ON PRECURSORS OF ELECTROMAGNETIC WAVE PROPAGATION

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ABSTRACT. The precursor theory of an electromagnetic wavetrain propagating through a dispersive medium is briefly reviewed. The few successful experimental determinations of these precursors, which have been reported in the literature, are limited to transients in waveguides. The determination of precursors in the optical frequency range appears to be achievable in the laboratory. However, in the geodetic context of long wavepaths through the troposphere the utilisation of precursors for measuring distances and directions free of atmospheric effects is judged as not feasible at the present time.

1. INTRODUCTION

1.1 Preamble

An investigation concerning the state-of-the-art of precursors of electromagnetic wave propagation was instigated by Professor E.Tengström, President of the Special Study Group I:42 of IAG, during a meeting of this group at Wageningen, The Netherlands, in May 1977. The writer was invited to take charge of this investigation. The following note is essentially a compilation of material and information on this subject which the writer has been able to gather since May 1977.

1.2 Precursors in the geodetic context

In 1914, A. Sommerfeld and L. Brillouin showed in two papers that the *front* of an electromagnetic wavetrain propagates in a *dispersive medium* with *velocity* c_0 , the velocity of light in vacuum [a translation of both papers is contained in the book by Brillouin (1960)]. It is important here to distinguish clearly between phase-, group-, signal- and front-velocity (Sommerfeld, 1954). The arriving front is immediately followed by some very weak oscillations, called precursor or forerunner. Unfortunately the precursor has very small amplitudes and their experimental determination seems to be extremely difficult. However, the utilisation of precursors could be envisaged as an elegant method of measuring

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distances and directions free of atmospheric effects for geodetic purposes.

It appears that in the geodetic literature the first reference to precursors was given by Henriksen (1969), and the relevant paragraph from his paper is quoted here in full length: "A second question, first raised by Dr.H.W. Straub of ESSA, is whether the refraction problem can be eliminated entirely by using ultra-short pulses. A considerable amount of work has been done recently in synthesizing light pulses of less than pico second duration. A pulse 10^{-14} second long in time occupies only 1 micron of length; it contains less than two complete wavelengths of green light. Now it is easy to find from Maxwell's equations, that any electromagnetic wave consists of a precursor at a very low level of intensity, and a later packet of high-energy content. The precursor passes through without refraction, but the high-energy packet (which is what we usually observe) does get refracted. Straub's suggestion was that the ultra-short pulses may force enough energy into the precursor to allow detection of this portion and therefore do away with refraction problems."

From then on the Presidents of the Special Study Groups I-19 and I-23 of IAG encouraged in their reports further investigations of the precursor phenomenon (Denison, 1971; Tengström, 1971; Poder, 1975). However, at least to the writer's knowledge, investigations related to the precursor problem have not been carried out at geodetic organisations.

2. THEORY OF PRECURSORS

The precursor phenomenon occurs when a limited wavetrain or a wave packet propagates through a dispersive medium. In a dispersive medium the phase velocity, v, is a function of the wave's frquency, ω. If the dispersion is normal then an increase in ω leads to a decrease in v, and it can also be shown that for this case the group velocity v_g is less than the phase velocity v. For anomalous dispersion an increase in ω leads to an increase of the phase velocity v.

The original Sommerfeld-Brillouin problem is that of a limited sinusoidal wavetrain, which at the time t=0 is incident upon a boundary plane x=0 of a dispersive medium. The wavetrain propagates into the dispersive medium which extends between x=0 and $x=\infty$. Interest now focuses on the propagation of this transient signal, especially the behaviour of the amplitude and frequency of the wave with time at a certain depth x.

The mathematical treatment of this problem which is essentially the solution of the appropriate Fourier integral is rather demanding. A detailed discussion of the original derivations is given in the books by Sommerfeld (1954), Brillouin (1960) and Stratton (1941). Various asymtotic expansions (saddle-point method and stationary-phase method) have been used for the solution of the Fourier integral (Brillouin, 1960). In connection with these mathematical solutions the papers by Elices and

Garcia-Moliner (1968) and Felson (1969) should also prove useful. The propagation of transient signals in lossless waveguides has received considerable attention in recent years, and Schulz-DuBois (1970) has given a review of previous work. He also has discussed a rigorous theory of waveguide transients (Schulz-DuBois, 1970), and Vogler (1970a) has derived an exact expression for transients in an ideal waveguide using the Fourier integral convolution theorem.

In the following the result of the original development by A. Sommerfeld and L. Brillouin is briefly reviewed in a rather descriptive way. The very front of the sinusoidal wavetrain propagates with velocity c_o into the dispersive medium and reaches x at the time $t=x/c_o$, see Figure 1. This is understandable in that the electrons in the molecules of the medium are to be considered at rest when the wavefront reaches them. The following passage from the book by Sommerfeld (1954, p.117) may further help to explain this point: "This also is made clear by the following consideration: the dispersion electrons are originally at rest (their thermal agitation which is in no way related to the rhythm of the light wave can obviously be disregarded). But according to our theory, refraction and dispersion are due to the induced periodic oscillations of the electrons or ions. Thus, to begin with, the medium is *optically void* like vacuum. The propagation velocity is equal to c_o and the index of refraction, if one still cares to speak of one, is equal to 1."

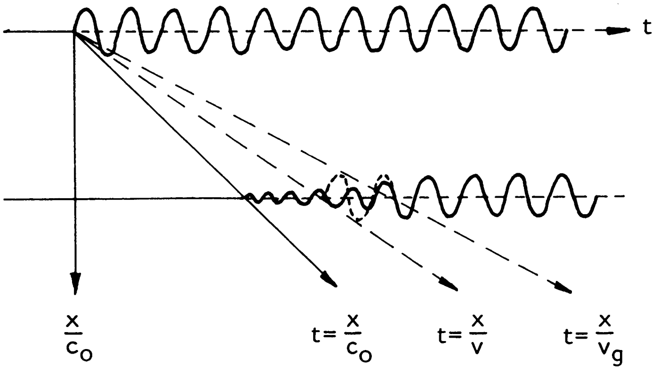

Figure 1: Scheme of propagation of a wave to a depth x in a dispersive medium. Transition of the precursor to the steady state situation. After Sommerfeld (1954).

Immediately behind the wavefront a disturbance arrives which is termed a precursor or first forerunner. The first forerunner arrives with zero amplitude which increases afterwards steadily. The low amplitude of the precursor may be explained by the withdrawal of energy from the incident wave to build up the oscillation of the electrons. The frequency of the precursor is initially very high (but finite) in comparison to that of the signal, and decreases continously. A second forerunner with an initially very low frequency follows the first forerunner and from then on the frequency and amplitude increase toward the steady-state values. An illustration of the forerunners is given in Figure 2.

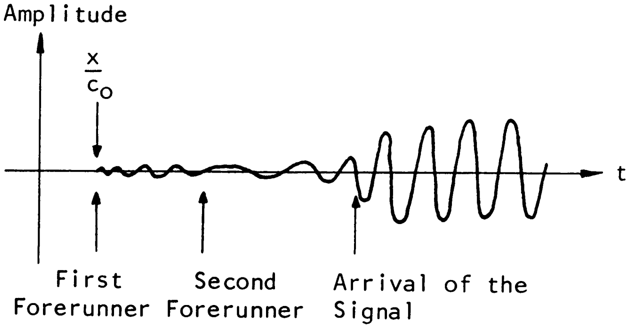

Figure 2: Variation in amplitude and frequency of the precursors, after Brillouin (1960).

The transition between precursor and the steady-state sinusoidal wave occurs rather suddenly but still continously. The time when the amplitude reaches half of the value of the steady-state amplitude defines the signal velocity. The signal velocity and the group velocity are equal but less than the phase velocity for normal dispersion. The incident wavetrain at x=0 reproduces itself identically at the depth x, its phase being merely shifted by (x/v), see Figure 1.

It appears from the literature on electromagnetic wave propagation, that the theory of precursors has been accepted without much doubt since the pioneering work of A. Sommerfeld and L. Brillouin. The only exception seems to be an unpublished paper by Vogler (1970b). He found that in an exact solution for the propagation of a step-modulated sine wave through a lossless waveguide no precursor occurs. However, the precursor appears when the method of stationary phase is used for the solution of this wave propagation problem.

3. EXPERIMENTAL DETERMINATION OF PRECURSORS

Precursors of sound waves and ultrasonic stress waves seem to have been detected experimentally. Brief notes about these experiments have been given by Elices and Garcia-Moliner (1968, p.171) and by Ito (1965).

Very few experimental determinations of transient signals in waveguides have been reported. Saxton and Schmitt (1963) have investigated transient oscillations in a large waveguide, and Ito (1965) has reported about the behaviour of very short pulses in a waveguide. Pleshko and Palócz (1969) have experimentally detected the Sommerfeld-Brillouin precursors in the microwave frequency range. The precursor phenomenon has also been studied experimentally using a travelling-wave maser (E.O. Schulz-DuBois, personal communication, 1978).

No experiments on the determination of precursors in the optical range of electromagnetic wave propagation seem to have been reported in the literature, at least to the writer's knowledge. The suggestion by H.W. Straub to use ultra short pulses of laser light for such experiments (see Section 1.2), does not appear to be associated with actual research in this direction by him or his institution. However, it seems that the recent advent of highly controllable and very short-pulse laser beams should raise hope for the experimental demonstration of precursors at optical wavelengths (A.E. Siegman, personal communication, 1978).

4. TEMPORARY CONCLUSION

The front of an electromagnetic wavetrain propagates through a dispersive medium with light velocity c_o (vacuum). It has been shown in numerous theoretical papers that very weak oscillations, so called precursors, should follow this wave front. The precursor has initially a very small amplitude but a high frequency, and both vary until the steady-state situation is reached. Geodesists have been interested in the precursor concept because its possible technical utilisation would provide an elegant solution to the atmospheric refraction problem of electromagnetic distance measurements and direction observations.

The theoretical prediction of precursors, which started with the pioneering work of A. Sommerfeld and L. Brillouin in the year 1914, has been followed up by a small number of laboratory experiments in which precursors have been successfully detected in waveguides at microwave frequencies. Therefore mathematical methods which do not predict the existence of precursors (Vogler, 1970b) should be carefully examined to resolve that discrepancy between theory and experiment.

It appears that detection of precursors should also be possible with presently available laser instrumentation in the optical frequency range. In designing such experiments the fact that the initial frequency of the precursor is very high (in comparison with the signal frequency), will become important for the selection of an optimal frequency for the transient. Furthermore, the choice of the depth x at which the determination of the precursor phenomenon is attempted, will play a crucial role, because Brillouin (1960) has shown that the amplitude of the precursor decreases exponentially with depth x. This supports the writer's opinion that for *geodetic field measurements* along wavepaths of up to several kilometres through an inhomogeneous and mostly turbulent atmosphere the chances are to be considered rather remote of successfully using precursors of electromagnetic wave propagation for the solution of the geodetic refraction problem.

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DISCUSSION

H. Kahmen: Thank you for this report. Is there anybody who can tell us if there has been any success in using these fore-runners?

J.C. de Munck: I have quite the same ideas as the conclusions by Brunner, more from intuition than from real knowledge. We cannot use them, because they are too small.

H. Kahmen: In Karlsruhe we discussed these problems with some physicists and they also came to this result.

P.V. Angus-Leppan: There has been interest in the subject for a long time. I think we should be grateful to Dr Brunner, that at last someone has taken the trouble to investigate the matter and to give firm conclusions about its usefulness.

H. Kahmen: I think it would be very interesting to remember the report we got yesterday by the professor of meteorology, Gösta Liljequist. He showed us that the curvatures of the electromagnetic waves follow very difficult ways, and he told us that endpoint measurements will not be able to give medium values of refractive indices. Now, we have done much work in improving the measurements at the endpoints. And I think we should discuss about these methods, if there are some possibilities to improve the measurements by observing more meteorological parameters at the endpoint.

T.J. Kukkamäki: I can tell you about some efforts we have made to get some information about the field of the atmosphere between these endpoints. It has been rather expensive, and not possible in the field work. At our 22 km long baseline, we have built two observation towers, 40 m high, at both ends. In these towers we are measuring the temperature and humidity at three different elevations, recording all the time when the measurements are going on. So this kind of measurements are not possible in field work. But we understand, that from these observations we get some knowledge, at least some experience. And we hope, that the extrapolation of our experience from this test field will have some value, at least in a qualitative sense, so that we can avoid some dangerous situations. However, this is for geodimeter measurements. When we have levelled across water, we have tried to determine the temperature field at both ends to see how the light beam is bending just at both ends. That is, of course, the most dangerous areas. So we have put there a set of termistors and determined the temperature field at these short areas. And we have found that, when the temperature on land is higher than on the sea surface, as is usually the case, and when the wind is in the direction from the land, these isotermes are going in one way at one point and in another way at the other, so the situations are different. We have at least been able to estimate, and even to compute, corrections for that. We also tried by running with a boat back and forth, to determine the integrated gradient along the whole length. These are some examples of our attempts to get more information.

H. Kahmen: Thank you, professor Kukkamäki. I think the Australian geodesists tried to find new assumptions by which the mean values of refractive index can be corrected. Professor Angus-Leppan, can you tell us how much the values might be improved by using your additional parameters?

P.V. Angus-Leppan: Yes, though our testing is somewhat limited. In one test we had a number of EDM lines, which were measured throughout 24 hours' period by microwave instruments. They were reduced in the normal way, and then with our particular atmospheric model, the TTM. On most of the lines the standard deviation was improved spectacularly down to less than 50% of the ordinary values. The standard method of reduction gives you a distance which is correlated with the temperature readings,

whereas with the model the errors appear to be random. But on one line the improvement was not very great. It came down to about 80% of the previous values. Similarly, we used the model for trigonometrical heighting over a fairly short line, but one which was grazing all the way, so it was a difficult case. The coefficient of refraction, which has a standard value of 0.13, varied by several units! But the prediction of that coefficient of refraction by our model was down to about 0.1 unit. So that it was able to model a very big variation rather accurately. What really impressed us, was the fact that the model was able to take into account some rapid changes, when shadow came over the line, and the agreement between the observed angles and the predicted ones was very good, even with the sudden changes.
Could I ask a question? One of the very easy things one can do in taking the endpoint measurements in EDM is to put the thermometers high, say 5 m above the surface. I get the impression that the results are not really improved conclusive. I think, that Mayer and Feletschin at Karlsruhe have tried this technique, and it doesn't appear that it improves the results.

H. Kahmen: In Karlsruhe we did many experiments concerning endpoint measurements. There was one other experiment where we measured vertical angles simultaneously with distances, and tried to correct the distances by the vertical angles. We found, that these further measurements cannot improve the results, but they can make the measurements more sure. If you are measuring vertical angles simultaneously, you can find out if the conditions are quite rough or if they are good. But there is no scientifically certain correction to the distances. I think that when we try to find a new method to correct our distances, it must give an error which is not much greater than 0.1 ppm. Only then we can call it a new method. And I think that now such a method could not be found. In England you have got experiences with some two-colour instruments. Are there any results til now?

D.C. Williams: Most of our two-colour angular results have been obtained during cloudy afternoons or evenings, and on every occasion we have found a k-value close to the standard adiabatic value. So the results are not much of interest for the present discussion. We are planning to make some measurements in the early morning, when one expects the k-values to be higher.

H. Kahmen: In Wageningen Mr Hugget showed us for the first time long period measurements with three-colour instruments over lines of 4 km, 6 km, 7 km and 10 km. Every day he made measurements for nearly one year, and he found out that the accuracy of the measurements was 0.1 ppm, when the mean square errors were calculated only for one day. But when he considered the results he got for many days, one could clearly see that the long time accuracy was only 1 ppm. We tried to discuss about the reason for this, but I think there was no quite clear answer then. There may be two reasons. The first reason could be that the atmospheric model is too idealistic, and the second reason may be that the curvature corrections are too rough here.

T.J. Kukkamäki: On this 22 km line I told you about earlier, where we have these meteorological towers, we were able to determine the gradients along the whole line, and also to compute the curvature. Then we observed simultaneously with a theodolite the vertical angles, and they were in very good agreement, not of course to 100%, but somewhat. Unfortunately I cannot remember any exact values to mention here. It was quite clear a diurnal period of this situation. So we have used this experience in our field work, when we do not have these direct gradient measurements. We think that the isothermic layers are following the form of the topography, and so we were able to determine the real temperature along the whole line through some extrapolation.

J. Milewski: I think that the main question of the inaccuracy of 1 ppm is in the geometrical reduction. The data, e.g. illustrated by Mårtensson, give us short periodic variations of the atmosphere, and from his concrete data we see that the short period variations can change the dispersion with very great magnitudes. Mr Mårtensson has detected variations in 20 minutes of 1000 centesimalseconds in refraction. This means, that in a turbulent atmosphere the paths of the rays are stochastic, and short periodic variations for them cannot be accounted for. So, I think that the nature of the atmosphere limits our achievements in this domain, the very short variations of the atmosphere. When they can give such great differences in dispersion, it means that they give great differences in the real geometrical path over the distance. I see this as the main limit of the possibility to obtain the accuracy of distance measurements. And I think that the two-colour method is limited because of the geometrical reduction, which changes rapidly with time as a consequence of the atmospherical turbulence.

H. Kahmen: What do you think of the limit of the scale factor b?

J. Milewski: I cannot answer now. I think, that when we investigate the true data of the real atmosphere, and when we have from the dispersometer such records as these of Williams' instrument, and records of short exposure times by Mårtensson, we can calculate the limit of possibility to reduce geometrically the distance measurements. But the differences in geometrical paths, I think, means the limit of accuracy when determining the true distance. And I think it is possible to calculate approximately this limit.

J. Saastamoinen: In these experiments, was the line very close to the ground?

H. Kahmen: About 10 m high.

J. Saastamoinen: There might be a possibility, when raising the line by about 10 m, to improve the measurements, because then the geometric corrections would smooth out.

J. Milewski: Mårtensson's height above the ground is 30 m. And he has such great variations.

H. Kahmen: There is another question concerning dispersive measurements. Are we quite sure today that the dispersive angle, that we can measure with these new instruments, is an integral over the distance?

J. Milewski: I think that we can be sure that we measure dispersion. The accuracy of the measurements, this is another question. It might be investigated, because we have many sources of possibilities, that the accuracies are not very high, in Williams' device and in Tengström's device, but I think we measure dispersion, that is the integral of the curvatures over the path, assumed identical.

D.C. Williams: When we ask whether we are measuring the dispersion correctly, we are really asking whether the long term averages of the direction of the red light and the direction of the blue light are reliable. They could only be unreliable if there were asymmetry in the turbulence distribution, which is conceivable in the vertical direction. But as far as one can tell, our telescope images and electronic signals appear to be symmetrical, so the average dispersion ought to be correct.

H. Kahmen: Did you find a clear correlation between the refractive angle and the dispersive angle?

D.C. Williams: During a particular set of measurements, we have not seen a significant change either in the refraction angle or in the dispersion angle. The changes between different occasions are also rather small, so we are hoping to make further measurements at times which will give larger refraction, or over a longer distance. Any lack of correlation that we are observing at present is probably due to instrumental imperfections rather than to real effects of the atmosphere.

S-G. Mårtensson: I think that these dispersion methods have to be checked with absolute tests. At the absolute tests we have done in Uppsala so far, only a few of our measurements show that the refraction and the dispersion are correlated. But that might be due to the fact that we are using very short exposure times. If we integrate over long periods we may have a very good value. But that is something we can't say today, because we can't measure with long exposure times because then the accuracy will be very low.

H. Kahmen: Are there any connections between three-colour distance measurements and dispersive angle measurements? I mean, is it possible by these two methods to find out something about the turbulent activities of the atmosphere?

J. Milewski: There are two sources of two colours, the red and the blue. The first problem is that the waves in the atmosphere are not exactly the same. And when one has a turbulence in the atmosphere, the dispersion depends not only on the refraction along the paths, but there are two waves, and we can make such an investigation. But we can also let the waves from two sources agree in the observation point. Then we can investigate, if these waves are closer in the part near the observer, but more separated in the source part. I think it would be good to suggest to the physicists to make laboratory experiments to explain the main question of the correlation between refraction and dispersion. Then we will be able to answer, what is the part of influence of turbulence of the atmosphere, and what is the part of wave propagation of the two colours.

H. Kahmen: But to make such experiments in the laboratory, where we have very short distances, will probably fail.

J. Milewski: No matter. It will be in the medium of controlled air, and means exist to study even extremely small effects, e.g. by the Michelson device introduced by professor Tengström.

H. Kahmen: So you think the atmospherical effects will be detectable in the laboratory, despite their extremely small values?

J. Milewski: Yes, with the great resolving power of the Michelson device, it will certainly be possible. The accuracy of our laboratory measurements of dispersion is something like 0".0005. The base is 30 m long, which means a normal refraction in the laboratory air of 0".1. Using UV and red, which means a magnification factor of about 25, dispersion toward refraction, we can study the non-turbulent situation to within 10% about. We have all of us some difficulties to treat questions about turbulence effects, method of registration of recordings, the accuracies in our instruments, the integration intervals, the quality of recordings etc. I think it would be very good to suggest to the physicists to help us solving these problems for our two- or three-colour methods.

H. Kahmen: I think, at this time we should leave the towers of our instruments and go back into the laboratory. But I think we have to do much work in analyzing the best time averaging procedures in the field. Are there some final results until now about the way such an averaging procedure must be done? It is an open question, and I think we can discuss the results when we have long time series of measurements. Apparently there is a great difference between the short periodic accuracy and the long periodic one. Are there any more results now about long periodic accuracy? Did you measure some long time series of refraction angles? Did you do some measurements during different periods of the year? And did you find there some significant differences in accuracy?

S-G. Mårtensson: Most of the experiments we have done in Uppsala have been done during summer time, for practical reasons. But I can tell that we have never been in the neighbourhood of what we call normal refraction. It seems more like we are measuring in periods where we have two times or even three times the normal refraction. With normal refraction I mean a k-value of 0.13 or 0.14. We have never been in that region. We have maybe selected a very bad base for our refraction studies. I think we have to investigate the base, as well, because most of the times we had two to three times the normal refraction, and even just one week ago we had a k-factor equal to 1.04 at 2 o'clock in the night. For practical reasons we only measure during night-time, and our extreme results might be explained by our existing inversion layers.

H. Kahmen: I heard something about altitude measurements which gave great differences. Is there any explanation for this error?

S-G. Mårtensson: You mean systematic errors between IDM and theodolite?

H. Kahmen: No. I have heard of great systematical effects in your measurements which could not be explained.

S-G. Mårtensson: Yes. Variations by IDM of 1150 centiseconds in twenty minutes of time.

H. Kahmen: I think that all your corrections are based on values which must be quite uncertain at a certain moment. And I have another question regarding the dispersive method. When do you think you will get the best result, when there is a large refraction, or when there is a normal refraction? Are there any results?

S-G. Mårtensson: I think we should get good results whatever the refraction is, hopefully. But maybe it is so, that due to the fact that the two rays, blue and red, travel through different atmospheric media, they are separated by some cm in space at maximum. Maybe the turbulence affects them differently. They are not affected with some correlation, for sure. In that case, of course, small refraction values should give better results, because then the rays are closer to each other and hopefully travel through about the same atmospheric layers. But when we have big refraction, the separation is big and the beams travel in different turbulent media, if looking at micro-turbulence media, not macro.

H. Kahmen: By experiences we have made in Karlsruhe, using both lasers and microwaves for distance measurements, we found that the results with microwaves were usually too small, and the laser results too long. We use formulas for integrating the correct distances. And we have found that these formulae of interpolation are very good if the distances are great, but quite unsure for small distances. Why is it so? All formulas are based upon assumptions and approximations, and therefore it is very important to consider the error propagation with various numbers of legs.

T.J. Kukkamäki: When measuring our 900 km traverse with 25 legs, 30 km long, with geodimeter, we have found that there is some slight systematical difference between the observations before noon and after noon. And we have not been able to find out the reason for that. So, it means there are some systematical errors in these reduction formulas and reduction observations which we have used to reduce our observations to one and the same consistent system. This geodimeter has been calibrated on that 22 km absolute baseline. Observations have been made before noon and after noon, with the same procedure as in the fieldwork, together ten observations in the morning and twenty in the afternoon. In this way we hope that this systematical error is eliminated in the fieldwork and in the results. But there is some slight unexplained difference between morning and afternoon measurements.

H. Kahmen: I think that in Finland you have very good field conditions.

B. Gächter: Is there any dependence of the dispersion from the polarization state of the light, from theoretical point of view or from experiment?

D.C. Williams: It can be said with certainty that the effect of the atmosphere on the state of polarization of light is very small. Some EDM instruments such as the Mekometer exploit this feature. Whether the effect of the state of polarization on the angle of bending is negligible, I am not sure. One guesses that it will be negligible, because the angles involved are so small.

H. Kahmen: I think that we may now conclude from the discussion that there is much fundamental work to be done concerning the dispersion method. One very important thing should be to find out, maybe by theoretical considerations, where there will be a limit for geodetic accuracy, for direction measurements and for distance measurements. What methods do you think could be used, e.g. the one by Tatarski in his "Wave propagation in a turbulent medium"? Have you made some investigations in this respect? I think there should be made some further investigations, so that also one can find the limit of accuracy on the method, theoretically. And I think this may be successful. On the other hand I think we must do much laboratory work concerning two- and three-colour distance measurements and two-colour direction measurements. Will there be some more success in correcting atmospherical models, geometric models? As noone seems to have any further comment about this, I think we can close the workshop now.