In other words, the mean value of the Δp in the northern hemisphere must be equal and opposite in sign to the mean value of the Δp in the southern hemisphere. Dividing the surface of the spherical globe into zones of 5° and 10° latitude, let us denote by $[\Delta p]$ the mean values of Δp for every of these zones.

Equation (1) shows that the $[\Delta p]$ brought into a $(\sin\varphi, [\Delta p])$ diagram must appear such (stairs curve) that the algebraic sum of the areas comprised between it and the "$\sin\varphi$" axis be zero.

It is easy to check that the $[\Delta p]$ values tabulated by W. H. Munk and R. L. Miller do not fulfill this condition. As a special case, one finds with those values

$$\Big[\Delta p\Big]_{0^\circ}^{80^\circ} \equiv \begin{cases} +0.85 \text{ mb for the northern hemisphere;} \\ -2.19 \text{ mb for the southern hemisphere;} \end{cases}$$

it follows that

$$\Big[\Delta p\Big]_{80^\circ \mathrm{S}}^{80^\circ \mathrm{N}} \equiv -0.67 \text{ mb},$$

$\Big[\Delta p\Big]_{\varphi_1}^{\varphi_2}$ representing the mean value of Δp between latitudes φ_1 and φ_2.

It seems quite unlikely that this mean negative pressure difference between 80° S and 80° N could be balanced by a reasonable positive difference in the two small polar caps of 10° of latitude.

4. In their above quoted paper W. H. MUNK and R. L. MILLER (1950) report about our paper (VAN DEN DUNGEN et al. 1949) in terms such that it could be understood that we were not aware of the importance for the problem stated of the correction from sea level pressure to local pressure.

We believe that every reader who will have read through our paper will be aware that this was not the case. Our scheme was fully developed by one of our students, Miss M. LAURENT (1949, 1950).

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Reply

By W. H. MUNK and R. L. MILLER
Scripps Institution of Oceanography
La Jolla, November 15th, 1950

1. The effect of the wind circulation on the earth's rotation can be computed either from (a) changes in the angular momentum of the atmosphere as a whole, or (b) momentum transfer between atmosphere and earth. We have utilized the former method, and arrived at results consistent with astronomic observations; as described in the above note, VAN DEN DUNGEN et al. (1950)[1] utilized the latter method, with similar results.

It is true that for our method it would have been highly desirable to have had available additional meridional sections in the Southern Hemisphere, and we have said so in our article. In van den Dungen's method, the pressure effect due to mountains and a choice for the frictional coefficient between air and land, and air and water, is also not without uncertainties. Since the transfer of angular momentum over six months from the westerlies or the easterlies to the earth is large compared to the angular momentum of the atmosphere at any instant,

[1] This paper was apparently published at about the same time as our paper, but no copy has as yet been available to us. In our article we refer to van den Dungen's earlier paper in which the variations in the earth's rotation are ascribed to latitudinal changes of air masses.

van den Dungen's method must depend on the small difference between two large and somewhat uncertain terms, whereas in our case the difference is of the same magnitude as the terms from which it is formed.

2. The pressure data in Table 2 go back largely to Napier Shaw, the source used also by van den Dungen in his computations. The fact that the pressure changes do not integrate to zero over the whole globe can be ascribed to uncertainties in these observations, and in the conversion of sea level pressure to local pressure. We have accordingly stated that "it would be desirable to recompute Table 2 using the original data for the uncorrected local pressure". In any event it can be shown that our estimate for the magnitude of the inertia term (which is small compared to the wind term) will hardly be effected by allowing for this discrepancy.

3. We regret to have given the impression that van den Dungen et al were unaware of the importance of reverting the sea level pressures to local pressures. The complete corrections involve the *elevation* and the *temperature* of the weather stations. The corrections, as entered in Table 1 of van den Dungen's paper, take into account only the elevations. This part of the correction, which is of course the same throughout the year, will drop out from computations of the seasonal *differences* with which our problem is concerned. This explains why van den Dungen's February minus August values (their Table 4) are nearly the same as our *uncorrected* January minus July values (our Table 2, column 2).

The complete corrections depend partly on *temperature*, and these corrections do not drop out when differences between seasons are computed. The corrections are remarkably large. North of 30° N latitude the average local pressure variation between January and July is about *minus* 1.5 mb, whereas van den Dungen et al. obtained roughly *plus* 4 mb.

Comments on the Behavior of Jet Streams over Eastern North America

By P. F. CLAPP and J. S. WINSTON
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October 11th, 1950

Mr Phillips' article in the May 1950 issue of TELLUS (Vol. 2, No. 2) touches on an interesting point regarding confluence. It is quite true that the mere confluence of streamlines on upper level charts does not necessarily lead to increased thermal gradients and consequent increases in high level wind speeds. Confluence of the streamflow must be accompanied by differential temperature advection at most levels of the troposphere before it can result in accelerating wind speeds in the upper troposphere. However, Mr Phillips fails to consider that lack of strong advection at 300 mb may not represent conditions at all levels. It is very likely that there is cold air advection in the northwesterly flow and warm air advection in the southwesterly flow at middle and lower levels of the troposphere (i.e. up to about 400 mb). Hydrostatically, the mean temperature field of the layer below 300 mb must be similar in appearance to the 300 mb height field, but it is well-known that the flow at lower levels in middle latitudes is generally less zonal and more meridional than at 300 mb. Thus, even in the case Mr Phillips presents the streamline-isotherm relationship prescribed by the confluence theory of NAMIAS and CLAPP (J. Meteor., 1949, Vol. 6, pp. 330—336) probably does exist at most levels of the troposphere. In fact examination of the pertinent sea level and 700 mb charts indicates that this is so in the layer from sea level to 10,000 feet. Our opinion, contrary to Mr Phillips' implication, is that the situation described by Namias and Clapp is the frequent rather than the rare occurrence in cases of confluence.

Note on recent study of stability by R. Fjørtoft

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August 1950

The purpose of this note is to call attention to a method for investigating the dynamic stability of certain flowsystems recently presented by R. FJØRTOFT (1950), and to show that his results may be expressed in a clear and concise form through the use of the quasi-Lagrangian system of hydrodynamical equations discussed by the writer (STARR 1945) on a previous occasion. In order to achieve clarity, the line of reasoning employed by Fjørtoft is here repeated in revised form.

Let us consider the two-dimensional horizontal motion of an inviscid incompressible fluid of uniform density on the surface of a sphere of radius r, in the region between two fixed latitudes Φ_1, and Φ_2 at which walls are assumed to be present so that a complete zonal belt extending through all longitudes λ is formed. Following Fjørtoft we examine