DISCUSSION.

WILSON. — I would like to discuss briefly a site survey as an optimalization problem.

In selecting the best site for an observatory, it must be recognized that we deal with several different definitions of *best*. The particular choice of *best* adopted depends on the use to which the observatory is to be put. For example, an observatory which is to be used primarily for photoelectric photometry would give greater weight to transparency of the atmosphere than an observatory whose principal programme is lunar and planetary photography, for which the heaviest weight would be given to the seeing. An observatory with a predominantly spectroscopic programme on the other hand would tend to give the greatest weight to the number of clear nights, permitting the seeing and transparency to be of other than the highest quality, etc.

Thus, through the weights given to the various parameters which affect the observations, the total observing condition to be optimized may be formulated in different ways. In general, we may consider an observing function $O(s, t, c, \ldots)$ which depends on seeing, transparency, the number of clear hours and several other meteorological and geographical factors. In site selection practice a function involving several non-meteorological parameters, such as logistics, accessibility, longitude latitude, availability of power, etc. is optimized. But here, let us simplify the problem by considering only the three basic meteorological parameters of seeing, transparency and cloudiness. We may accordingly, take the idealised problem of the optimization of a function such as

$$O = w_s . s + w_t . t + w_c . c$$

where the w's are the weights assigned to each parameter. Each of the observing parameters s, t and c is a function of geographic position and time. In the idealised problem within the area allowed for location of the observatory, values of s, t and c are determined for each geographic parcel on the basis of simple observations made on each parcel extending over a limited time. The site selection is then made through comparisons of the value of O, thus measured for each parcel, using the weights assigned according to the principal uses which the observatory is planned.

This idealised description of a site survey as a multi-valued optimization problem is at variance with the procedures and results of practice. Whereas a value of O similarly determined for every parcel is ideally sought as the basis of selection of the site, in practice the selection is

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made on the basis not of O but of a different function which is defined not by pre-assigned weights but by the procedure used in the survey.

To illustrate this, let us describe a typical procedure used in a site survey in the context of our optimization problem. In starting a site survey, the first step is usually to analyse the available meteorological data concerning precipitation and cloud coverage for the regions under consideration. Those portions of the area under consideration for which the cloud conditions are optimum for observation are then selected for further tests of sky brightness, transparency and seeing. Since haze and transparency conditions seems to be variable over smaller areas than the general cloudiness, the next tests, seeking to find the optimum regions of transparency within the areas of minimum cloudiness, are dispersed to give data with higher geographic resolution over smaller areas. The best parcels for transparency within the previous selected areas of minimum cloudiness are chosen on the basis of the best transparency conditions. The process is iterated further. Since seeing conditions are variable quite locally, the final tests, seeking to find the optimum regions of seeing within the previously optimized parcels, are dispersed with even finer resolution. The final selection is then made from among the " nested " parcels.

This procedure means that instead of determining the best value of O, as defined above, we are determining successively the best value of $O_c = c$, then in the region where O_c is the best, determining the best value of $O_t = t$, then of $O_s = s$. In other words, instead of using fixed weights, the optimization problem is solved by first making $w_c = 1$, $w_t = 0$, $w_s = 0$, then within the regions satisfied by $w_c = 1$, setting successively w_t and $w_s = 1$. This procedure is employed because it is economical, saving on not conducting relatively expensive seeing tests on all parcels. This procedure furthermore leads to a reasonable solution because of the natural scale-wise nesting of meteorological phenomena. Areas with the same average cloudiness are larger than areas with the same average haziness, being in turn greater than areas with the same seeing, etc. However, until the relations between seeing, haziness, and other meteorological parameters are better understood, there is no assurance that the optimum site as originally specified by the use to which the observatory is to be put, can be located by this procedure of " nested tests ".

Recapitulating, the definition of *best* results from the assignment of a set of weights to the different factors affecting observing conditions. The location of a site which fits this definition of *best* must follow from an optimization process in which these weights are held constant. Any procedure which varies the weights in the process of the survey, even though economical, may not necessarily result in finding the best, as specified, site. This aspect of optimization has not been too important in

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the past, but now in competition with and for supplementation of space-based observations, it is a matter of great importance that best sites of various sorts — sites with best seeing, sites with highest number of clear nights, etc. — be determined, and test procedures for locating such sites be perfected. But it is equally important to learn something of seeing conditions in all haze and cloud areas in order to learn more completely the meteorological relationships between these parameters. A knowledge of the dependance of observing parameters would allow the design of site surveys on the basis of the optimization of the function O subject to the constraint $f(s, t, c, \ldots) = 0$, where f describes the dependencies. Such an optimization problem is to be vastly preferred to the one which is at present defined procedurally.

Stock. — I am one per cent in agreement with Dr. Wilson's remarks. You should not try to optimize the various factors in succession and I do not think that any of the three speakers, including myself, have brought this out, although this is a factor that we certainly have taken into consideration. The first thing you start out with, of course, is cloudiness, and you want to bring this at least into a reasonable range. In Chile, we started out in an area near the capital where we had about 150 to 180 clear nights, per year; then we pushed North to gain more clear nights and at the same time we wanted to see what the seeing did. It turned out that the seeing improved when we went to an area where, instead of 150 to 180, we had around 250 nights, so that we were optimizing two factors at the same time and we knew we were going in the proper direction. We are now working in two areas where the number of clear nights is different by about 25 nights per year, the seeing is different by about 30 or 40 %, and the wind is different by about a factor of two. Some factors are in favour of one site, some in favour of the other. So now we have to find out which is the optimum of the two.

Queney. — For five years now we have had a very powerful mean of knowing through the *Tiros Satellites* data the detailed distribution of clouds. In other words, over the larger part of the Earth's surface, we have the distribution of clouds every hour, with details of the order of 5 km. This is true even during night, since by means of infrared records we can get the distribution of temperature at many levels and deduce the distribution of clouds. We may have very accurate information on the distribution of clouds and temperature, so we cannot say that the network of stations is not sufficient to get detailed data on cloudiness and temperature.

Scorer. — I think I should say a little bit about the use of meteorologists. The general attitude seems to be that you should try to get out of the meteorologists what they have in their books or in their heads,

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and if you cannot get anything quickly, then you go away disappointed, disillusioned and perhaps glad to be rid of them. But it is not as simple as that. Meteorologists are able to serve farmers and aviators because they have been working with them for many years. The only way to get the best out of meteorologists is to ask them to come and work with you and make them interested in your problems. A very similar situation existed with glider pilots about 10 or 15 years ago, when very few meteorologists knew anything about gliding. At first they gave rather silly answers to the glider pilots' questions. But by means of cooperation during the last 10 years, the science of soaring in gliders has been advanced enormously, and I think we now have a very much better understanding of the atmosphere in which glider pilots fly than we did before. This is because the two groups of people have been working together. It is not just a matter of going and seeing what the meteorologist has stored away in his archives. In my view you should have a meteorologist on your expeditions — maybe a student who has just got his Ph. D. This is perhaps just the sort of person to have, because he has exploratory mind.

Theoretically it is quite correct, as Dr. Wilson said, to optimize the conditions; but the way one actually does it is by walking around on the mountains and flying around them in an aeroplane. This is, I am sure, what Dr. Stock has been doing. He has been optimizing something in his mind all the time, by using his eyes. If you had a meteorologist with you, I am sure this process could be done more efficiently.

I would like to take up one of Dr. Stock's remarks about the air flow over a mountain top. It is quite correct that if you have a hump in the ground, for instance a building or a dome, then the air flow is, as he said, " compressed ". The pressure is not any greater, it is less if anything, but the streamlines are closer together and the velocity is greater. A small obstacle, and by small I mean something of up to about 100 m height, does have this effect; but this is not true of large mountains. If you have a mountain which is much more than 100 m in height, then a new aspect of the aerodynamics comes in, namely the stable stratification of the atmosphere. The analogy with the aerodynamic flow over small obstacles, such as one may put in a wind tunnel, no longer holds.

Quite a lot of this prospecting has been done in the United States, Great Britain and Israel, for windy sites on mountain tops, for the purpose of placing wind-power electric generators there. It has been found that the air flow over the top of a large mountain is not necessarily any greater than over a small one, or even over the open plain, and certainly not always greater than over the open plain at the same altitude.

The stable stratification of the atmosphere produces a very different pattern of flow. But if you have a large mountain with a small hill

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on top, then you may well find that the wind close to the top of the small hill, say 30 m above it, is much stronger than at the same level 200 or 300 m to one side. I think Dr. Stock's point is this : if you do not make any allowance for this sort of thing, you may find, when you build a larger building than you had for your prospection, that you have a very strong wind flow near the opening of your dome.

STOCK. — I do not disagree with your remarks, but I suspect that this effect will also occur over a large mountain with a long ridge perpendicular to the direction of the wind.

SCORER. — This is not always so. It may be so in some cases but not in others. You cannot make any generalisation about this because it depends on the stratification. This is not a theoretical answer, it is based on surveys made on some mountains where they hoped to get strong winds, and did not.

SIEDENTOPF. — Some time after the first seeing expedition to South Africa, which had observed at many points between the Small Karroo and the Transvaal, the E. S. O. Council resolved to concentrate all further activities in the region of the Great Karroo near 33° Southern latitude. The Karroo lies between the region of tropical summer rain in the North and the region of the subtropical winter rain near the Cape. Therefore the relative number of clear nights is nearly uniform over all seasons amounting to 10-12 photometrically usefull nights per month.

In 1961 a quick-look expedition was sent to the Nieuwe Veld, a mountainous region to the north of Beaufort West with heights up to 2 000 m. It seemed desirable to compare the seeing on several mountains in the Nieuwe Veld and Zeekougat in the Great Karroo, where good seeing conditions had been found earlier. The finally chosen observing stations are marked by crosses in figure 73. The distance between Flathill, in the north of the Nieuwe Veld, and Zeekougat, near the southern border of the Great Karroo, is about 120 km.

The final site-testing programme consisted of seeing observations with Danjon telescopes, observations of extinction with small photoelectric photometers, and meteorological observations (cloudiness, wind, temperature, humidity). It started in May 1961 and will be continued until April 1963. Three teams, each consisting of one observer and one assistant, worked alternatingly at the four positions. For several months an additional observer was occupied with the erection and use of the photoelectric telescopes. The whole work is done under the guidance of Dr. Muller, of Groningen, who also takes care of the reduction work at Groningen.

Beside this site-testing work, two observing groups have done astronomical research work for E. S. O. near Zeekougat in the Karroo and

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on Rockdale Mount in the Nieuwe Veld. The work of the French group from the Observatory of Marseille will better be described by Dr. Courtès, so I report only on the photometric work of the Tübingen group on

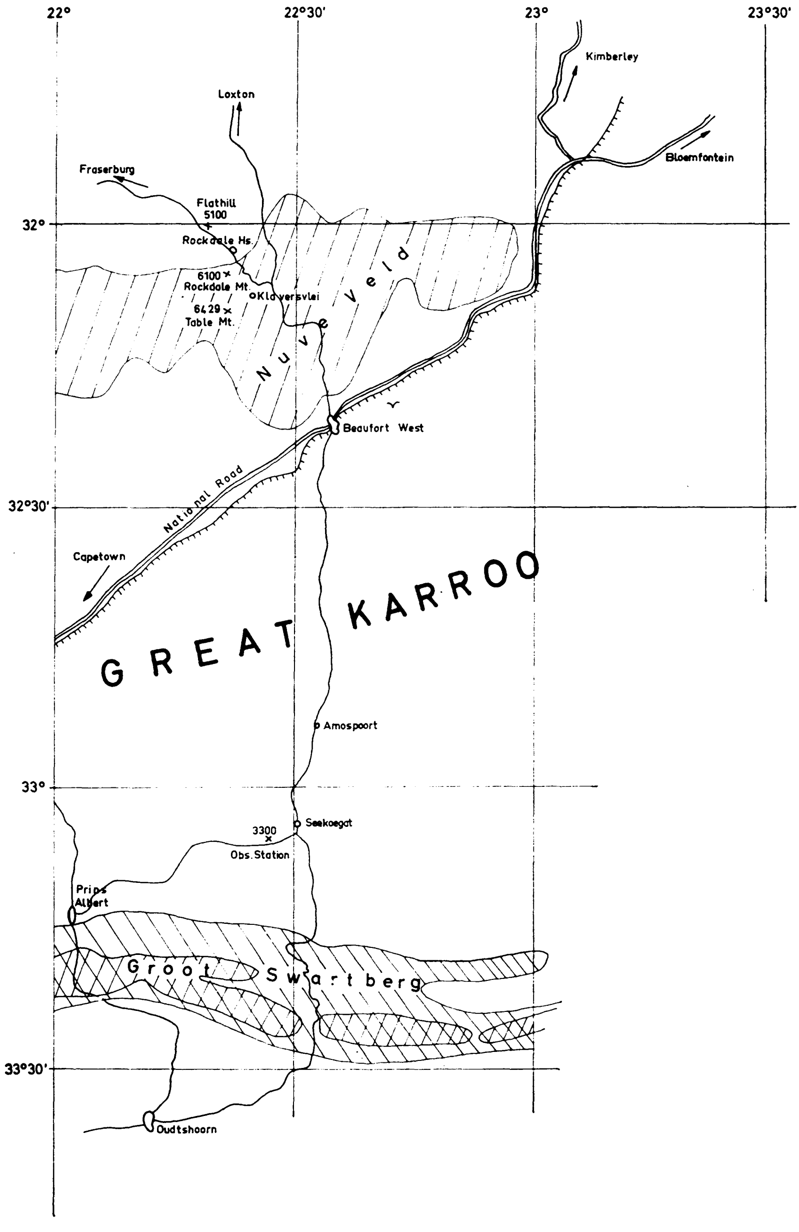

Fig. 73. — Observing stations in Great Karroo and Nieuweveld.

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Rockdale Mount. We installed there in April 1961 a 40 cm Cassegrain telescope in a hut with removable roof. The telescope is equipped with a three-colour pulse-counting photometer. The readings of the counters, together with the observing time and an identification card for each star, are recorded photographically. The programme consisted of early type stars in the Norma and Circinus region, star scales in four Selected Areas at 0° declination on both sides of the Milky Way, and some other selected stars. For comparison stars, objects of the Cape Photometry and Johnson-Morgan standards were used. The work will be finished in November 1962. A second photometric programme, with another photoelectric instrument, is devoted to the surface photometry of the Milky Way in three colors and 1° resolution with a scanning method, and occasional observations of the brightness and polarisation in the zodiacal light, the counter glow, and the Magellanic Clouds. This programme is now finished.

With respect to transparency, the observing conditions were very satisfying, the extinction showing nearly always the values for a pure Rayleigh atmosphere. On the other hand, the observations with the 40-cm telescope were much troubled by strong winds of over 10 m/s and a good deal of observing time was lost. The conditions would of course have been better if the instrument had been in a dome.

The periods of good weather were generally short; only once in the year a sequence of nine consecutive good nights occurred. Usually there were only two to four consecutive good nights before cloudiness returned. On the whole, after one year's experience, we had the impression that Rockdale Mount is not the best site for a big observatory.

Courtès. — I shall give you some information about the results of the French Programme in connection with the problem of seeing. Prof. Fehrenbach has established in Zeekougat an objective-prism of 40 cm and its guiding refracting telescope of 24 cm aperture. You know that for this technique of measuring radial velocities with an objective prism very accurate guiding is needed, and for this reason we have a very steady mounting. This is not an experimental expedition but a real observatory instrument. This mounting is located in a small building like a dome, but limiting observation to 3 h before and 3 h after the meridian. Since the objective-prism requires symmetry of atmospheric dispersion, the work is systematically made between 2 h before and 2 h after the meridian.

When we observe with this guiding-telescope of 24 cm, we can compare the results with those obtained by the Danjon method on the 25-cm telescope used by the seeing expedition of E. S. O. organisation. I spent two months in South Africa this year and the correlation between results of the 25-cm telescope and the guiding telescope is quite good. What

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I can say about the accidental refractions is that the deflection of the star from the crosswire may be of the order of one diameter of the diffraction spot (about 1 second of arc) during approximately 2/10th of the time where the star is behind the wires. The high frequency vibration of the star around the wire is very small, about 0.25″.

I can give you, very briefly, the programme of this instrument. It has been erected to observe the distribution of the radial velocities of stars in the southern Milky Way and also to identify the stars of the Magellanic Clouds. As the stars of the Magellanic Clouds have radial velocities of about + 200 km/s, it is easy to distinguish a star of the cloud from the general background of stars of the Milky Way. The change in the position of the lines is about 100 μ, very easy to detect just by looking at the plates. Of course, we have a programme of spectral classification too. For my own work, I have a small telescope of 10-inch diameter to measure, by Perot and Fabry rings, the radial velocities of H II regions in the Southern Milky Way. In this second programme we cover two degrees of field on each exposure; it will be completed very shortly.

RANDIC. — I wish to ask only one question. How were the parameters mentioned by Dr. Wilson evaluated during the choice of the site of the new big observatory built in Germany, at Tautenburg ?

SIEDENTOPF. — No site testing was made.

LINDEN. — I can tell you briefly about the organization of the site testing done in South Africa. This site testing started at the beginning of 1961 and will last till February 1963, if I am rightly informed. The object is to compare the region behind the Nieuwe Veld mountain range, with the region down on the Great Karroo. For this purpose we had six people to our disposal, that is to say three groups each consisting of two people (one observer and one assistant). Observations are made of the turbulence according to Danjon's method as described by Dr. Dommanget, and also of the extinction. Two new photometers from Germany were used during my time on Rockdale Mountain. I think one of them was moved to Flat Hill later on. Cloudiness observations were made every hour and at two sites we had thermohygrographs to measure the temperature and the humidity. I am sorry that I have not the results here now, but they have been sent to Holland.

RAMBERG. — It is a great pity that we have not seen the Russian astronomers here; it would have been extremely interesting to hear from them. Perhaps Dr. Rösch can give us some information about their work ?

RÖSCH. — I know very little, but I had a talk with Dr. Kutcherov while we were stopped at the Leningrad Airport in December 1960 because the runway was iced and we had to wait several hours. Dr. Kutcherov

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was just leaving for Novosibirsk in order to prepare the whole campaign of the coming summer. I talked with one of his assistants who was also there and who had taken part in some field surveys in Turkestan and the southern borders of Siberia, both in the lowlands and the highlands. As far as I remember, he said that they were organised into groups of some fifteen people in a region something like 100 by 100 km. This group of fifteen included observers and also drivers and other people to help; certainly this could increase the efficiency of the observations; I asked him how many groups of this sort they had in one year to cover the various regions. He said that this depended on the money available but I do not remember the number of groups he told me; maybe some half dozen groups a year, operating in various parts. They use Danjon's method with 10-inch telescopes, small Maksutov reflectors, the advantage being that for their diameter they are very short. They are less than 1 m long and therefore quite light. They are mounted on poles about 2 m high, in order to avoid the ground effects; 2 or 3 m is perhaps not enough, but they are aware that one should put the instrument high enough to try to avoid these effects. This is all I know.

Randič. — Maybe I know something through Dr. Kukarkin that may interest you.

Prof. Kukarkin was in Jugoslavia at the end of June or the beginning of July, and we had a brief talk. I was very interested in the progress of site testing for a big 6-metre telescope which they are hoping to build. I remember that when I was first in the Soviet Union in 1957, they were hoping that they would find the best site somewhere in the south of Central Asia, on the high plateau. They had sent many expeditions there, but when I was in the U. S. S. R. again in 1958, during the I. A. U. meeting, they said they were still looking. Again in 1959, they said it was still not decided where to build. So I was interested to hear about their recent progress, because I had heard that they had really decided to construct. Prof. Kukarkin said that the site is still not chosen, but probably the best site is in Crimea. He said that it is better there than in Caucasus or in Central Asia. It was also his personal opinion that under these seeing conditions, it would be better to have two telescopes of 3 m than one of 6 m. This was just an unofficial talk, but I think it is interesting.

Rösch. — What Dr. Randič has said reminds me that Dr. Kutckerov's assistant told me that generally in the plains or plateaux of South-West Siberia, they found bad conditions. This would explain the fact that they consider putting the big telescope in Crimea, and may be interesting for our general discussion.

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