LETTERS TO THE EDITOR

Über eine neue Deutungsmöglichkeit von Terrella-Experimenten by Horst Hänsel, *Tellus*, **17**, pp. 194–203.

In a recent paper DR. HORST HÄNSEL (1965) has reported on some terrella experiments which he claims are of essentially the same kind as earlier experiments by MALMFORS (1946) and myself (1955, 1958). According to Hänsel the interpretation by Malmfors and me is incorrect, and our experiments have nothing to do with geomagnetic storms and the auroral phenomenon.

The light phenomena around Hänsel's terrella are shown by him to occur predominantly where the electric field lines are nearly parallel to the magnetic field over a large portion of the distance between the terrella and some electrode. There seems to be no doubt that this is true. The same phenomenon has also appeared in my own experiments and they were briefly—apparently too briefly—explained in the same way by me (BLOCK, 1955, Fig. 9 on p. 77 and the corresponding text on p. 73). I have therefore regarded them as uninteresting from a geophysical point of view, and I have refrained to discuss phenomena of this kind which inevitably must occur in model experiments, due to technological imperfections.

Geophysically interesting light phenomena can only conspicuously appear in the experiments if the terrella is covered with fluorescent material. These are the ringshaped auroral zones around the poles, covering *all or almost all longitudes*. From the pictures published by Hänsel (loc. cit.) it is obvious that his terrella was not covered with fluorescent material, because no such auroral zones can be seen in his pictures.

That these auroral zones do not agree with Hänsel's interpretation is perhaps most easily shown by quoting his section (b) on page 202. "Die Zeichenebene der Abbildung 9 ist die einzige Ebene, in der elektrische und magnetische Feldstärke über längere Strecken Parallelitätsbereiche besitzen können. Die Leuchterscheinungen können also nur in dieser Ebene an der Terrella ansetzen."

The mentioned "Zeichenebene der Abbildung 9" is the plane which is described by Hänsel (p. 198) as "die Ebene, die die Elektroden senkrecht schneidet und sowohl Terrellamittelpunkt als auch den Vektor ihres magnetischen Dipolmomentes enthält".

Since the auroral zones occur at *all longitudes* in Malmfors' and my own experiments, and not only in a plane perpendicular to the electrodes, they cannot be explained by Hänsel's theory.

On the other hand, the plasma which is responsible for the auroral zones is to a great extent produced by ionization of neutral atoms by the energetic electrons, causing the light which Hänsel has given a correct but not new interpretation. However, these plasma particles drift in orbits described by Alfvén's theory. This is shown e.g. in Fig. 11 of my 1955 paper (p. 78).

It is most likely that the magnetospheric plasma is not produced in this way. This is certainly one of the imperfections of the so-called visible discharge experiments (BLOCK, 1955) which Hänsel has treated. The invisible discharge (BLOCK, 1955 and 1958) is surely of more geophysical interest. The value of the visible glow discharge experiments—*with fluorescent powder*—is, that they show that Alfvén's mechanism is operating under very general conditions, because the ringshaped auroral zones are produced even in that case.

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Dear Sir:

The following comments are offered in reference to the paper entitled "Approximations in the use of pressure coordinates" by T. M. L. Wigley: *Tellus*, **16** (1964), no. 1, 26–31.

Plane-to-plane transformations from (x,y,z,t) to (x,y,p,t) coordinates are formally equivalent to, although somewhat lengthier than, the standard point-to-point transformation procedure, since the meteorological equations apply to the entire atmosphere and not just to a finite number of selected constant level surfaces. Wigley's analysis is incorrect because he considers the first order terms in the Taylor expansion—the Δz terms—singly and not collectively. For instance, the equation of motion on a constant level surface, $z=Z$, using Wigley's notation, may be written:

$$\left[\frac{DV_h}{Dt}\right]_z + [\hat{k}\,X\,f\,V_h]_z + \left[\frac{1}{\varrho}\nabla_h p\right]_z = 0. \qquad (1)$$

In the plane-to-plane approach, the use of the Taylor expansion to obtain values on a constant pressure surface, where $z=Z_p$, gives to first order:

$$\left[\frac{DV_h}{Dt}\right]_{z_p} + \left[\frac{\partial}{\partial z}\left(\frac{DV_h}{Dt}\right)\right]_{z_p}\Delta z + [\hat{k}X\,f\,V_h]_{z_p} + \left[\frac{\partial}{\partial z}(\hat{k}\,X\,f\,V_h)\right]_{z_p}\Delta z + \left[\frac{1}{\varrho}\nabla_h p\right]_{z_p} + \left[\frac{\partial}{\partial z}\left(\frac{1}{\varrho}\nabla_h p\right)\right]_{z_p}\Delta z = 0. \qquad (2)$$

Wigley argues that the standard point-to-point transformation procedure overlooks the Δz terms; and then he goes on to conclude that this must mean that the standard transformation implicitly assumes these terms to be *individually* zero. That this conclusion is not a valid one, that the Δz terms must be considered collectively, can easily be shown by rewriting equation (2), without carrying out actual transformations:

$$\left[\frac{DV_h}{Dt}\right]_{z_p} + [\hat{k}\,X\,f\,V_h]_{z_p} + \left[\frac{1}{\varrho}\nabla_h p\right]_{z_p} + \left[\frac{\partial}{\partial z}\left\{\frac{DV_h}{Dt} + \hat{k}\,X\,f\,V_h + \frac{1}{\varrho}\nabla_h p\right\}\right]_{z_p}\Delta z = 0. \qquad (3)$$

The "assumption" that the Δz terms collectively are zero implies nothing more than the validity of the equation of motion itself.

Meteorologists can go on using pressure coordinates for their baroclinic models.

Sincerely yours,
DAVID DAVIES,

Central Analysis Office,
Dorval, Canada.

Dear Sir:

I appreciate the interest Dr. Davies has taken in my recent publication (*Tellus*, **16** (1964), no. 1, 26–31). However, from the comments following his equation (2), it unfortunately appears that he has misinterpreted my remarks relating to equation (10) in the above paper. In fact, I have not implied anywhere in my work that the coefficients of the Δz terms have been taken to be individually zero in the work of other authors, but simply that in other works they have either been *neglected* or *overlooked*. By including a Δz term in equation (10), I sought to improve its general use in this field of analysis. I completely agree with Dr. Davies that, in any relevant dynamical equation (Davies considers as an example the equation of motion), the collective coefficient of Δz is, in fact, zero. To put my work into correct perspective in the context of this discussion, I feel that my conclusion regarding the accuracy of the general baroclinic model requires some modification. Thus, while equations (14), (15) and (18) of my paper are, as stated, not exact, one should be careful not to extrapolate this conclusion to the dynamical equations of meteorology, with p(or ξ) as an independent variable.

Yours faithfully,
T. M. L. WIGLEY,

The University of Adelaide,
Australia.