F. A. VENING MEINESZ, F. BAESCHLIN et M. HOTINE

NEW FORMULAS FOR SYSTEMS OF DEFLECTIONS OF THE PLUMB-LINE AND LAPLACE'S THEOREM

A la suite de l'article dû au Prof. VENING MEINESZ, *paru dans le* Bulletin Géodésique, *n° 15 (p. 33-42) et intitulé*

New formulas for systems of deflections of the plumb line and Laplace's theorem

la rédaction du Bulletin Géodésique *a reçu du Prof.* BAESCHLIN *la lettre publiée plus loin sous le titre :* Document n° 1.

Le Prof. BAESCHLIN *avait préalablement communiqué sa lettre au Brigadier* M. HOTINE, *en partie pour en avoir une traduction en anglais parfaitement correcte, mais également pour avoir sur les importants problèmes étudiés l'avis de cet éminent géodésien. Avec l'assentiment du Brigadier* M. HOTINE *nous publions plus loin (Document n° 2) les réflexions que lui avaient suggérés et l'article du Prof.* VENING MEINESZ *et la réponse du Prof.* BAESCHLIN.

Enfin nous publions également (Document n° 3) une nouvelle mise au point du Prof. VENING MEINESZ *à qui les deux documents ci-dessus ont été préalablement communiqués.*

Il va sans dire que les éléments essentiels de cette question devront être repris lors de notre Assemblée générale de Bruxelles. L'Association Internationale de Géodésie ne peut que se réjouir de voir se produire de pareils échanges d'idées avant *ses Assemblées générales. Il appartient à ces dernières de rechercher la conclusion à apporter à de tels débats. Mais tous les problèmes posés sont-ils susceptibles d'avoir une conclusion ?...*

P. Tardi.

DOCUMENT N° 1

Note du Professeur Baeschlin au sujet de l'article précité du Professeur Vening-Meinesz

I have studied Prof. VENING MEINESZ' article with great interest. Unfortunately his investigation seems to me to be based on a fundamental error which invalidates his results.

VENING MEINESZ is correct in his assertion that terms containing ξ, η (the components of the deflection) should not enter into the Laplace Equation. In my derivation (contained in the *Bulletin Géodésique* N° 52, 1936) I had overlooked that α_G (the geodetic azimuth) cannot be obtained from observation of a distant object and can only be obtained by computation on the reference spheroid. The right (extra-meridian) arm of the angle α_G is accordingly horizontal and could not be affected by inclination of the vertical.

On the other hand I am unable to accept VENING MEINESZ' contention that HELMERT's formulae for the deflection at a point are falsified through neglecting the effect of a change in the length of the geodesic joining it to the origin. I show below that HELMERT's conception is the only possible one. His formulae are therefore correct. To make the matter clearer, let us consider it further.

The essentials of a triangulation are first the field measures of angles and second its computation on the reference surface, e. g. the spheroid. Usually a triangulation will be computed on a spheroid which rests on a geoidal origin. We call this spheroid or reference ellipsoid R.E. I and the deflection at the origin on it is zero.

If we now introduce components of deflection ξ_0, η_0 at the origin, then the spheroid R.E.I must be moved into a position R.E.II so that the tangent plane at a point P'_0 $(\varphi_0 + \xi_0, \lambda_0 + \eta_0 \sec \varphi_0)$ falls on the tangent plane to the geoid at P_0. The axis of rotation of R.E.II must remain parallel to that of R.E.I after this displacement. Triangulation of the first order should in principle be computed afresh on R.E.II.

To transfer the spheroidal coordinates of a point P_1 in relation to P_0 we have to compute the position of a point P'_1 in the same relation to P'_1. For this purpose we have :

(a) The spheroidal coordinates $\varphi_0 + \xi_0$ and $\lambda_0 + \eta_0 \sec \varphi_0$ of the origin.

(b) The azimuth of the geodesic at P_0 towards P_1 just as it was used for computation of P_1 on R.E.I.

(c) The length of the geodesic P_0P_1 also exactly the same as it was used for computation on R.E.I.

In all rigour the azimuth (b) should be slightly altered because the correction from the normal section to the geodesic is a function of the latitude, which has been changed by ξ_0.

This correction is small and would have no effect, to the usual accuracy of computation, for alterations in latitude up to a sexagesimal minute.

It is only in this that we depart from the field measures of the triangulation; the azimuth α_{01}, and the length S_{01}, of the geodesic are retained as they have been obtained from the results of the field triangulation. It is obvious that the geodesic $P'_0P'_1$ on R.E.II is not the same as the geodesic P_0P_1 on R.E.I. The essential requirement so far as triangulation is concerned is that the two geodesics shall have the same initial azimuths *and the same lengths*. Any alteration of these two elements, which would be opposed to the whole purpose of primary trigonometrical measurement, must be avoided.

Helmert proceeds correctly from these considerations to find the new geodetic coordinates of the triangulation station on R.E.II by using differential formulae which are sufficiently accurate in view of the fact that ξ_0 and η_0 are comparatively small. That he *must* make $\delta S_{01} = 0$ in the process is clear from the above. Helmert's formulae are accordingly correct and those of Vening Meinesz are incorrect.

If we ask ourselves where Vening Meinesz has gone wrong, the answer lies in the fact that he has transferred the results of the triangulation by means of a projection from R.E.I to R.E.II. But the fundamentals of the problem do not anywhere provide a relation between the two in the sense of a projection of P_1 on R.E.I through a transfer of the vertical on to R.E.II.

The conception of such a projection is entirely heuristic and contrary to the fundamentals of the problem; it must therefore lead to erroneous results.

F. Baeschlin.

DOCUMENT N° 2

Lettre du Brigadier M. Hotine (extraits) au Prof. Baeschlin au sujet de la Note précédente

My dear Baeschlin,

..... I think both you and Vening Meinesz are wrong over this question of Laplace azimuths. I agree with you that Vening Meinesz's « projection » has nothing to do with what actually happens in the computation and adjustment of a triangulation and must accordingly give a wrong answer. But I think you also are wrong in holding the length and initial azimuth fixed in all circumstances.

May I briefly go over the ground once again ?

I think we shall agree that a necessary condition for the Laplace Equation to apply at all is parallelism between the minor axis of the spheroid and the axis of rotation of the Earth.

We can ensure this by accepting an astronomical origin for the geodetic starting elements, viz. we make the two meridian planes and the two surface normals parallel, and in addition make the latitudes equal, so that the two axes must be parallel. The two axes need not be *coincident,* except of course in the spherical representation of directions used to obtain the Laplace equation. Consequently, so far as the application of the Laplace equation is concerned at any other pair of corresponding points, the *position* and dimensions of the spheroid are immaterial.

But we also measure a base at or near the origin and reduce its length to mean sea level from spirit-levelling. We assume the result is an actual length on the spheroid. The effect of this is to bring the surfaces of the spheroid and geoid into coincidence at the origin and the spheroid thus becomes fixed in position. It must remain fixed for all triangulations extending from that origin and computed on that spheroid.

Now suppose we depart from the astronomical measures at the origin and introduce, for instance, a prime vertical deflection of η_0 at the origin. We must immediately correct the initial astronomical azimuth by $[\eta_0 \tan \varphi_0]$ before using it as the initial geodetic azimuth. Otherwise the two axes come out of parallelism, the Laplace equation woudl not hold at the origin and could not be applied anywhere else. But we should not alter lengths unless we also postulate moving the two surfaces out of coincidence at the base and then correct the base length to the altered datum level. Having done that we must again hold the spheroid fixed in both position and orientation for all subsequent operations on it.

Another result of introducing deflections at the origin is of course to introduce an effect of geoidal tilt on the angular measures around the origin, and in all probability to increase it elsewhere. This may well be considerable.

All this is straightforward until we measure another base. This ought to be reduced to the level of the (fixed) spheroid by geoidal integration before we use it to adjust the intervening triangulation. But the usual procedure is to reduce it to the local geoid from a spirit-levelled height. This does not bring the two axes out of parallelism and thereby invalidate the theoretical Laplace equation-nothing can do that if the starting elements are correctly chosen. But it will as a rule lead to a fictitious terminal longitude (and probably azimuth as well) and thereby lead to a wrong application of Laplace. In effect, we should no longer be dealing with corresponding points on the two surfaces (within the margin of random error of observation) and it would be idle to talk of deflections between them.

I think the matter should be discussed at Brussels. It is important because much good field work may be ruined in the office by wrong theory. And the fact that such fundamental argument can arise at this stage between two such distinguished geodesists as yourself and VENING MEINESZ certainly indicates that the theory is not entirely above suspicion.

M. Hotine.

DOCUMENT N° 3

Reply to the remarks made by Prof. Baeschlin about my article on Deflections of the Plumb-Line

Grateful for the attention given by Prof. BAESCHLIN and Brigadier HOTINE to my article on Plumb-Line Deflections and for their remarks on that subject, it seems to me that they raise an important question regarding our fundamental concept in Geodesy. It appears to me that our difference of opinion centres in the difference between two concepts about the computation and adjustment of the triangulation on the reference ellipsoid.

We know that the observed triangulation-data have regard to a system of coordinates based on the geoid but as this surface is usually unknown and never easy to use for mathematical deductions, we take instead an ellipsoid which we have reason to assume to be a good approximation to the geoid.

Now two viewpoints are possible. One is to consider the ellipsoid as a so close approximation that we may replace the geoid by it without further requiring attention to possible errors thus incurred. A change to another ellipsoid or to the same ellipsoid in a shifted position (keeping the short axis parallel to its original direction in space) requires a new computation of the triangulation-net which, however, can be done in a simple way by applying a correction to the results of the original computation. The new positions of the triangulation stations are not necessarily in the same verticals as the old positions and in fact will usually deviate from them.

This viewpoint seems to me that which is taken by HELMERT and BAESCHLIN.

The second viewpoint is that we think the geoid as projected on the adopted reference ellipsoid and that we try to compute the thus obtained projection of the triangulation-net. As we know the distance between the geoid and the ellipsoid to be small, we need not accurately to define the way of projection; we can neglect the difference caused by using the vertical on the ellipsoid, the vertical on the geoid of the curve tangent in each of its points to the direction of gravity.

The computation mentioned of the net-projection on the ellipsoid requires of course the projection of the angles and base-lines on it but in many cases we shall not sufficiently know the geoid in our area to do this. We can hardly do else in that case than use the angles and base-lines themselves and in practice this has always been done. We must consider it, however, as necessary to apply at some time, when the geoid has been sufficiently determined, the corrections to our net-figures corresponding to the projection on the ellipsoid of our observed data. A net-adjustment without this complement can indeed not be considered as finished. It is clear that from the adjusted net on the ellipsoid we can derive the positions of the net-stations on the geoid by shifting each station in its vertical towards that surface.

According to this conception the introduction of a new reference-ellipsoid or of a shifted one must mean the projection (1) of each station along this same vertical to the new ellipsoid and this comes of course to the same as the direct projection form the old to the new reference-surface. If at the same time of this change of ellipsoid the adjustment of the net had not yet been completed by the above-mentioned projection-correction, this correction has afterwards to be applied to the nets on both ellipsoids; it is clear that it has then to be deduced with regard to the old ellipsoid. For a change of position of the stations of the net on the geoid as a consequence of the introduction of a new or a shifted ellipsoid as it is brought about when adopting the first mentioned viewpoint, our present conception leaves no place.

For the final result of the net-adjustment we thus have become independent of the choice of the reference-ellipsoid, that is to say that the triangulation-net on the geoid is no longer influenced by the ellipsoid adopted for the deductions. This is not the case if we adhere to the first viewpoint and herein, I think, we may already see a conclusive argument in favour of the second conception; it does not seem acceptable that the final result of our station-net on the geoid would depend on a surface which we only needed for our computations but which has no physical meaning in itself.

A further argument in the same sense may be found in the difficulty we have when adopting the first viewpoint to see the consequences of a shift of the ellipsoid in the direction of the vertical in the central station of our triangulation; such a shift may e. g. be

(1) Here again we need not trouble about the way of projection; the remark made above about this point is also valid here.

necessary if we have to adjust neighbouring triangulations to each other.

Concluding the writer thinks that we cannot come to a clear understanding of the basic problems of the computation and adjustment of our triangulation and levelling-nets without the conception of the projection of the net-stations from the geoid to the reference ellipsoid; an identification of the nets on both surfaces appears to him to be bound to lead to confusion.

F. A. Vening Meinesz.