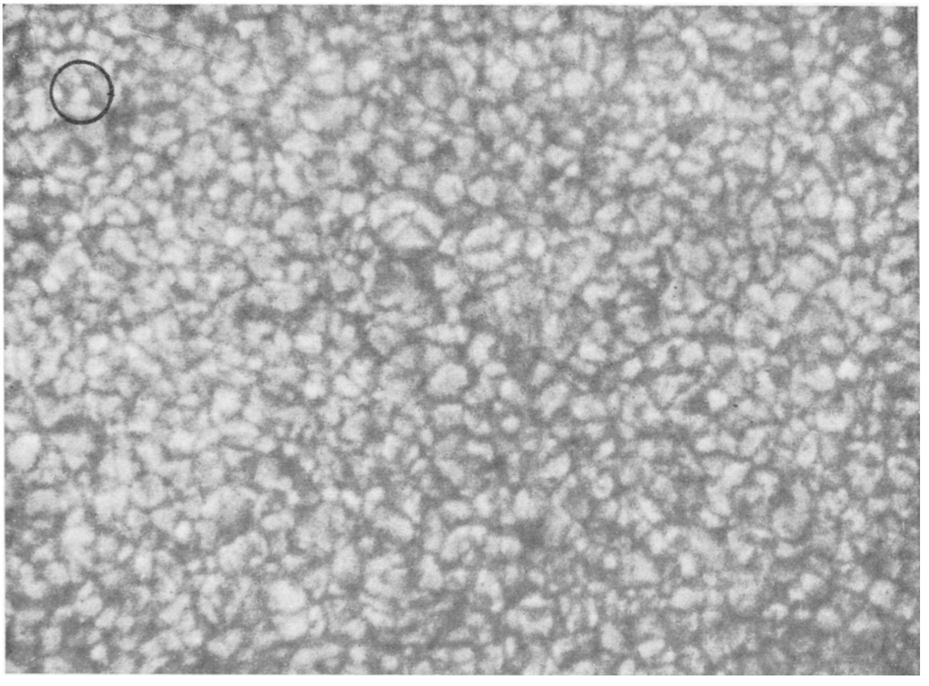

Fig. 58. — Granulation solaire. Objectif de 38 cm.
Pose : 1/580 s. Le diamètre du cercle représente 5″.

Scorer. — It is very difficult to know where to begin because Prof. Kiepenheuer has ranged over a course of lectures in meteorology. Let us consider some of the points. I have first a separate point but I must say it before I get onto the others : in his consideration of areas of sunshine, he stopped at about the southern borders of the Mediterranean area and ignored Sweden altogether. Now it seems to me that in the summer there may be some very good places in central Sweden, in the valleys, and I do know for a fact that secondary scattering is very much less and the air is very much clearer there than in the Mediterranean area.

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KIEPENHEUER. — I agree with you that there is very clear air but there is definitely less sunshine and the Sun will be very low and therefore the image quality will be bad because of that.

SCORER. — Well, then we have to forget everything North of the southern borders of the Mediterranean area. Now, a few things. You said that in the meteorological sunshine records there is no indication of the distribution of sunshine during the day, but this is true only if you look at the statistics written down numerically, not if you look at the original records. From the Campbell-Stokes recorder, for instance, you can get an idea of whether the hours of sunshine are distributed in small quantities throughout the day or whether it is predominantly in the morning or in the evening. But this is not recorded in statistics, you have to examine the original card that was in the instrument.

KIEPENHEUER. — Idid not say that there is not such a record but I said that this record is very complicated. In Freiburg we are getting observations from many European solar observatories, from Sweden down to Africa, and from these observatories we get much detailed information about the sunny hours available, usable for solar work or not. So we put all this together, and we know that things are much better if we do them ourselves.

SCORER. — Well, I see. Now I wish you would stop using this word turbulence because in meteorology turbulence means a type of motion, and there are some kinds of turbulence that produce no refraction problems and other kinds which do. This is particularly true on the lee side of mountains, where some kinds of turbulence are purely motion and will produce no refraction problems; but there may be others which do. You cannot generalize about what the seeing will be like on the lee side of a mountain from observing in one situation.

This really is the main criticism that I would make; you said that you will find this and you will find that, but in fact what you should have said was : I have found this in one particular place.

KIEPENHEUER. — I must add that I talked about the effect of the atmosphere but I did not mention that we have even observed the lee effect of small mountains; we were walking around putting our telescope on the north side and the south side, and even a mountain only 60 m high produced a strong lee effect with moderate wind; so it is quite definite that an observatory never should be in the lee even of a small mountain. I can give you at least a dozen examples of that; it is not just that I did it, but other people did it too.

SCORER. — This is a very important point. It may be true that in many places these fluctuations of the optical image are observed on the lee side of hills, but until we have understood the mechanism whereby this is produced, we cannot generalise. There may be places where the causes that you have observed do not operate.

KIEPENHEUER. — This meeting is, I think, a meeting where we all tell of our experiences in searching for the best site; even if we do not know the meteorological theory of this effect, we should warn others. Maybe I did not say clearly enough that one cannot generalise. What I mean is that we have observed bad disturbances in the lee of mountains and we therefore say " Be careful ! "

SCORER. — Now, I think we are in agreement. We have got this down to " be careful in the lee side of mountains ", not that there should be no observatories there.

Now, on the question of what happens to the energy of sunshine that impinges on the ground. The amount by which the maximum temperature is reduced by the presence of vegetation depends on how much water is evaporated from the vegetation and I think this depends, among other things, on the length of the grass and the nature of the grass. There are some grasses which grow on sandhills for instance which would be bad, but there may be other kinds of grass or vegetation which might be very good.

KIEPENHEUER. — We have made measurements on different types of grass or plants and we came to the conclusion that as soon as there is no water, grasses are very bad.

SCORER. — Yes, this is simply that the energy of sunshine is turned into latent heat.

I would like to mention some very recent work by Monteith, concerning the absorption of radiation and temperatures observed in various kinds of vegetation and various coloured surfaces, in the *Quarterly Journal of the Royal Meteorological Society*; there has been a series of papers over the last three or four years. He used thermometers and hygrometers among the vegetation; this may be extremely helpful in getting to understand the processes that are going on in different kinds of grass and that sort of things.

Now on the question of sea breezes. If you go to a place where the sea breezes are very extensive, for instance on the land coasts of the Mediterranean (not small islands) then you find that they quite often carry considerable haze inland. This is certainly true in times of light wind : the sea breezes are well developed when there is a stable layer a few hundred or thousand metres thick, and the area over the sea is very much hazier than the air above this level. This occurs usually after a burst of polar air has come into the Mediterranean and become stagnant there, and very damp. The convection does not rise above an inversion formed by a shallow layer of cold air spilling into the Mediterranean, and the only way to get out of this haziness is to get up above the inversion. You mentioned Gibraltar: this particular climatological station is very badly affected by this haze, and you should not take any Gibraltar figures really because you would get up above this inversion at 300 or 400 m.

You gave figures showing a temperature gradient up to a height of 20 to 30 m and then an abrupt change. This may be true of a number of sites investigated, which may have been limited to a certain type. But quite often this change is not so abrupt; there are cases in which the temperature gradient behaves differently and this regime extends much higher, particularly in some deserts.

You mentioned very briefly rotating currents and thermals coming from the ground. It is when the surface gets very hot that these rotating currents appear; they are in fact dust devils but they do not always contain dust, unless there is sufficient dust on the ground; these are characteristic of flat desert rather than anything else. Now there is a very real distinction, on which I shall concentrate, between convection over a plain and convection up a mountainside, and this distinction depends on the weather situation very largely. In the centre of a high pressure region you might get one regime and in a calm situation without very high pressure you might get the other. But suppose we have a valley and the air stably stratified; then we can draw the surface of constant potential temperature with potential temperature increasing upward (*fig.* 59). If the potential temperature is uniform, then the air is neutral. If it is stable then we might have the values indicated in a typical valley 1000 m deep. During the day the Sun heats the ground, the slopes are therefore warmed and so these potential temperature surfaces become sloped as indicated by the dashed lines and the gradient

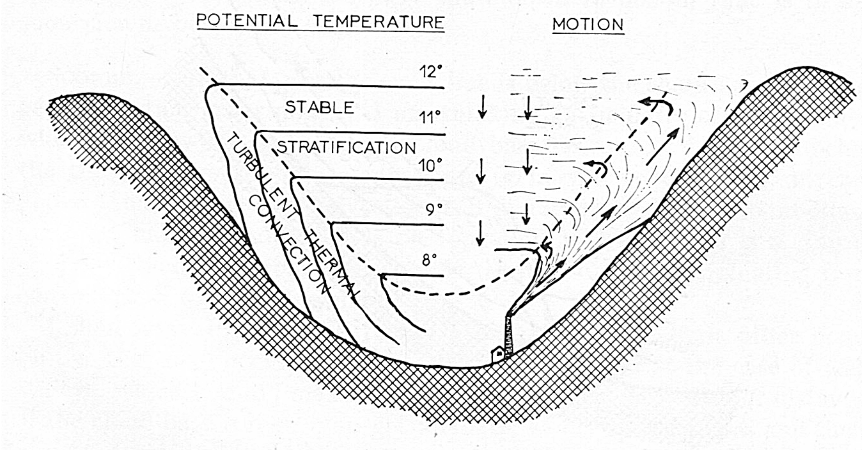

Fig. 59. — **Stratification and anabatic wind in a valley.**

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becomes unstable in a shallow layer whose buoyancy causes it to flow as an upslope wind. This is called an *anabatic wind* (not thermal wind, which is something different). If the air is very stable then there is an outflow at all levels away from the mountain side. One can observe smoke following the tracks indicated by the arrows spreading out from the hillside at all levels. This is one possibility, but it depends on the degree of stability in the main air mass. If it is not stably stratified, the depth of the anabatic wind is very much greater and there may be a wind in the opposite direction in some places and one cannot generalise from Wagner's diagram. I think Prof. Pollak may say something more about this because he has done much more on this than I have. I have been merely concerned with the air pollution case when it is very stable. The direction of the circulation may be very different in the unstable case.

KIEPENHEUER. — We have been observinghere on the terrace (*fig.* 56, 5), the image was excellent, and then we observed at the same time 200 m higher on the same slope, and the image was bad. But the general behaviour was that the morning the images were good everywhere and at all o'clock local time still good on the terrace but became very bad higher up. There is a critical height from which obviously this model doesn't work anymore, this might mean.

SCORER. — No, it does not mean that, it might mean that there is a critical distance from the average slope at which something happens. This is very important, I would say that is much more likely than the other. Maybe if you could stick the thing out in (*a*) you would get better seeing than in (*b*) (*fig.* 60).

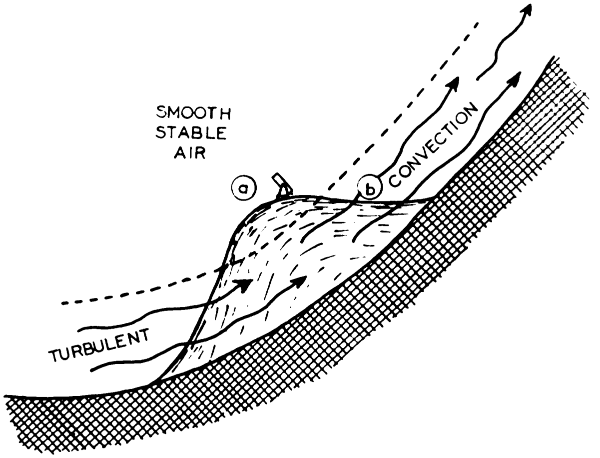

Fig. 60. — Location of an observatory on a promontory.

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KIEPENHEUER. — We have used 30 m high towers on mountains, and there the seeing was extremely bad; we have also the experience of our own observatory in the mountains.

SCORER. — What you should try to do is to get out of these anabatic winds and apparently you succeeded in doing so on this little promontory. Now if you could find a similar promontory higher up, I would say that inevitably it must be better. We should not conclude that such a promontory must be low down because nature has provided you with one low down; if nature provides somebody else with one higher up, they should do better than you.

You also said that the up-currents go up and off the tops of the mountains; this is true sometimes, but I showed some pictures yesterday in which they did not; they only went up to about 8 000 or 9 000 feet and from there upwards the air was extremely stable. This level, if you have an anticyclonic situation, tends to be at the snow line, because the sunshine produces much hotter ground below that. Incidentally, snow is much better than white paint, because snow cannot get above 0°C; if you put more heat into it, it just evaporates and stays cold, whereas white paint can get too hot to walk on. If you have a slope with a snow line, it may be hazy everywhere below, but if you go above the snowline, you may expect even in summer to get very much better seeing, from the point of view of scattering, because you will be above most of the haze on good days.

KIEPENHEUER. — As far as scattering is concerned, this is certainly true; but in our case, for instance, in the Black Forest which is covered with snow for several months, seeing is extremely bad during winter in spite of the fact that we have snow down to 600 m, that is to say some 500 m below the observatory.

SCORER. — In that case the snow line is below the cloud base, and you get convection lower down. I am thinking particularly of the summer, when the snowline is above the cloud base there may be clouds below the snowline produced by convection, or maybe no clouds at all; but above the snowline the convection will definitely be less. Mount-Blanc does not have cumulus clouds on top of it, whereas there is a ring of cumulus clouds all around on summer days, which are originating from convection below the snowline.

I shall leave Prof. Pollak to say a bit more about some other possibilities that may occur. But I would like to point out the case of wide valleys, which you did mention, as there are many in Spain, for instance. If the cloud base is high, you get anabatic winds up the slopes and there is a tendency for a sinking motion to occur in the middle of the valleys counterbalancing the up-currents over the mountains. Now, the air above cloud base is stably stratified and the only convection currents are within the clouds (*fig.* 61); this means that if a sinking motion brings this air down, it produces a stable layer of air over the plain below the condensation level, so that convection currents cannot reach the condensation level; this is why you do not get so many cumulus clouds over the plain as you do over the mountains. If the condensation level is low,

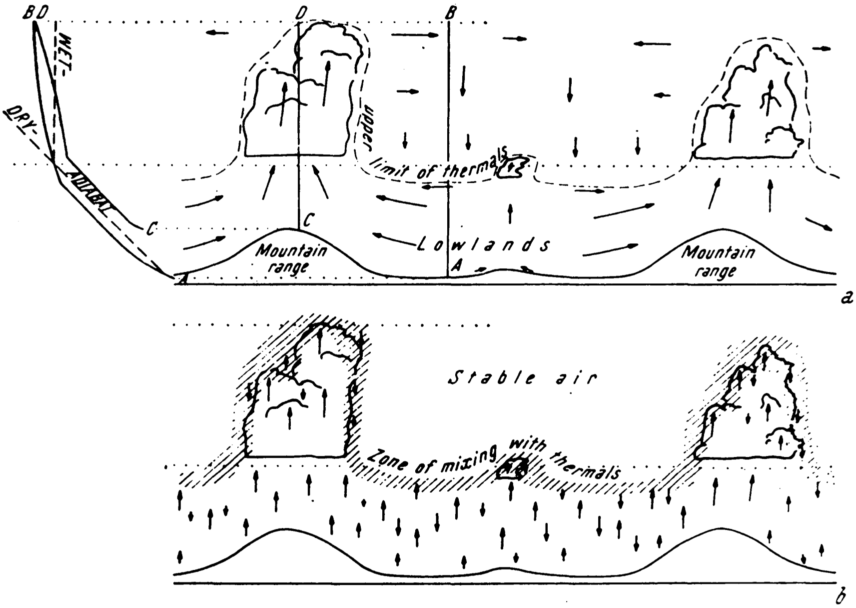

Fig. 61. — Thermals over lowlands and mountains.

a. The circulation during the afternoon showing anabatic flow towards the moutains. The arrows do not show the thermals. The temperatures along AB and CD are shown on the left in relation to the wet and dry adiabatic lapse rates. Except over good thermal sources, such as the small hill shown, the thermals over the lowlands do not ascend enough to form clouds. The clouds are maintained by thermals ascending from the ground.

b. The same time as (*a*) but here only the thermals are indicated by the arrows In the shaded zone the air of the region of thermals mixes with stably stratified air. The thermals provide « viscous » stresses which help to maintain the temperature distribution shown in (*a*).

the effect is very much less noticeable; a little hill may produce sufficiently strong convection currents just to penetrate the stable layer and produce a little cloud, but that is rather rare. In this case, there is convection reaching close to the condensation level even over the plain. This is a situation where the air over the plain is not stably stratified (typical summer air); there is convection but there are no clouds. The absence of clouds does not mean there is no convection. This is what we call a sub-cloud inversion, which occurs over broad valleys. The glider pilots can is fact soar in some of these up-currents but they cannot get higher than the currents, which do not go very high. When you said that if the thermals reach above a certain height, the intensity of the temperature fluctuations is so small that they do not seriously interfere with the seeing, I think you tended to deduce from the good seeing from a promontory that there were not any convection currents reaching its level.

Now, although you get these up-slope winds towards the head of valleys in many parts of the Alps in quiet weather, this should not be taken as a general rule : in canyon-like valleys, particularly in Colorado, the direction is much more determined by a larger scale pressure distribution overhead, and the wind blows along the valley towards low pressure.

Finally I do not think anybody should waste time in looking for turbulence in the sub-tropical jet-stream. We know there is a lot of turbulence in the jet-stream associated with the polar front, but this is a very much more active one. As Prof. Queney mentioned yesterday, there is a great heaving of air going on, with the generation of cyclones, release of rain, and all sorts of things. In sub-tropical jet-streams everything is much quieter and I do not think they should be regarded, at this stage, even as a potential source of disturbance.

KIEPENHEUER. — Correlations have been found between star scintillation and winds at these heights, so it is not impossible to think that there might be an effect of the jet-streams.

SCORER. — What I want to do is to get away from this idea that by reciting the two words *jet-stream* you are on to some clue; it is not as simple as that. These observations may be of something which is due to the local topography more than to anything else, perhaps local high level thunderstorms, or something of that sort, which have been distributing the humidity.

POLLAK. — I wish only to say a few words with respect to the slide of A. Wagner shown by Prof. Kiepenheuer. In this diagram Wagner intended to illustrate the circulation across a valley in which a mountain and valley wind is generated. Wagner's theory was initiated by my observations in the Adige valley near Trento which I made with pilot balloons at day and night and from an aeroplane (in an open cockpit) in 1916 during the first world war. Prof. Kiepenheuer considered here only one half of Wagner's diagram, which should only be applied to the conditions, say, in the plain of the Po-river near the mountains.

In a valley, of course, it is not sufficient to consider for solar observations what is happening on the slope, since at night, e.g. in the Adige valley near Trento, the *whole* valley up to 2 000 m is filled with an air current from the North. When the sun rises, this northerly air stream is replaced by a southerly current starting at the top and gradually working down until, at midday or early is the afternoon, the *whole* valley is filled with a southerly air stream. This process is reversed in late afternoon and night. I think that this change of air current coming at one time from the Alps and at another time from the overheated Po-valley is also important for the quality of the solar image.

My observations (together with a selection of pilot balloon diagrams) are published, I think, in the *Meteorologisches Zeitschrift*, 1920. During the war I published on this subject, for military purposes, under the title " *Temperatur und Böigkeit* (gustiness) *im einer* 4 500 m *hohen Luftsäule im Becken von Trient* (Trento) " a paper (a photocopy of which I can supply on request) which contains mine and my collaborators' measurements in the aeroplane and my discovery of the super-adiabatic lapse rate in the free atmosphere.

Rösch. — I have several questions about what Prof. Scorer has said. Concerning haziness and what you said of valleys in Sweden, I think it is good to mention a point which is accepted by those among the solar astronomers who observe the corona. They say that the polar air is good and that the tropical air is bad, and that if you want to set up a coronagraph you must put it at an elevation which increases with decreased latitude. As a matter of fact, there is a coronagraph operated in Pulkovo, near Leningrad, at sea level, and indeed they get good observations of the green line of the corona. I noticed also that apparently we have put some confusion in the minds of the meteorologists by our use of the word *seeing* in this meeting; it happens that *seeing*, for astronomers, refers to the appearance of the image, regardless of the transparency; haze has nothing to do with it.

There is another point which I think is important, and which I would like Prof. Scorer to make clearer for me, because I am neither a meteorologist nor an aerodynamicist, and maybe his explanation could also help some of you to a better understanding of the facts.

From an aerodynamical point of view, I think I am right in saying that there are two types of flow, laminar flow and turbulent flow. Two years ago, in a Symposium on Aerodynamical Phenomena in the Stellar Atmospheres, we heard very much about turbulence, and I am sure Miss Underhill remembers that an aerodynamicist at that meeting said that there is no intermediate situation : turbulence exists or it does not exist. Well, the second step is : when there is turbulence in an aerodynamical sense, does this turbulence introduce optical effects such as those which we want to avoid ? In other words, is it true that when we have rays travelling through a region where the flow is laminar we have no small scale optical turbulence, and that when we observe through a region where the flow is turbulent it may give small scale deviations of the wave front ? Or not ? This is a very important point for us.

Scorer. — The turbulence, I think, will only produce optical effects of importance to you, if the air that is being stirred has large temperature gradients in it *before* it is disturbed. In the free atmosphere, the vertical temperature gradients are of the order of 5° per kilometre; this means that if you stir that air in order to get temperature fluctuations of the order of 1°, you must stir the air through a depth of at least one-fifth of a kilometre. If you do that, the scale of the turbulence will be too large to affect your seeing, so I would say that in the free air the sort of turbulence that an aeroplane observes in the jet-stream, for instance, which is stirring up stably stratified air, cannot possibly affect a telescope; because when you get down to small-scale turbulence, then the depth of air that is stirred is such that the temperature difference may be only of the order of 1/100th of a degree centigrade.

This is again why humidity is important; because after all the actual stirring motion has died down, there can still remain very large gradients of humidity accompanied by temperature gradients, though there are no density gradients. Let us try to put some figures to this. The density of water vapour is 0.62 times the density of air, approximately; this means that if we take 1 kg of absolutely dry air, and we replace 10 g (this is an extreme case) by water vapour, this will be equivalent to reducing the density of 1 % of the air to two thirds of that it was, so that you alter the density by 0.38 %. If we multiply that by a typical value of the temperature, say 300°K, this gives the corresponding temperature change which must take place simultaneously to keep the density constant. Roughly speaking, this means that by putting 10 g of water vapour into 1 kg of air where there was none before, you change the density by the same amount as that produced by 1°C difference of temperature. Now, this is very very large; we might get something of the order of a fifth or a tenth of this up in the free atmosphere, and perhaps more near the ground sometimes. Anyhow, this is going to be a much bigger effect than the effect of stirring a stably stratified layer. If the air is not stably stratified and it is stirred, then, you get no effect at all. But if it is stably stratified and stirred, then on the scale that you are interested in, the temperature differences can only be of the order, say, of a 1/100th of a degree centigrade. This is less by a factor of 100 than in the extreme case I gave; in a more typical case it would be a factor of 10, which means that the variations in humidity would produce variations in density which are ten times greater than those produced by stirring stable air.

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Gratton. — Do you mean that if you have completely developed turbulence with all the spectrum of bubbles and eddies, then you may have small eddies causing bad seeing, but that if the turbulence is not well developed, then it depends to a very large degree on the humidity ?

Scorer. — It is really a bit more complicated, but it can be clarified in a minute. Suppose that you have a row of posts in front of your observatory and the wind is blowing through them, causing turbulence by stirring the air. This is just what you do in a cup of coffee : you mix the milk and the coffee, and this shows the difference between laminar and turbulent flow; in the case of turbulence, milk or anything else is diffused into the whole volume. Well, these posts will produce a fluctuating flow, so that the relative position of two parcels of air will be modified. If these two parcels of air have the same temperature when they arrive at the posts, then you will get no optical effect downwind. What is going to worry us is the mixing of two parcels of air having different temperatures. Dr. Lynds showed us some very fine examples yesterday of what happens when a parcel of air rises up from the ground. It produces a temperature fluctuation of 1 or 2°, and this is very serious. During the day the air is warmer near the ground than at higher levels, whereas during the night the air is colder near the ground; it is only during the small periods of sunrise and sunset that the temperatures are the same. When you get higher up, the difference in temperature between neighbouring parcels of air is so small that the major effect is produced by the difference in humidity, which can be very large. So that up in the free air we would only expect the turbulence to produce an optical effect if there were, before the turbulence occurred, some large gradients of humidity.

Rösch. — I started this discussion, but I do not regret it at all, and I am very grateful to Prof. Scorer for his contribution. We have not been discussing site testing, but the physics of seeing as Dr. Meinel said. But obviously we have to learn the physics of seeing before site testing; so we have not wasted our time. Now, I just would like to show Dr. Scorer, instead of profiles of mountains, a real mountain and the location of our observatory (*plate* I *b*). It happens that we now have a television tower, 85 m high, on the very top of the mountain, and this aerial view may give you some ideas for experiments that could be conducted there. The mountains to the North are approximately 500 m lower than the Pic du Midi. The bottom of the valley, further North, is about 1000 m below the top; to the South, we have a drop of approximately 500 m down to a saddle, and still lower there is a small lake; the east ridge falls 1200 m and, to the West, there is only a sharp ridge which is the highest spot. The prevailing winds come from West and

South-West; the main chain of the Pyrenees (the Spanish border) lies 25 km to the South, with high mountains 12 km from us.

Just to the East of the Observatory is the closed dome, which I have already mentioned, clear of the slope on almost all sides and completely free in the direction of sunrise, so that it offers good conditions in early morning. Where the very top of the mountain was, there is now the platform where the television building and the 85-metre tower have been erected. I think Dr. Lynds may suggest some experiments, with sensors all around the tower at different levels; we can well have some point sources of light located on the tower and observe them from our telescopes (one of which is a 1 m reflector) through the low layers. Dr. Scorer may find it interesting to make experiments on the flow over the very top of the mountain, in the first 85-metre layer.

Also, we have a platform, or little promontory, 200 m below the top, along the quite regular south slope, where we can well put an equatorial if Dr. Kiepenheuer thinks it would yield some further information along the lines of his own experiments.

KIEPENHEUER. — It would be still more important to put the telescope on the television tower!

RÖSCH. — Indeed, I have this in mind. Unfortunately it is a very elongated steel structure with fairly important flexures.

FRACASTORO. — This I think is a very fundamental question, whether even on a mountain, if you go high enough, maybe 80 m, do you reach a region without optical effects.

RÖSCH. — Yes, I think it is. That is why I am showing this picture, to invite anybody to suggest, before the end of this meeting, experiments which could be made.

SCORER. — We have a television tower in the middle of London on which we have placed thermometers, but we found that they are affected by the radiation from the television. This is something about which you have to be careful, because you may get completely false temperatures.