by the same \( \epsilon \) value and
\[
1 - \int_{h}^{0} (\tau/\tau_0)' \, ds \leq \epsilon > 0, \tag{2}
\]
when the smallest \( H \) value which satisfies (2) is considered. Since in all practical cases \( \epsilon < 0.5 \), it follows from (1) and (2) that \( h \) is considerably smaller than \( H \); approximately, \( h \approx \epsilon H \).

In the atmosphere, one can think of \( H \) as given approximately by the "geostrophic wind level." Then, the order of \( H \) is \( 10^6 \) cm and, for \( 0.01 < \epsilon < 0.1 \), \( h \approx 10^4 \) cm. Boundary layers in wind-tunnel experiments are of the order of \( 10^6 \) cm. Owing to the geometrical dimensions, the application of the surface-layer concept is much more justifiable in the atmosphere than in aerodynamical experiments.

It follows from (1) that throughout the surface layer we have \( \tau \approx \tau_0 \), even though the equation of motion shows that \( \tau' \) is different from zero and has its extreme value at the ground surface. Consequently, the vertical profiles of horizontal momentum are nearly completely controlled by the boundary conditions (i.e., the ground drag) and practically independent of the internal forces (i.e., the frictional force \( \tau' \) or other forces) throughout the entire surface layer. The existence of internal forces of varying intensity merely means that \( h \) is not a fixed level for a given observational site and a given \( \epsilon \) value.

In view of the complicated physical structure of atmospheric boundary-layers and the present-day lack of a universal turbulence theory, it is very useful to single out the less complicated problems inherent in the lowest fraction of the atmospheric boundary-layer. Montgomery\(^2\) has investigated independently the same stratum \( 0 \leq z \leq h \), to which he gave the name "constant-flux boundary layer." I prefer the shorter term "surface layer."

For the definition of the surface layer, no hypothesis is required concerning the validity of a mixing-length theory. However, one can be relatively confident that gradient-type conduction applies to this stratum, as evidenced by the logarithmic wind-profile in adiabatic surface-layers and the semi-logarithmic profiles in non-adiabatic cases. It appears possible that a breakdown of the gradient-type conduction in the major parts of the boundary layer (as discussed by Dryden\(^3\)) does not affect, of necessity, the lowest stratum which is controlled so strongly by the boundary conditions.

**Vertical mixing velocity and horizontal friction velocity.**—I have defined the product of horizontal friction velocity \( u^* \) and vertical mixing velocity \( \bar{w} \) as follows:
\[
u^* \bar{w} = \tau/p. \tag{3}
\]


A characteristic of the surface layer is that $\tau$ at $z \leq h$ equals, to a sufficient degree of accuracy, $\tau_0$ at $z = 0$. Since height variations of air density $\rho$ can be neglected, differentiation of (3) with respect to $z$ gives
\[ \ddot{u} \dddot{v} + \ddot{u}' \dddot{w} \approx 0, \] (4)
for $0 \leq z \leq h$. Equation (4) is satisfied when either
\[ \dddot{v} = \dot{u}' = 0, \] (5)
or
\[ \ddot{u} \dddot{w} = - \ddot{u}' \dddot{w} \neq 0. \] (6)

I have shown that (5), in combination with
\[ \ddot{u}_a = \ddot{u}_a = (\tau_0/\rho)^\frac{1}{4}, \] (7)
can be considered the main characteristic of adiabatic surface-layers. In non-adiabatic states, (6) rather than (5) holds true, so that
\[ \dddot{w} \neq \dddot{w}, \dddot{w} \neq (\tau_0/\rho)^\frac{1}{4}, \dddot{w} \neq (\tau_0/\rho)^\frac{1}{4}, \] (8)
while the product $\dddot{u} \dddot{w}$ as defined by (3) is still quasi-independent of height. The singling out of a vertical mixing velocity along the lines indicated above proved very helpful for a consideration of the vertical buoyancy acceleration, which is the dynamic factor of prime importance to explain the difference between non-adiabatic and adiabatic turbulent structure.

When the notation of Inoue is used, Reynolds' expression for the eddy shearing stress is
\[ \tau/\rho = \langle u \rangle \frac{1}{4} \langle v \rangle \frac{1}{4} \tau(u, w), \] (9)
where $\tau(u, w) = \text{correlation coefficient between horizontal and vertical air motions } u \text{ and } w$. It is known that $\tau(u, w)$ deviates considerably from unity. Therefore, (3) and (9) show that $\dddot{u}$ and $\dddot{w}$ do not correspond to $\langle u \rangle \frac{1}{4}$ and $\langle w \rangle \frac{1}{4}$, respectively, and Inoue's conclusions are unwarranted. The above formal definition of $\dddot{u}$ and $\dddot{w}$ is absolutely free from assumptions regarding a mixing-length theory.

**Use of the term "isotropic."**—I have used the terms "isotropic" and "non-isotropic" to denote turbulent states where $\tau \neq 0$, $\tau_0 \neq 0$, while for regions where $\tau \approx \tau_0$ either (5) and (7) or (6) and (8) are true. In doing so it was assumed that the isolated forms $\dddot{u}$ and $\dddot{w}$ are unspecified functions of the turbulent velocity components, so that in adiabatic stratification they become independent of a rotation of the system of coordinates.

It is agreed that such use of the word "isotropic" is too restrictive. I have since decided to speak of adiabatic and non-adiabatic rather than isotropic and non-isotropic turbulence. A public statement to this effect was made on 8 June 1951, during the "Symposium on turbulence in the atmospheric boundary layer" at the Massachusetts Institute of Technology.

**Diffusion coefficients in shear zones.**—During the aforementioned symposium, I presented a paper dealing with the effect of shearing fluid flow on the distribution of diffusing matter released from a point or line source. It is planned to publish the papers and proceedings of the meeting in the *Geophysical research papers* series.

In contrast to Inoue's and Batchelor's hypotheses, I have explained the observed increase of apparent diffusion coefficients in the down-wind direction in terms of volume deformation in the fluid (or "shearing advection"). In the discussion of the paper to which Inoue refers, I pointed out that the effect should exist also in the viscous shear zone of purely laminar fluid flow. Thus, it appears possible to decide experimentally whether "shearing advection" or "eddy structure" is the cause of the phenomenon. Diffusion experiments in laminar shear flow would be most promising, inasmuch as "eddies" are non-existent and, on the basis of Inoue's and Batchelor's theory, the diffusion coefficients should remain constant in the down-stream direction of laminar shearing flow in contrast to the prediction based on my statements.

Just recently I have found that Roesler has discussed experimental evidence which proves that the diffusion of small air bubbles in purely laminar oil and water flow is highly affected by the intensity of viscous shear. Roesler explains the phenomenon in terms of volume deformation in the fluid. This is basically the same mechanism utilized independently by me to explain the effect of shearing flow in the atmosphere. It is hoped that future experimental work will clarify the problem definitely.

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