COOPERATION BETWEEN ASTRONOMERS, METEOROLOGISTS AND GEODESISTS FOR SOLVING REFRACTIONAL PROBLEMS

Chairman: G. Teleki

DISCUSSION

G. Teleki: After professor Liljequist's lecture I am more pessimistic than before. I am sorry that Dr Remmer is not here, because this morning he was practically against any measurements. But in any case, professor Liljequist documented very well the importance of a cooperation between the astronomers and the geodesists, as regards the refractional investigations. I suppose that you agree with me. This session is devoted to discussions about the cooperation between astronomers, meteorologists and geodesists in the refractional area. I hope that special questions concerning ice covered surfaces and refraction etc. might be dealt with during informal talks between professor Liljequist and especially professor Angus-Leppan. I suppose we have finished - in the pervious session - the essentials of a future improvement of the cooperation between astronomers and geodesists. Professor Liljequist's contribution today has emphasized the difficulties in treating the refractional problem without careful meteorological information. I agree with him, but I hope that neither astronomers nor geodesists are intending to try to solve the refractional problem without meteorological information, especially in such cases when the atmosphere behaves in a very odd way. Both have always to select observational conditions which are not too far from so-called normal conditions.

B. Garfinkel: What I have in mind, specifically, is a systematic check of the polytropic theory of astronomical refraction, the one which I published in the Astronomical Journal 1967. This theory has not been in much use, because it involves IBM cards and large electronic machines. Now, what I propose to have done, is that one systematically calculates the O minus C residuals for a range of zenith distance from 45° to 90°. The reason for this choice of the range is that below 45° most theories will give the same results, no matter how bad they might be. This has been pointed out by professor Saastamoinen this morning. He said, that all we had to know is the boundary conditions at the observing station and also at infinity, where the refractive index is 1. This is all very

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true, if the zenith distance does not exceed 45°. But what I am interested in is large zenith distances, in particular those going down tc 90°. One can get 90° under certain conditions from ground station. And I am concerned about the observations of cosmic refraction made by astronauts. It is possible to use their results for the purpose as well, because the range there actually extends all the way to 180°, and these results might be used for a check of the consistency of my theory. Now, some details of my proposal. First step: observations. What I propose here is direct measurements of vertical refraction. I am not concerned here with anomalous refraction, it is not possible to fit it with any existing theory. The more I listen to the discussions here about anomalous refraction, the more I have to regard it as a stochastic process, which this kind of theory does not cover. However, the vertical component of refraction can be measured directly, let us say, by taking a star, the declination of which is approximately equal to the latitude of the station. So it goes to the zenith, where the refraction is negligible. So one knows its true coordinates, and then, when it descends to the horizon, one can measure its apparent zenith distance and calculate what the refraction is. That's one approach. Another one is the dispersion method, which was developed or at least proposed here by Dr Currie, as soon as this method becomes available. That is step number one, the observation. Now incidentally, before we go on to step number two, this observation must be correlated with the polytropic index. This parameter of the theory is not a constant, but it varies with the season of the year and possible also with the geographical location. That is the polytropic index, which is related to the temperature gradient. Now, there are no other temperature derivatives invloved, because we assume that temperature is linear in the geopotential. We have to know what the polytropic index is, if we want to have a good calculation for a large zenith distance. Now the sources of information as regards the temperature gradients are several. One can have resort to direct meteorological measurements of vertical temperature gradients by means of balloons or, if that is not available, one can use publications of, let us say, the U S standard atmosphere, that gives the distribution of the gradients at different locations on the earth and different seasons of the year. Or, if that is not easily available, one can use the mean value of this polytropic index, which is taken as 4.256 in the U S standard atmosphere. Now, point number three. With regard to calculations, of course, the electronic machines are necessary, and in the published paper we have a flow chart, but that has been superseded by an improved version, and we also have new sets of the IBM cards available. Those would be furnished on request to anybody who has a sincere interest in checking the theory. Also we will furnish corrected reprints of the paper, with removal of all misprints found so far. Now, this is my proposal. I also have prepared some supporting arguments. Shall I read them too?

G. Teleki: I propose you to discuss these in our working group on astronomical refraction in detail. Have you any proposition how to promote the joint research, the cooperation between the geodesists

and the astronomers? I myself, have proposed the organization of joint meetings in the future too. It is one step. As next step I propose the organization of an interunion commission for studies of refractional and atmospherical influences. Why atmospherical? Because that is connected with lunar ranging. Therefore it is a wider problem. The name of the commission is not so important, but the content of the activity. Have somebody some other proposition or comments connected with this proposition?

E.G. Anderson: Just a comment on the actual terminology: in geodesy there is also work progressing on the gravitational effect of the atmosphere, so if you use "atmospheric influences" you would need to carefully distinguish between refraction and other effects such as gravitation.

G. Teleki: In that case we choose the word refraction.

T.J. Kukkamäki: It would be fine if this proposal could be ready for the Executive Meeting of the International Association of Geodesy, which will be held at September 19-20, so that the Executive Committee can consider this question already there. And then the final establishing from geodesists' side will be ready next year in Canberra.

G. Teleki: Some other propositions or comments, ideas?

K. Poder: We discussed in the resolution committee that such an interunion commission might be supported to take care of all things, and it might be some trouble. I might be too careful or too pessimistic about it. I ask professor Kukkamäki directly, is there any reason for not having this commission? I fear that people will say: now, there is a commission to take care of all these things, and anyone who wants to supply must be backed up by the commission.

T.J. Kukkamäki: From our side it will be fine if it will be possible to consider that next September. This Executive Committee cannot establish this kind of cooperation or commission. That will be made in Canberra next year. But it must be prepared carefully beforehand. This might be a good start.

K. Poder: What I fear is, that if somebody is going to work on these problems later on, he might be unsupported because one would refer to the commission to take care of such research. I don't know, maybe I am too cautious here.

T.J. Kukkamäki: So we are not ready to make this proposition?

K. Poder: Yes, by all means. I was just explaining the reason for my hesitation.

J.A. Hughes: In a brief note which I had from Dr Teleki he made a very good point; that refraction is something which has been investigated

piecemeal in the past with everyone going off in different directions. Sometimes, I think with artificial distinctions, certainly not always. In any event, the opportunity here for a synthesis of ideas would be valuable.

E. Tengström: There exist interunion commissions before, which take wide areas into account, for instance in geodynamics. And there they are working in so-called working groups. Each working group has its special interest, but the common task is in the commission. So I don't think there will be any difficulty to cover all areas and include all interested people in this total research of the commission. Its work also comprises the use of all types of earth-bound observations, and this proposed commission on refraction will be able to carry out research in a much closer collaboration with all interested scientists in the field than has ever been possible before. We will have the opportunity to give one another good advice. So I don't think there will exist any difficulties from any personal directions in this work.

J. Dommanget: I would like to mention that there has been already some collaboration proposed between astronomers and meteorologists in the case of image quality which is directly connected with irregularities in refractive index. The first official such collaboration to my knowledge may be found in the proceedings of the IAU Symposium No 19, held at Rome in October 1962 (Bulletin Astronomique, Paris, 34, 1964, pp. 85-160). See also Transactions of the IAU, XII B, 1964, Commission 9, pp. 133-135. Before writing any proposal, one should especially consider the resolutions given on p. 135 of this reference.