

## GENERAL SUMMARY AND CONCLUSION

J. RÖSCH.

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It is a great honour and pleasure for me to welcome, at the opening of the last session of this Symposium, the presence among us of Prof. Oort, Retiring President of I. A. U. I would like to thank him for his interest in the problems we are dealing with, and I am confident that our discussions to day will enable him to tell the Executive Committee of the Union that we have usefully spent our time here.

As I told you yesterday, my ambition today is to try to summarize what we have said. I think it is better for me to speak in English for the understanding of most of the audience.

What about the broad lines of this Symposium? At the beginning, I mentioned the aims which could be ours and now my feeling is that we are still very far from achieving these aims. But this is not surprising, after all; I have mentioned the remote aims, and if we have not reached them, at least we are on the way, but on the right way, I hope.

You may remember that in one of my circulars before the Symposium I said that the proceedings of the Symposium should be something like a Guide for Site Testing; my guess was a volume of some 200 pages, a guide for all those who want to undertake a Site Testing programme. After the Symposium, it would probably be more correct to say that the proceedings will cover 300 pages at least, and will be an introductory volume to a series on Site Testing and Physics of the Seeing. I am sure you will agree that such a volume had to be written.

This is because it is the first time, I think, astronomers coming from all parts of the world have met in the same room to discuss the problem of the quality of images. There have been several meetings where this problem has been discussed, but only partly, or too briefly, or with astronomers of certain groups; here we have had for the first time a general meeting of those who have various ideas on this question. Secondly, it was certainly the first time that people met to discuss the problems

of site testing. We have not reached the point where we can make recommendations for a regular procedure for site testing, but at least, we have confronted all the various ways of thinking about this problem. Thirdly, it is definitely the first time that we have brought together astronomers interested in these problems, with, may I say, extremely competent meteorologists coming from many places. And I think it has been a discovery both for astronomers and meteorologists, that they have so much to learn from one another.

One point I regret is that our Russian colleagues did not arrive for this meeting. However, may I say it is less serious than if we were at the point of taking general decisions about normalization; I hope that in the future, when we shall really discuss the problem of adopting regular procedures, they will come in time and work with us.

Now, I shall divide my review into several parts. The first part will be *what are the points about which we have reached a general agreement*. The second will be *what have we still to do*. The third will be *what are the points which have not been sufficiently clarified*, or not at all. Another will be *what are the matters which we have completely forgotten*, or, if not forgotten, *which we did not speak about during the meeting*. And finally I shall consider *what to do now after this Symposium*, what are the decisions to be taken.

First, the points which are now established. The first one is the need for cooperation between astronomers and meteorologists on this problem. About this, you remember that a few days ago, I had asked Dr. Dommanget to get in touch with the meteorologists present here, and to summarize what can be said about the future of the cooperation between astronomers and meteorologists. Dr. Dommanget has been kind enough to write a paper which I shall read in French :

Les astronomes et les météorologistes présents au Symposium sur le Choix des Sites estiment que leurs échanges de vues ont été indispensables. Bien des incompréhensions par manque d'informations réciproques ont été mises en évidence et discutées. D'une part, les météorologistes n'étaient pas suffisamment informés sur les données nécessaires aux astronomes pour procéder au choix d'un site, ainsi que sur les expériences acquises par eux en ce qui concerne l'observation, dans leurs lunettes et télescopes, de certains phénomènes d'origine atmosphérique. D'autre part les astronomes paraissaient ignorer les interprétations que les météorologistes peuvent donner de ces phénomènes ainsi que l'aide que ceux-ci peuvent apporter dans l'établissement de statistiques appropriées relatives aux facteurs météorologiques. En ce qui concerne en particulier l'usage des observations des facteurs météorologiques, faites jusqu'à présent ou à faire dans l'avenir, les astronomes et les météorologistes

s'accordent à dire en ce qui concerne le choix des sites d'observatoires astronomiques :

1° que l'usage des moyennes publiées généralement par les institutions météorologiques nationales et internationales ne peut être fait qu'avec une extrême prudence (sauf peut-être en ce qui concerne les vents aux différentes altitudes), parce que ces moyennes ne répondent généralement pas aux desiderata des astronomes;

2° que les données existantes peuvent peut-être servir toutefois à reconnaître des régions à étudier plus à fond d'un point de vue météorologique;

3° que l'emplacement des stations synoptiques et le rythme avec lequel les observations y sont faites ne sont que rarement conformes à ceux nécessaires pour permettre de caractériser entièrement, du point de vue de l'astronomie, la qualité météorologique des nuits (ou jours) disponibles en un lieu donné;

4° que l'organisation d'une prospection astronomique d'une région doit donc être doublée d'une prospection météorologique en liaison avec les stations synoptiques et climatologiques existant dans la région et que la présence d'un météorologiste dans le personnel affecté à toute prospection pour le choix d'un site d'observatoire est très souhaitable;

5° qu'une collaboration étroite doit être établie dès à présent entre les météorologistes et les astronomes afin d'étudier en général les problèmes posés par le choix des sites d'observatoire, mais aussi afin d'étudier les corrélations susceptibles d'exister entre les situations météorologiques et les conditions d'observations astronomiques faites dans les observatoires existants avec des instruments standardisés. Des périodes d'observation systématique pourraient ainsi être envisagées et les résultats de ces observations, être étudiés en fonction de la situation météorologique à une échelle synoptique (mouvement des masses d'air);

6° qu'il faut créer un groupe de travail dont le but serait de promouvoir une action commune de l'Union Astronomique Internationale et des organismes compétents en matière de météorologie, conformément à ce qui a été dit plus haut.

This is a general statement about the possible cooperation between astronomers and meteorologists but I think we have to take some formal steps in this direction. I shall come back to this point at the end of my talk.

Next among the things which have been established, I think, is the importance of turbulence, this word being taken here as an expression for disturbance in the quality of the images, due to the atmosphere. We have emphasized the importance of this factor as compared to all other meteorological factors. As I said at the beginning of the Symposium, this was already so clear that we had planned to devote one session

especially to these problems and one session to all the other meteorological problems. It turned out that in the first session devoted to such other problems, many people wanted to speak on the next one, so it appears that the proportion is at least  $1/4$  to  $3/4$ , I think, and that this is really the point where we must concentrate. In this field also, we have much to do with the meteorologists.

Then, there is the fact that everybody seems to agree that the use of the wave-front arriving on the objective is a very practical way of expressing both the deviations introduced by the atmosphere, and the appearance and properties of the focal image. There has not always been agreement on this way of looking at things, and, I think it is very satisfactory that almost everybody is now speaking in terms of the wave-front arriving on the objective. This will clarify the discussions very much. I shall come back later to this point about the wave-front in connection with other matters on which the agreement is not so general.

Next is the fact that on various occasions, several speakers have insisted on the importance of making experiments with existing large telescopes. As Dr. Meinel said, this is not *site testing*, but the *physics of the seeing*. However, it is clear that our knowledge of the physics of seeing is so limited, that before good site testing can be done, we must obviously extend this knowledge and to do this, we must use the large telescopes. This point has been already stressed in the report of the Working Group on Site Testing in preceeding years, but I think we must again insist very strongly on it.

The next thing, about which I am sure you have agreed, is the importance of the elevation of the instruments above the ground. We have been given a lot of facts about the turbulent layers at low level, and the importance of having instruments at a certain elevation above the ground. There is something which is not yet clear; at what elevation, and how, is the instrument to be mounted? But the point is that we are sure that something takes place in the low layers, and that it is important both for the instruments used during site testing and for the location of the final telescope.

Unfortunately, these are the only points about which my feeling is that everybody agrees. On all the remaining questions, we have still to do something. On some points we know what we have to do; for instance, we know we have to develop relations with the meteorologists. On some others, we do not yet see our way.

We have certainly to develop new experiments and new types of instruments. Among these experiments which have to be conducted I think the most important will be what I may call the Lynds-type experiments, I mean measurements of temperature changes as a function of elevation above the ground. As Dr. Lynds told us, they have not yet

given their full results, for we still need experiments showing at the same time changes in temperature, and the optical effects; this has so far not been done. This is a very important matter, which brings me back to the point which I raised in my first introduction and which Dr. Bowen discussed, namely the use of direct or indirect methods. I said then that I was in favour of direct methods, as long as the correlation between the direct phenomenon contemplated and the indirect phenomenon observed is not definitely established. The situation now is that these Lynds-type experiments are extremely promising, but we must complete them with optical experiments before we can decide between direct and indirect measurements. These experiments could give valuable information about the thickness and structure of the turbulence of the layers near the ground. They have of course to be conducted not only at one elevation but at different levels.

Another type of experiment which could be made is the aerodynamical one. I have shown some examples for domes. Prof. Scorer criticised them, not because the results were not convincing, but because they could be known in advance and because he was not satisfied the technique was correct. Prof. Pollak told me that he thinks that I am not as wrong as Prof. Scorer said. Prof. Pollak is not here, and so we cannot pursue the discussion. But this morning Prof. Scorer suggested to me some experiments in this field, so I think that he is not absolutely sure that such experiments are useless. Maybe their results are not as direct as in the Lynds-type experiments, but they should be continued nevertheless.

We also know that we have to develop certain new types of instruments. We have had a number of suggestions which have been presented in the session of yesterday afternoon. These suggestions have been summarized by Dr. Wilson in a paper which I shall read now.

*Suggestions made during discussions of Normalization Problems.*

MEINEL :

Instruments for site surveys be considered in three classes :

III. Small portable visual instruments for reconnaissance.

II. Multibeam impersonal instruments.

I. Larger wind-compensated instruments to be located at the height of the final instrument.

For larger telescopes (I), image diameter be measured by image size including 80 % of the energy, integration time  $\geq 1$  s. A frequency function of seeing be given showing the relative times the image is of various sizes and this function be normalized in terms of the percentage of clear nights.

## COURTÈS :

1. Les méthodes de contrôle de la qualité des images doivent fournir une représentation suffisamment bonne des déformations de la surface d'onde  $\Sigma$ . Les images de bonne qualité sont obtenues quand  $\Sigma$  comporte des régions qui peuvent être considérées comme planes sur une étendue suffisante de diamètre moyen  $d$ . Ces éléments pseudo-plans  $\Gamma$  de diamètre  $d$  doivent assurer un pouvoir séparateur de  $0''{,}2$  puisqu'on espère obtenir des images à courte pose grâce aux convertisseurs d'images, ce qui conduit à

$$d \neq 35 \text{ cm.}$$

2. Ces éléments ne doivent pas avoir une structure comportant des ondulations de courte distance de crête compromettant leur planéité, donc le pouvoir séparateur escompté. (Il est à craindre que ce genre de courtes ondulations soit difficile à détecter avec les méthodes de déviation d'image, mais facile par contre à contrôler par l'aspect de la figure de diffraction donnée par un télescope de diamètre  $d$ .)

3. L'inclinaison moyenne des éléments  $\Gamma$  doit être contrôlable.

Les appareils permettant d'évaluer les conditions 1, 2 et 3 peuvent être :

— pour les conditions 1 et 2, réseau de miroirs Hartmann, ou bien méthode de Danjon avec télescope  $d = 35$  cm;

— pour la condition 3, équatorial de diamètre  $d = 35$  cm en station avec possibilité de contrôler les réfractions accidentelles; ou appareil à double faisceau non équatorial (qui ne donne pas la possibilité de contrôler les réfractions accidentelles). Ces instruments du point 3 doivent être érigés à la même hauteur que l'instrument final.

DOMMANGET. — For class III instruments :

1. They be simple, light-weight, readily moved and set up.
2. The method of measurement be rapid.
3. Quick reduction of observations.
4. The instrument be capable of observations at all azimuths.

KIEPENHEUER :

For solar seeing tests :

1. All observations be made at a given height greater than 20 m.
2. Observations not be made in domes.
3. Small instruments be used (10-15 cm).
4. Exposure times be of a few seconds.

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May I add to these suggestions (1) the engineering study of towers for mounting site testing instruments. Although it seems to be a side problem, or a logistical rather than an astronomical one, it would be extremely important to develop parallelogram-type towers, high enough and strong enough, easy to move, easy to set up, because after all the size of the instruments which we can set up on these towers will depend upon their construction.

Then there is something very important which, we know, must be done, as the second step of what I have said in my introduction. It is to study if possible in every existing observatory, the quality of the images as a function of the type of weather occurring and according to the location of the site. I think that we cannot yet draw any general principles regarding the consequences of locating observatories on mountains, on plains, or elsewhere, nor, generally speaking, regarding the effects of what I called the *microclimate* in my general introduction, because microclimate depends on so many parameters. Maybe I am not quite right in meteorological terms, but I hope you understand that in speaking of microclimate, I mean local conditions showing departures from the general situation, such departures being, indeed, dependant on the general situation. We can at least study the observing conditions in existing stations, the microclimate of which we know quite well. I shall come back later to what we can do in this way.

Now there are the problems which, in my opinion, have not been clarified, or are insufficiently clarified; maybe it is only in my mind that they are not clear enough; so if some of you have clearer ideas, please tell me.

First, there is one point about which we have had discussions, or we have heard discussions among the meteorologists. What can we do, we poor astronomers, if the meteorologists do not agree with one another? Again, it concerns turbulence. Which are the conditions for turbulence to give optical effects? This is a very important point for us, at least for our understanding of physics of seeing. We heard about humidity, but the problem is certainly not completely solved. There are other

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(1) Prof. H. von Klüber, prevented by illness from attending this session, has asked the editor to include the following remark, which he would have presented during the discussion: "For site testing, at least in solar work, it seems of importance to collect also information about the probable transmission of the sky. In an otherwise good climate this depends mainly on the distribution of dust and of high thin cirri clouds. A good deal of such disturbances are caused by meteorological conditions over large areas and long periods and the meteorologists, once their attention had been directed to this fact, could probably help a good deal. But for site testing (for solar work) we should also measure quantitatively the sky transparency all the year round. If possible this should be done by a simple device that could be used reliably by some of the existing meteorological stations for some years on a routine basis."

meteorological facts which the meteorologists know quite well but which we have not discussed enough. One of these is the effect of the jet-streams, to which Prof. Queney said we have not given enough time, and I agree with him : one week for this Symposium was too short. We have dropped many things, and that is why we have not given enough attention to the jet-streams.

Another question which I would like to raise is the following : I have spoken about microclimate, but what about large flat countries? It seems that the conditions ought to be well known, because in this case as we are dealing with very large uniform surfaces. On another hand, it has been said yesterday that, apparently, the results of the field surveys of our Russian colleagues in Turkestan and other large flat areas are not very good. So, I ask : why should the conditions be bad in a flat country? Probably the meteorologists will tell us that it is a matter of thermal convection from the ground after heating during the day. The fact is that the conditions are not necessarily good, and I would like to know why not.

Also a point which we have not fully discussed (unless I missed something in the session of yesterday morning), and which was raised by Drs. Courtès or Dommanget some days ago : how can we manage to organize field surveys by steps, using first small portable type III instruments, and then type II or possibly type I? How can we select the regions where we start with type III instruments and then, in the more promising places, set up other types of instruments. This is certainly a point of importance.

Lastly, among the problems which are not completely clarified, there is a terrific one, which is the problem of terminology. We have spoken about site testing and about the physics of seeing, so let us come back to this horrible word "seeing". You know that I find it horrible. I realise that it would be extremely difficult to forbid definitely the use of this word. I agree that in some cases it is practical because it is short. But in my opinion it is bad, not so much in the world itself, but because it has been badly used. You can choose any conventional word to mean something provide you give it a good definition, and do not use it for anything or for several different things. May I make a suggestion; not a formal one, but I think all of you may give consideration to it, make up your mind, and perhaps after some time there will be a general agreement, when everybody has become accustomed to it. "Seeing" is a word which, I think, cannot be assigned a quantity except in an empirical scale. For instance, we know what scintillation is : it is a fluctuation of brightness, and we can define numerically a factor which is the value in percent of the root mean square fluctuation. Image motion can also be defined numerically, in seconds of arc. But "seeing" is not



a quantitative word. It can therefore be used only for something qualitative, which cannot be expressed by *one* number.

Now, let us go back to the wave front. The wave front arriving on the objective is a corrugated surface. Why do we not state this? We have a corrugated surface; by a least square method, we can define the nearest flat plane; this plane is tilted, and the tilt, which changes from one instant to the other, is nothing else than the image motion. Apart from the image motion, the fact that the surface is corrugated results in an image which does not resemble the Airy theoretical diffraction pattern. Let us state that the *seeing* is the fact that the image differs from the Airy pattern.

Then, it is clear that according to such a definition, the seeing depends upon the size of the instrument, because with an instrument of the order of 15 cm, the difference between the focal image and the theoretical diffraction pattern is generally small. If you use a larger instrument, you have a broken ring, and so on, at the same time. Along the same lines, what you estimate in the Danjon scale is a *fraction* of the diameter of the theoretical ring corresponding to the instrument used. It was objected by Meinel that this changes with the size of the instrument; but why not? The image motion also changes with the size of the instrument, but nobody objects to using the words image motion. Everybody knows that with a large instrument we have a very fuzzy image, which does not change too much. It is just the same with scintillation, because we integrate the local fluctuation of energy over a large surface. For a small objective the scintillation is much greater, and this is why scintillation has been well known since early times because you can readily observe it with the naked eye. So that in every language you have the word scintillation, or its equivalent; and since everybody knows what it means, there is no discussion about it. But with the Palomar telescope, the fluctuation of the total energy received on the mirror is so much smaller that, had not the phenomenon been discovered with naked eye, visual observations with this giant instrument would probably have failed to reveal it.

So I do not see any objection in the fact that seeing, as I propose to define it, changes with the size of the objective used. As I said, it is difficult to ban this word seeing, because it is short. For instance, Prof. K. O. Kiepenheuer asked how could we call these towers where he studies the seeing. *Seeing-towers* is very practical; we cannot say the *quality-of-images-towers*; I have another suggestion, which is to call them K. O. K. Towers. But seeing-towers, why not? I do not object. When you look in a telescope, you see an image which has a certain changing shape and a certain random motion; what is most striking is in fact this changing shape of the image; so why not "seeing" for the name of this phenomenon?

But it hurts me when sometimes I read in the literature “there is seeing”, or, “the layer which causes seeing”; what does it mean? What is the type of deterioration of the image? My hope is that if a number of astronomers become accustomed to use the word “seeing” only in the sense I mentioned, maybe after some time things will become clarified, and we may remove the confusion with which we have been faced up to now.

Lastly, there are things which have completely forgotten, or which, although not forgotten, we have not discussed. I guess that the meteorologists will have been surprised that we have not discussed the following point. We have never really stated what it is we want to find. Do we look for a site where we have many hours of excellent conditions, or do we look for a site where we have, sometimes, outstanding conditions? Probably we have not explained sufficiently to the meteorologists the fact that the answer depends upon the type of observations we want to do. Suppose we want to study variable stars; then we need long periods of good conditions, a clear sky but not necessarily perfect stability of the atmosphere, since we are dealing only with photometry. On another hand, if we want to study the planets, we need outstanding definition, for sometimes with a limited period of outstanding definition we can get information which we could never get at another place where there may be 320 clear days per year, but never as perfect for the sharpness of the image. Thus, there is a question of thresholds of disturbance below which one can get such and such information, which one can never get in other places. This is a general point which has not been discussed. And although it is certainly too early to draw a world-map with good regions and bad regions, we must already keep in mind, at least, that there is a difference between the capabilities of places for various types of observations.

Now, what are we to do after this Symposium? Obviously the first thing is to publish our proceedings as soon as possible, hoping that people will read them, try to understand and discuss what we have said. I am pretty sure that these proceedings will start some ideas among astronomers and among meteorologists for new experiments and new research. I plan to mail to all the participants of the Symposium, as soon as it is ready, a mimeographed text of this last session, so as not to delay reflexion about this until after the publication of the Symposium volume. It would be good if, within some weeks, members of the Symposium have already in their hands the points which have been discussed today. As for the publication of the final proceedings, we are taking care of the editing. Within some weeks also, when we have looked into the first drafts, I shall send a circular to those from whom we need further corrections to what has been typed, and a circular to all those from whom we want figures, slides, or blackboard sketches to be

inserted in the proceedings. May I ask you to do your best to send me back all the documents I may request from you at your very earliest convenience in these coming weeks and months, so as not to delay too much the final publication.

Then, you remember that yesterday the question has been raised of a bibliography and I said that this would be extremely useful but that I did not know how to manage it. I have very good news for you now : our colleague Prof. Siedentopf has kindly offered to me yesterday evening to take care of such a bibliography. And, as all of us know the care taken by Prof. Siedentopf in all his work, we may be sure that this bibliography will be an excellent one and we thank him very much for his kind offer.

Let us come back to the other steps we have to take. I mentioned new instruments and new experiments. It was suggested yesterday in the discussion that a pool of instruments could be constituted, in order to have site testing instruments at the disposal of the various people who would like to conduct site testing programmes. This is certainly an excellent idea, although I do not see exactly how it could be worked out practically; we must keep it in mind in the future, but, first of all, we must decide which type of instruments we want. Anyhow, one thing we must do without delay is to insist on having at least the moral support of I. A. U. in conducting new experiments and developing new instruments, because I guess it would help many of us in getting funds from our own countries or institutions if we could say that I. A. U. stresses the importance of such experiments, and of the development of such instruments. And I suggest, in order not to disband without leaving something behind us, that we form a group from this meeting to keep an eye on these matters and to make proposals later on. I do not think we need appoint formally a group of participants, but I would like to suggest somebody responsible for this group : Dr A. G. Wilson seems to be the right man for this, and Dr. Courtès with him; we may keep in touch with one another and see what we can do.

Along parallel lines, we have also the development of studies by the meteorologists. Dr. Scorer is quite willing to undertake experiments and also to foster among meteorologists the idea that not only have the astronomers something to learn from them, but, that, conversely, they have something to learn from the astronomers in the field of this small scale turbulence about which they do not know too much. So it is very fortunate that we have something to give them from one hand while we take something else with the other hand. Dr. Scorer has some useful ideas for promoting the interest of meteorologists in this field; we must take the opportunity offered by this good will, and I suggest that again we set up an informal group with Dr. Dommanget to follow up this part of the problem.

Last but not least, we must emphasize the importance of studies to be conducted in existing observatories. This is really a matter for the I.A.U., at least in principle. Large observatories are not the only one concerned. Some experiments can be conducted with instruments of moderate size. I am sure that all over the world there are many University observatories and others, which have the impression that they have been superseded by the large observatories, and that there is no longer any point in them proceeding with their work because their instruments are too small. I am sure that such instruments, if properly used, could yield valuable information about the physics of seeing and especially about the correlations with local conditions. My feeling is that it is absolutely necessary for I. A. U. to recommend, as we have already said in our report of the Working Group, that experiments be conducted in various observatories in order to learn more about the observing conditions. Of course the ideal would be a world-wide inquiry among existing observatories : When do you observe in good conditions; in which type of meteorological situation; what is the location of your observatory; elevation above the surroundings, lakes, winds, and so on? Prof. Queney said some days ago that he is worried about some meteorological facts concerning the Alger Observatory, but that nobody from that Observatory was here to present his knowledge of astronomical conditions. My reply was that it would have been very difficult to convene representatives of all the observatories in the world. But a general inquiry to get more information in this fundamental question could at least be attempted. This is not site testing, this is preparation for site testing. Or, rather, this is *testing known sites*, as a preparation for *testing unknown sites*.

And this will be my final conclusion. Although we are still in the stage of preparation for site testing rather than that of effective site testing, we are certainly on the road, and I hope it will prove to be a good road in the right direction. Coming back to the story of the tortoise and the hare as I did in the beginning, we, the Earth-based astronomers, may be compared jokingly to the tortoise, while the rocket launching people are just like the hare. But, let us trust in our great fabulist La Fontaine and be confident that we, the tortoises, have a good chance of reaching the goal before the hare does.

