Comments on "Free Convection in the Turbulent Ekman Layer of the Atmosphere"

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The paper by Tennekes (1970) is a bold, fresh and provocative approach to a difficult problem that has not yet been solved successfully. Although it contains some interesting results, there seems to be a certain amount of confusion about scales as well as some mysterious physical concepts. Some comments are in order.

The most interesting results of the paper are the Eqs. (12) and (13). These dimensionless equations seem more meaningful than the dimensional form [Eqs. (7) and (8)] because they reveal that the $\frac{1}{3}$ power in both equations is required if $\langle\sigma_\theta\rangle$ and $\langle\sigma_w\rangle$ (σ_θ and σ_w in Tennekes' notation) are to be independent of u_*. It is of interest to relate these equations to recent observations in the surface layer. Haugen *et al.* (1971) present excellent observations of σ_w/u_* in the surface layer as a function of z/L. A fair fit of these data is given by

$$\left(\frac{\sigma_w}{u_*}\right)^2=1.6+(2\zeta)^{\frac{2}{3}},\quad \text{where } \zeta=\frac{-z}{L},$$

Now, the constant term on the right-hand side of this equation may be interpreted as the contribution by the mean wind shear; the contribution by buoyancy is therefore the second term on the-right hand side. If we assume this equation may be extrapolated over the entire boundary layer and the mechanical term may be neglected, we then obtain

$$\frac{\langle\sigma_w\rangle}{u_*}=0.95\left(\frac{h}{-L}\right)^{\frac{1}{3}},$$

This equation is remarkably close to Tennekes' Eq. (13).

Some observations of σ_θ/θ_* have been summarized by Wesely *et al.* (1970). This set of data may be approximated by

$$\frac{\sigma_\theta}{\theta_*}=0.5\zeta^{-\frac{1}{3}}.$$

If we extrapolate and average this over the entire boundary layer in the same way, we obtain

$$\frac{\langle\sigma_\theta\rangle}{\theta_*}=0.75\left(\frac{h}{-L}\right)^{-\frac{1}{3}},$$

again a result rather similar to Tennekes' Eq. (12) although in this case the difference between the coefficients is larger. However, we should remember that these coefficients are rather rough estimates. It may well be worthwhile to try to establish more accurate values for these coefficients using good quality observations throughout the boundary layer.

Even though the paper is an "exploratory study", one would hope that some of the results can be tested readily and that they would point toward simplified experimental procedures to obtain quantities which are usually difficult to obtain and which are important in the parameterization of the boundary layer. Such quantities are the heat flux H and the friction velocity u_*. These quantities are related to the bulk averages σ_w and σ_θ over the entire boundary layer, or to $\Delta\theta$ (the temperature difference over the boundary layer). A tentative solution of how $\Delta\theta$ may be formulated has been given by Leovy (1969). However, all these quantities are at least as difficult to come by experimentally as the fluxes themselves.

Then there seems to be a fair amount of confusion about "characteristic lengths and times." First the height of the thermal boundary layer is considered unknown but in the afternoon it is "assumed to be the same as the height of the momentum layer." Actually there is little mystery about the relation of the height of the thermal layer and the heat flux. The first law of thermodynamics requires that

$$\int_0^{h_t} c_p\rho\frac{dT}{dt}dz=H, \tag{1}$$

where h_t is the height of the thermal boundary layer. Because the potential temperature $\bar\theta$ is independent of height over the bulk of the boundary layer, a good approximation is

$$h_t=H\bigg/\left(c_p\bar\rho\frac{\partial\bar\theta}{\partial t}\right). \tag{2}$$

Usually an inversion is formed at a height somewhat higher than h_t because the convective motions will penetrate into the stable atmosphere beyond the level where $H=0$. As a result a downward heat flux will be generated from the inversion layer to h_t (see, e.g., Ball, 1960). It is clear that h_t is determined by the total amount of sensible heat which has entered the atmosphere from the surface during the course of the day and the original temperature distribution in the lower atmosphere, early in the morning (assuming horizontal uniformity).

In a similar way the height of the momentum layer may be formulated as the height h_m of the layer that contributes to the surface stress τ. The real physical force which generates the momentum is the pressure gradient. The momentum which is dissipated at the surface is produced by the cross isobaric flow. We might therefore define a boundary layer height h_m by

$$\tau=\tau_x=\int_0^{h_m}\frac{\partial p}{\partial x}dz, \quad \text{and } \tau_y=0. \tag{3}$$

Or, if the pressure gradient is independent of height in the boundary layer,

$$h_m=\tau\left(\frac{\partial p}{\partial x}\right)^{-1}. \tag{4}$$

This height is probably rather close to the height h introduced by Tennekes, i.e., $0.25\ u_* f^{-1}$.

There is no *a priori* reason to assume that $h_t=h_m$. In fact, at first sight, this seems a rather wild assumption. However, it may just be that thermal convection determines the thickness of the momentum boundary layer because it usually generates an inversion at the top. This inversion forms a natural top for the momentum layer as well because turbulence and consequently momentum transfer is suppressed in the stable layer with the result that over a short height interval the adjustment to the geostrophic wind occurs. Therefore, it may well be that $h_t \propto h_m \propto u_* f^{-1}$, and that convection is the controlling mechanism. However, the author does not seem to be aware of this possibility.

A real difficulty is encountered with Tennekes' theory if the mean wind disappears and $u_* \rightarrow 0$, because according to his Eq. (9), then $h\rightarrow 0$ also. The thermal boundary layer still has to obey our Eq. (1) and there is no possibility for $h_t \rightarrow 0$ if H has a finite magnitude. Thus, the asymptotic case of free convection is not included in Tennekes' paper. This difficulty may be avoided by using Eq. (4), because when $u_* \rightarrow 0$ it must be a consequence of the fact that $\partial p/\partial x \rightarrow 0$, and $h_t \propto h_m$ may still be true.

Also there is some confusion with respect to time scales. The inverse of the Coriolis parameter f is typically the time scale of the Ekman layer to adjust to changing conditions, and this may be compared to the time it takes to build the thermal boundary layer. Clearly those two times are normally of the same order of magnitude. It is difficult to understand why f should be compared with an imaginary Brunt-Väisälä frequency and that this should lead to the conclusion that there is little interaction between thermal convection and momentum transfer because these frequencies are an order of magnitude different. It is not clear either why the inverse of the Brunt-Väisälä frequency is a relevant time scale. A more straightforward and simpler time scale is, e.g., $h\sigma_w^{-1}$. This is roughly the time it takes for a convective element in an organized plume to make a roundtrip from the surface to the top of the boundary layer and back to the surface. However, most convective elements are associated with smaller scales. Also the transfer of momentum is usually associated with the relatively small scales of turbulent elements. The cospectrum berween u and w fluctuations extends typically over a frequency range from $10^{-3}-1$ Hz, with a maximum between 10^{-1} and 10^{-2} Hz, or a period of the order of a minute. The time scale range corresponding to the cospectrum may be characterized by z/u_*. There is no doubt that strong interaction between mechanical and convective turbulence exists in the lower part of the boundary layer. The dramatic change in the wind profile with only a slight imput of buoyant energy is an indication of this.

Perhaps the strangest observation in Tennekes' paper, apparently related to the difference in the time scales, is his statement that "the buoyant eddies are fast and energetic, so that it would seem that they could increase the momentum transfer by orders of magnitude. Clearly this does not occur." He seems to imply that the buoyant turbulent energy might also produce horizontal mean momentum. But this he probably did not intend to suggest. Rather, if a reservoir of momentum were available to be transferred down, this would occur. The fact is that with an increase of vertical mixing by thermal convection, the momentum does not necessarily increase as can be seen from Eq. (3). It usually will increase somewhat, but certainly not in proportion with the turbulent kinetic energy. The height h_m of the boundary layer will increase with increased heat flux at the surface but at the same time $\partial p/\partial x$ will decrease somewhat because with increased convection the cross-isobaric angle decreases as is normally observed. In any case it will be of interest to know what Tennekes had in mind when he wrote the quoted statement.

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