CORRESPONDENCE

Comments on "Pressure-change theory and the daily barometric wave"

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Through the courtesy of Mr. Harris, I have been given the opportunity to read the preceding paper before its publication. I should like to make the following comments.

Like the isallobaric-wind relation after which Mr. Harris' theory of the semi-diurnal pressure variation is modeled, the derivation of his (32), or (8), is based on certain assumptions which may or may not appear plausible, but which can be justified only by a comparison of the resulting final equation with observational evidence. It will be recalled in this connection that there are two theories, by Brunt-Douglas and by Ertel, which give isallobaric winds perpendicular to each other. The observational evidence does not indicate that either of these alternatives occurs more frequently than the other. An empirical study[1] of the terms neglected in the derivation of the isallobaric wind suggests strongly that they are of the same magnitude as the retained terms.

Space does not permit detailed comments on the derivation of (32); but it should be pointed out that a contradiction exists between (21) (where the accelerations of motion are assumed to be due only to the Coriolis force) and (18) (where pressure-gradient forces also appear).

Mr. Harris introduces and justifies (8), or (32), first empirically. Simple considerations are used to establish a proportionality between the pressure deviation from its daily mean and the horizontal Laplacian of the temperature [equation (5)]. The latter, or at least its part depending on the east-west variation of temperature, is assumed to be proportional to the second local derivative of the temperature. In support of (5), it is shown in fig. 1 that the shapes of the curves representing the daily variations of p and $\partial^2 T/\partial t^2$, the latter on an arbitrary scale, are very similar. However, to compute $\partial^2 T/\partial t^2$ only the first two harmonics of the daily variation of T are used, because these are the largest. This is no longer true for $\partial^2 T/\partial t^2$. It can easily be seen that the amplitude ratio of the n'th to the k'th harmonic of $\partial^2 T/\partial t^2$ is $n^2 A_n/k^2 A_k$, if the A's are the amplitudes of the n'th and k'th harmonic of the diurnal variation of T. A casual inspection of a few stations seems to indicate that the ratio $n^2 A_n/A_1$ for the fourth harmonic is about 0.8, so that it would be quite noticeable in fig. 1 and disturb the agreement between temperature and pressure curves. There is, indeed, also a 6-hr pressure wave; but its amplitude is only about 1/40 of that of the semi-diurnal wave. Since for the diurnal temperature variation A_4/A_2 is approximately 1/4, one would expect according to Harris' formula that the amplitude of the 6-hr pressure wave is about equal to that of the semi-diurnal wave.

Mr. Harris computes then, by means of (8), the temperature variation required to produce the observed diurnal and semi-diurnal pressure variations and finds agreement with the observed temperature variation. But in view of the large scatter in figs. 4 and 5, this agreement does not appear very convincing proof of the correctness of (8), especially since the numerical factors in the latitude functions of these figures are empirically chosen. It would also have been interesting if Mr. Harris had used Simpson's[2] analytical expression for the semi-diurnal pressure oscillation rather than an expression based on much less data.

It appears to me that the theoretical foundations of Mr. Harris' argument are questionable, and that the empirical facts presented in support of the theory are unconvincing. The resonance theory, on the other hand, explains why the semi-diurnal pressure oscillation is so much more regularly distributed over the globe than the diurnal oscillation. It also gives, within the framework of the same theory, an explanation of the lunar tide of the atmosphere. Further, recent investigations by Prof. F. Möller and myself (to appear in *Archiv für Meteorologie, Geophysik, und Bioklimatologie*) have shown that it accounts also for the standing semi-diurnal pressure oscillation and for its geographic distribution, which differs very considerably from that of the generating standing temperature wave.

The work of Pekeris and others has shown that the atmosphere, because of its stratification, has a free period in the close vicinity of 12 hr. Nevertheless, the resonance theory is often criticized because of the apparent improbability of this accurate tuning of the atmosphere. This objection has been weakened considerably by a paper by Holmberg,[3] in which it is shown how the apparently fortuitous tuning of the atmosphere may have originated because of the slowing down of the earth's rotation by tidal friction.

[1] B. Haurwitz, "On the relation between the wind field and pressure changes," *J. Meteor.*, **3**, 95–99, 1946.
[2] G. C. Simpson, "The twelve-hourly barometer oscillation," *Quart. J. r. meteor. Soc.*, **44**, 1–18, 1918.