Reply

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Businger (1971) skilfully exposes the weak as well as the strong points of my paper (Tennekes, 1970). The agreement between the data he quotes and my theoretical predictions of σ_w and σ_θ encourages me to continue my studies of what Deardorff calls "our favorite subject." Incidentally, the results of Deardorff's (1970 a,b) computer experiment also appear to agree with my main thesis.

The definition of the height h_t of the thermal layer put forward by Businger may need some further thought, because the upper limit of integration in his Eq. (1) generally changes in time. If the air above the boundary layer is stably stratified, with a gradient $\partial\bar{\theta}_a/\partial z$, and if the mean potential temperature in the boundary layer is uniform, with a temporal rate of change $\partial\bar{\theta}/\partial t$, it would seem that h_t should increase according to $dh_t/dt=(\partial\bar{\theta}/\partial t)(\partial\bar{\theta}_a/\partial z)^{-1}$. It is not clear to me how this compares with Lilly's (1968) ideas on penetrative convection and with Deardorff's (1970a) use of a fixed inversion base. It is evident that there is a real issue here.

The height h_m of the momentum layer, as defined by Businger, is indeed proportional to the Ekman length scale u_*/f. The geostrophic wind equation gives $\partial p/\partial x=\rho f V_g$ (the surface stress is assumed to be in the x direction, and V_g is the component of the geostrophic wind normal to the surface stress); Ekman similarity (Blackadar and Tennekes, 1968) gives $V_g/u_*=-B/\kappa$, where $B\approx 4$ and $\kappa=0.4$. Substituting these expressions into Businger's Eq. (4), one obtains $h_m=0.1\ u_*/f$, which, apart from the coefficient, agrees with the Ekman height $h_m=0.25\ u_*/f$ proposed by Blackadar and Tennekes.

Concerning my assumption that $h_t=h_m$, I feel that the two have to be equal because we are considering a single "mixed layer," which only can have one characteristic height. However, this does not solve the problem of selecting an appropriate length scale. I do not know of any mechanism by which the height z_i of the inversion above the boundary layer should remain comparable to u_*/f. Conversely, if the inversion height is prescribed, as in Deardorff's work, what happens if it is either very much larger or very much smaller than the neutral Ekman height? I understand that Deardorff (private communication) has looked into this problem; I am anxious to see his results in print.

The time scale h/σ_w proposed by Businger is identical to the one first derived by Lilly (1968) and subsequently rediscovered by Deardorff (1970a) and myself [Eq. (16) in Tennekes, 1970]. It is tempting to invert the chain of arguments leading from Eq. (14) to Eq. (16) in my paper, and to *conclude* from the assumption $t_b\propto h/\sigma_w$ that the time scale t_b can also be interpreted as the reciprocal of an imaginary Brunt-Väisälä frequency.

I agree with Businger that the time scale corresponding to the maximum of the cospectrum of the Reynolds stress *in the surface layer* is of order z/u_*. By the same extension principle used in the second and third paragraphs of Businger's comments, the time scale associated with momentum transfer *above* the surface layer will be of order h/u_*. Because h is almost always comparable to u_*/f, this time scale turns out to be of order $1/f$. This is the very point I was trying to make: the time scale h/u_* of momentum transfer is large compared to the buoyant time scale h/σ_w, if $-h/L$ and σ_w/u_* are large. In the lower part of the boundary layer (more precisely, in the region where $-z/L$ is of order one) the interaction between mechanical and convective turbulence is strong, but in the bulk of the boundary layer it must be weak.

The concept of weak interaction, however, only gives a *qualitative* explanation of the relatively small increase for momentum transfer in unstable conditions. From Businger's last paragraph I get the impression that he is satisfied with a qualitative understanding of this phenomenon. He apparently failed to see that I was attempting to raise the issue of what would be involved in a *quantitative* theory. This is a central issue, not merely in planetary boundary layers, but also in basic turbulence theory, because no turbulence problem with conflicting time scales has yet been solved successfully.

It is unlikely that the problem of convection in planetary boundary layers will be resolved overnight. My paper is exploratory, addressing itself mainly to the selection of length and time scales and merely probing at the elements of a complete theory. I am grateful for Businger's comments because we cannot make headway until we have obtained at least a partial consensus on what assumptions should be used in a definitive theory.

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