ROUND TABLE DISCUSSION ABOUT THE USE OF DISPERSION METHODS FOR DETERMINING REFRACTIONAL EFFECTS IN ASTRONOMY AND GEODESY

Chairman: E. Tengström

E. Tengström: During the discussion today I like to hear the opinions not only from people dealing with observations of geodetic nature, but also from people who are astronomers or meteorologists, concerning their ideas of eventual applications of the multi-wave method in their investigations. We may start with meteorology. We have unfortunately no pure meteorologists here, but some of you are experienced in what we may call geodetic meteorology, a name for an area, which was introduced already by Brocks, and dealing with wave propagation, turbulence and refraction. The meteorologists have used sound waves to study various problems. Perhaps there may be someone who knows what has been done to study the propagation of sound through the atmosphere, in order to correlate the refraction of these waves with various meteorological models and conditions. As to my knowledge, no attempt has been made to use dispersion for sound waves of various frequencies, up til now. I like to ask some representative here, if they have any experience of success in studying these problems with sound waves. Is there anyone familiar with this area of research? Professor Liljequist and his assistant Dr Israelsson will join us during the next session, when we are going to discuss the cooperation between astronomy, meteorology and geodesy. And they will probably be able to inform you further about any existing contributions from the side of meteorology concerning refractional problems.

So, having no meteorological contribution from the floor at present, I like to direct myself toward the astronomers. At first I would like to address myself especially to Dr Teleki, Dr Hughes and Dr Sugawa, asking them if anything has been done to these problems of eliminating refractional effects, before Dr Currie started his important investigations, which he told us about the other day. Or is this the first time the two-colour principle has been applied in astrometry?

G. Teleki: I have never been informed about any investigations of such kind before. I have only heard about your suggestions to investigate astronomical refraction with multi-wave methods. Yesterday we listened at Dr Currie's interesting paper about realistic research in this field. Before that, no other investigations in astrometry.

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E. Tengström: As I know from Dr Brein, Hertzsprung started to measure directly the atmospheric dispersion at stars of different elevations, by applying an interferometric method, using a diffraction grating in front of the telescope objective. Method and results were published in Astronomische Nachrichten, Vol. 192, 1912, under the title "Photographische Messung der atmosphärischen Dispersion".

G. Teleki: But he did not use the results for astrometry.

E. Tengström: No, but it was the first time an astronomer tried to determine the actual atmospheric dispersion, which could have been used for deriving astronomic refraction. His work might be regarded as the origin of a research into a direct determination of astronomic refraction. The origin of research into geodetic refraction was the work by Näbauer. He was the first one to derive a formula for the relation between refraction and dispersion. His technique, using pointing with alternating read and blue filters, was however too inaccurate.

D.G. Currie: I think there was an intention to use this method at the automatic transit circle of the U S Naval Observatory, for recording in two colours with alternating filters in red and blue.

J.A. Hughes: It has been a practice to use yellow and blue plates in photographic astrometry for purposes other than refraction. I should mention that the automatic transit circle is now back in the factory being reworked because it did not perform satisfactorily. The filters which were, at least provisionally, included in the design were not directed primarily towards refraction. I agree with what you said about Hertzsprung.

E. Tengström: I believe, it is not so important just to give a review of ideas we should already know of, and which are rather new as to their practical applications, but such a review may form a good background for starting to think of some proposals for the future use of this method in astronomy and astrometry. The refraction problem is, of course, very important for our star catalogue work, which we have touched, but also for geodetic astronomy, which may be regarded as a part of astronomy, as it always was. In the case of the two-colour approach, I think, wouldn't it be possible to construct such an instrument, with which we can measure the absolute angular distance between two lines in a star spectrum and compare this measured angle with the angle calculated from the exact properties of the reflecting optics? E.g. having a Rowland grating system, which gives you two indications, one for the atmospherically transferred spectroscopic beam from the star, and the other from the corresponding spectral lines observed by the same instrument, in the laboratory, using the exactly known mathematical properties of the receiving system and earth bound sources. I don't talk about the technical possibilities, but only about principle of application. As we have a round table discussion about the two-colour method, I thought I should also take this idea up. The difficulty, from the theoretical point of view, would be that Edlén's formula, which seems to be slightly better than Barrel and Sears' formula, both used in geodetic work, will not necessarily be applicable to the problems of astronomy because the composition of the atmosphere ought not to be the same as in the laboratory, where the formulas have been derived. I have talked to Dr Bender about this problem, and he was pretty sure that the percent of different gases, even CO_2, was rather constant up to at least 500-600 km. So that the contribution to the total astronomical dispersion coming from this part of the atmosphere should be a very essential part. Is there any objection against my trying to investigate such a method of deriving astronomic refraction? If you are not totally negative, I shall start a cooperation with the people at the Schmidt observatory here to see if we can do something together. Of course, the humidity is again a problem, but the content of water vapor goes rather quickly to zero and its gradient profile would be possible to obtain by meteorological soundings. Unfortunately we can't do as with the distance measurements, eliminating the humidity term by using an additional frequency in the microwave region. The humidity gradient integral cannot be eliminated by using a microwave frequency, because the corresponding dispersive effect for the optical waves and the microwave is too small. In astrometry, by the way, we have probably to stick to optical frequencies only. Now I ask the astronomers if they like us to help them to investigate the possibilities to apply the two-colour method for the determination of the astronomical refraction, or if they think they do not need such a help.

J.A. Hughes: Honestly I must say that astronomers appreciate all the help they can get.

E. Tengström: Also in this particular problem?

J.A. Hughes: Yes. Regarding the humidity; I have taken part in LIDAR measurements which work fine for humidity profiling, so I don't consider that a problem. Well, it might be a financial problem. In any event, since we only have to probe 2 or 3 KM for humidity, a modest laser, say 1 millijoule or so, does it easily. There are radiometric methods as well, so let's assume, for discussion purposes at least, that the humidity is known. I do have a question though. Earlier, Dr Teleki read a Soviet paper having to do with the chromatic effects upon refraction. In the past the refraction formulas, or tables based upon them, have been based upon some particular wavelength. Departures from this wavelength are then tabulated in some way, for instance as in Pulkovo III. It is not clear to me in the case of the multi-wave approach exactly what wavelength one is effectively using. Is it some integrated visual wavelength, or some weighted results which depends upon the details of the dispersion curve? What precisely is it? It could be an important point, or it may not be a problem.

E. Tengström: You know you have one equation for the deviation of one line, another equation for the deviation of the other. In the first you multiply the unknown atmospherical integral with the calculable n-function. If you forget about the humidity, the measured dispersion between the lines is equal to the atmospherical integral times the difference between the n-function values. From this dispersion equation you can calculate this unknown atmospherical integral. With this you can then derive the refraction for any desired wavelength, having its appropriate and calculable n-function. In the case of a star I think the desired one has to be the effective wavelength of the incoming spectrum. The assumption I have made, namely that the atmospherical integral is the same for every considered wavelength, is of course a weak point, but from measurements along our 20 km test line we know that existing refractions of 3' or more in this complicated atmospherical region gives a maximal distance between the UV beam and the red beam of only 0.5 dm. So we are convinced, that in dry air the two atmospherical integrals are identical, especially as the effective total spacing must be much smaller. Perhaps the assumption is more dangerous in astronomical refraction?

J.A. Hughes: Yes, I think it's alright. I just want to make sure that we are coming up with results which are well defined in the sense that they represent a standard. For example, a situation where model number one of a multi-colour device using some particular wavelengths gave you something referred to one thing, and model number two gave something else, would be bad. Apparently that's not true, and I am happy to hear that.

E. Tengström: And you may imagine, that we liked to check the consistency, using various wavelengths. Of course, two colours could be replaced by three or four, and we have done that for three. Mårtensson has proved that even if we have He-Ne red, Argon blue, and He-Cd UV, and we use blue - UV to compute the refraction, or we use the red - UV, we get the same result within the accuracy we have predicted.

D.G. Currie: With regard to Dr Hughes' question, as far as it was presented here for the two-colour refractometer, the wavelengths mentioned are 3400 Å and 6000 Å and considered as the effective wavelength for the two relatively wide band filters. And that is what I have mentioned earlier about the effective wavelength, perhaps changing as you absorbe on the blue edge due to increased zenith distance. The result, which was the number which converted the apparent separation of those two effective wavelengths into what I call the refraction. On the listed equations I had, was the conversion 30.55, which was the conversion to a visual D line, mentioned by professor Tengström, that is to 5600 Å. So the conversion from the effective wavelength, which are instrumental statements in my case because I am not using lasers, I presume to be a reasonable standard, but which perhaps might be discussed of as to the standard used.

E. Tengström: There is another thing also. Even if you don't say that you define the refraction for the D line, that is if you use something in the neighbourhood of that which has to do with our visual of photo visual experience, it does not matter, because the dispersion change is extremely small for small changes of the wavelength.

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J.A. Hughes: Yes, that's true.

G. Teleki: Can I continue this discussion about the red and the blue problem? I asked yesterday Dr Currie how to select the stars for your investigations. Because, for one star you have a very intensive red line and a very weak blue line. Therefore I would like to ask you how this difference between the intensities in the lines influences the refractional determinations?

D.G. Currie: I believe, and this has to be a statement of prediction not measurement, that this will not resolve in a systematic error. It will resolve in a lower signal. Therefore we require a longer integration to a given accuracy. That means, if you look at a M star, you must either look at longer, or except in the standardized programmes, larger error barriers. But in the analysis, nothing has arisen which indicates that there would be a systematic effect, apart from what I mentioned about having to calibrate where the effective center is. For a M star it would not be 6000 Å, it would be 6020 Å. That change you would have to calibrate. And I believe, at that point we will not have a systematic error, or it might be ignored.

J.A. Hughes: I think that out of this discussion I have gotten enough information so that I can phrase my question better. The thing is, I am concerned about whether or not atmospheric attenuation on the one hand, coupled with the spectral type of a star on the other, could possibly give us a zenith distance dependence of the refraction measurement. This is exactly the kind of thing that plagues us now. I am willing to admit it may not be a problem at all, but that is my question, and I think it is being answered.

G. Teleki: It would be very good to have the refractional correction for the moment of observation of the star. Connected with this, I ask for some information about the difference between Tengström's proposed method and Currie's method for the determination of this effect.

E. Tengström: I think, in principle the methods mean the same thing, but with the big resolution we can get from the spectra, and knowing the real wavelengths we can perhaps achieve more. The effective wavelength which is responsible for the position of the exposed image, might easily be determined through photometry of the incoming spectra.

G. Teleki: So there is basically no difference between your idea and Dr Currie's?

E. Tengström: An attempt to increase the accuracy of dispersion by having a very big resolution from the reflecting optics. Then it is not necessary to use filtering separation. The separation is made in the spectrograph, and you measure with the same spectrograph, in our case the Rowland grating, the distance between well defined spectral lines. It is not any interferometer or any special instrument of other type necessary. I think there exist for solar investigations already

Rowland gratings which can be used for our purpose.

B. Garfinkel: I learnt about this two-colour method just a few days ago, and I have only a vague recollection of the principle involved here. Was it not assumed that the refraction in a given wavelength is proportional to the refractive index in that wavelength minus one or something like that? That is certainly true for the first order in the refraction. But there are also higher order terms.

D.G. Currie: That is correct, but the accuracy increase by including the higher order terms is well beyond our needs in the optical region.

E. Tengström: I agree with that. I also like Dr Milewski to say something about the behaviour of the refractive index in different parts of the spectrum, related to the atomic structure of the atmosphere.

J. Milewski: I think that from the physical point of view we are in a favourable position. The region 2000 Å to 7000 Å is without great resonance influences with the atomic and molecular structure of the atmosphere. This is important, because for frequencies very near to the resonance frequencies of atoms and molecules, we have extremely great difference in refractive index. Great molecules have some resonance effect beyond infrared, but also here it is not dangerous because the content of great molecules in the atmosphere is very small, in any case in the used spectral region of the multiwave method. This method is very accurate everywhere. It is a direct method and a very objective one. It is only the question of how good the Edlén's formula is for our purpose, being derived by physicists in the laboratory. I believe that we now know that the formula works with an accuracy of, say 4 parts in 100 millions, and this is in astronomical and geodetic practise a very good accuracy. The multiwave method ought to be a fine and objective method, at least in the optical region. There are however some technical problems and difficulties, but this is another question.

E. Tengström: Thank you Dr Milewski. In the Association of Geodesy we have since long time agreed upon that we need a certain formula for cm-waves and one for the optical region. Your information has now told us where we can be safe. And I believe that not only the geodesists can use Edlén's formula, or Barell and Sears', but also the astronomers when studying refraction by means of the multiwave method in the whole optical region including UV.

J. Milewski: Yes, down to about 2000 Å which is a very deep UV. Dr Currie, do you agree with me?

D.G. Currie: Yes!

G. Teleki: I don't think we can make some definitive conclusions out of this discussion, which is mainly an exchange of informations. I can say, that we from astrometry support this kind of investigation without any further discussion. We expect with great interest the results of these experiments. Therefore we propose this kind of investigations to be put in the resolutions.

E. Tengström: Thank you Dr Teleki. We have previously been talking very much about tilt of the isopycnic layers and its effect on astrometric work, catalogue work and geodetic astronomy. This problem contains the zenith refraction question. I think we can master this question by using two-colour devices as Dr Currie has proposed, or looking at star spectra with high resolution near the zenith. The deviation from the theoretical shape of the layers in the models we use and the real shape would perhaps be possible to study also using greater zenith distances, though that is not yet proved. On the other hand the refraction at any elevation and the tilt near the zenith might be studied also by simultaneous, or almost simultaneous, latitude or longitude observations, carried out separately with transit observations and elevation observations, e.g. in the case of zenith refraction with latitude out of Struve observations and Horrebow-Talcott observations. Dr Teleki, I wrote to you about such attempts we plan to make, and I know that you considered them realistic long time ago. What is your attitude today? Have you done any observations using different methods of latitude and longitude determination to study these problems?

G. Teleki: I do not understand the question.

E. Tengström: I can take an example. You make a latitude observation by means of Struve's method, and you make a latitude observation by means of Horrebow-Talcott's method. In the last case you have a zenith refraction. That is you will have a shifted zenith, and you will have half of the zenith refraction which appears as a term in the difference between the result of Struve and the result of Horrebow-Talcott. There, I mean, we have a possibility to study the zenith refraction. About the attainable accuracy I cannot tell you, but I have a feeling that the internal accuracies of the two methods permit us to solve for an eventual zenith refraction with reasonably small error.

G. Teleki: Perhaps you are thinking of my own and professor Shevarlich's proposition of determining the anomalous refraction in the zenith zone (Publ. Dept. Astron. Belgrade, 3, 1971, pp. 5-16), maybe? Because it is a comparison between Horrebow-Talcott and Struve methods.

E. Tengström: I refer to a letter from me to you, and an answer from you to me. I do not by references know about your investigation. We have been thinking of this ourselves for many years, but we have not yet been able to start any observations along the line. The investigations could be done with longitude observations also.

G. Teleki: You know, we proposed measurements in the zenith zone in the meridian and in the prime vertical for the same star, using Horrebow-Talcott and Struve, respectively. However, with this method we had special problems, especially instrumental problems. You need very good instruments for these observations. It is a basic problem. The second problem is the symmetry. And you have no observation at the same time. You need, but I don't exactly know, something like half an hour for the observation of a complete set, that is from one point to the other. Therefore, separating out from these observations the anomalous refraction is a very problematic question. For this reason I expect a new type of investigation, yours or Currie's investigations, using the same moment of observation. It is very important for us. Because we need a correction just for the moment of observation. It would be very useful - this might be a strange idea - to use the same star for the determination of the refraction as you use to determine the declination.

E. Tengström: Thank you very much for this information about your important work already done to try to solve this problem.

K. Ramsayer: We have developed some devices for automatic star tracking. With them you can measure simultaneously the vertical and horizontal direction to a star. If we observe a set of stars we can compute latitude and longitude from the observed vertical angles alone or from the horizontal angles alone. The results will be different because of the errors of the measurements and the different influence of the vertical and lateral refraction. If there is a difference larger than tolerated we can say we have a refraction anomaly, and we have an indication that the measurements are to be repeated. Some preliminary theoretical investigations of the influence of the tilt of optical layers to the vertical and lateral refraction have shown, that in general both components of refraction have different influences on the simultaneous determination of latitude and longitude, that this influences are proportional to the square of the secant of the zenith distance and that they get smaller the more you come to the zenith. The investigations will be continued.

E. Tengström: Your important investigations of anomalous refraction in the zenith zone have been highly appreciated by us geodesists during many years. You are the first who could talk realistically about amounts of zenith refraction to be expected at sites like yours. You have been optimistic in this respect til now, relying upon your own experiences, but I think that every observer has to make observations by himself to get results which could be applied to that site where he is working, and the time of his observations at that site. Your method, I think, however, must be very valuable to use for all observers concerned, especially because it seems to have solved the simultaneity problem.

J. Dommanget: I think that the position for the astrometrists in general is somewhat different and is fundamentally connected with the field in which they are working and the equipment they have at their disposal. Astronomers are not really interested in the atmospheric refraction itself but only in the way of how to get rid of it. So practically speaking, when the astrometrist is in his observing room, the point for him is to have at his disposal a method of observation (or of reduction) such as to free the observation from the refraction effect. The best way to do this should be of course to use a technique giving, in the case of photographic astrometry for instance, the importance of the refraction all over the plate's field. The measurement of the refraction effect should be made and the photographic plate should be taken simultaneously. This is unfortunately not easy to do at least with the existing equipment: one cannot pursue two programmes simultaneously at the same instrument. It may be different for Dr Teleki when he is observing at the meridian: he may perhaps determine by the same programme of observation, the latitude, the declination of the star and some information on the refraction. So, concerning photographic astrometry, my point of view is that two kinds of observers should be considered. On one side, a very small group which is interested in the study of the refraction phenomena and who should try to fine correlations between refraction effects and atmospheric characteristics for improving our refraction model and our refraction tables. In that respect, the formulation I have proposed for the refracted X and Y coordinates (in my paper presented earlier) may be of some help. The other astrometrist who have to use the improved tables on the basis of any information concerning unfortunately ground level meteorological observations only.

E. Tengström: What you are talking about, as I understand, is the difficulty of having simultaneous information. This is one thing. But when Teleki started to try to improve the refraction tables he was probably not aware of the possibility to determine simultaneous effects. So there is just a way of improving the whole thing. That is, with refraction tables, dynamical atmospheres included, we can nowhere get the correct refraction values. I think the astronomers should be grateful if any instrument could be constructed which eliminates the refraction from the data observed or gives a simultaneous information about it, which can be used for correcting the observed astrometric results. I think we should work on this technical problem. As Hugget has been able to construct a distance meter for refraction, it would perhaps be possible to do something in the astrometric work, photographing the star spectrum for instance at or almost at the same time as we photograph the star. I believe this would not be impossible.

D.G. Currie: For certain areas in astrometry, that is PZT, transit circle and astro geodesy where one observes the star, and may observe it photoelectrically, the ability to do it simultaneously is possible. The brightness of FK 4 permits you to make these measurements. In the case of the PZT, when one extends the observations beyond the FK 4 one has to work with a larger aperture. In order to make an instrument like the two-colour geodimeter, which does both astrometric work and refraction, will be a new astrometric instrument. New astrometric instruments are very expensive. So what we are doing first is to make an observing programme to determine what the time spectrum and the magnitude of the fluctuations are, and how they change across the sky. If we see that the refraction remains constant to a 20th of an arc second - I am talking in extremes now - one would not need to make it at the same time. If it changes rapidly, in a spatial sense, then obviously we have to progress to a joint instrument. This would probably from some initial

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studies be more similar to an astrolabe than a transit circle, because of questions of support of equipment and calibration. You do have enough brightness to look at the refraction, that is the dispersion, at the same time as you look at the positions. The initial programmes with the PZT that we would do will be two separate telescopes, and we would have looked at whether room refraction is bad or not. Another thing is, you have a joint instrument, you don't care about the room refraction, because everything comes through the same telescope. So, yes it is possible, but I hope we will not go into the expense of doing that.

J.A. Hughes: There is one great difference between fundamental astrometric observations and astronomical geodetic observations, and that is: one sets up a transit circle for example, and it stays put, observing on a single program night after night for five or ten years at that spot. So one is immediately concerned with how the refraction is systematically changing. But I think that this precise simultaneous determination is much less necessary than you might think. At least in the transit circle case. What I mean gentlemen, is that when a geodesist is out on a Laplace station he doesn't stay there for five years, I am sure you are much more efficient than that, but the astronomer must stay put if there is ever to be an FK 4 of FK 5. That's the only way it can be done. The point is that the astronomer does integrate and sample over a great many different atmospheric conditions. So the short range of variability does not worry me nearly as much as the isopycnic tilt that sits over your site for five years, at least statistically, and every time you observe it's there, or perhaps it changes annually. That's the kind of thing we are concerned about. The shorter term things, with which the geodesist is very definitely concerned, is not quite the problem in the astrometric case. This is perhaps one small advantage we do have.

E. Tengström: Thank you very much. That was a clear statement of the difference between the geodetic astronomy and astrometric work.

C. Sugawa: At Mizusawa, the international latitude observatory, we are now carrying on simultaneous observations with a visual zenith telescope, a photographic floating zenith telescope, a PZT and a Danjon astrolabe. At almost the same parallel of latitude we are simultaneously observing time and latitude every night. The refractional influences in the free atmosphere should be the same, but the mutual comparison indicates fairly peculiar differences. Dr Currie says that these might be due to different room refraction for each instrument. But for explaining these complicated differences, I think that special experiments must be carried out using various wavelengths.

E. Tengström: Have you not detected any seasonal variation in the zenith tilts?

C. Sugawa: No seasonal variation in the zenith tilts has been found from our observations.

G. Teleki: We permanently discuss about the multiwave observations, but you remember that we have many problems with chromatic refraction. Connected with this it is possible to propose (A. Kubichela suggested this idea) to observe only in one colour, for instance in red, all stars in red colour, or in blue. What will happen in this case with refraction? We can eliminate chromatic refraction, maybe. Maybe because the intensity of the line can influence our observations. Suppose there are no influences of this kind, in this case no chromatic influences. But the normal refraction exists. Anyway, this idea of an one-colour instrument is interesting, and must be investigated.

E. Tengström: I think we have to close this session now. But if there are additional questions about this we can continue during the next session.

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