Reply

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3 December 1974

Zilitinkevich deserves credit for discovering an elegant solution to a problem that was left unresolved in my paper (Tennekes, 1973). In his comments he offers a useful generalization of my treatment of the turbulent energy budget near the inversion base. In retrospect, it is obvious that I made an unduly restrictive assumption. I took the energy budget to be quasi-stationary because I needed a diagnostic equation in conjunction with the prognostic equations for the inversion strength Δ and the mixing height h. At the time, I did not realize that I was ignoring a potentially important term, though I did warn repeatedly that my equations are not valid in the limit as the inversion strength vanishes.

In that limit, the downward heat flux $-(\overline{\theta w})_i$ at the inversion base must go to zero, because my Eq. (1) [Eq. (4) in Zilitinkevich's comments] states that

$$-(\overline{\theta w})_i=\Delta\frac{dh}{dt}. \tag{1}$$

As a consequence it becomes impossible to obtain a non-trivial stationary energy budget near the top of the mixed layer if $(\overline{\theta w})_i\rightarrow 0$ and $\Delta\rightarrow 0$. Instead, the temporal rate of change of turbulent kinetic energy must be the term that balances the flux convergence of kinetic energy in the limit as $\Delta\rightarrow 0$:

$$\frac{\partial}{\partial t}(\tfrac{1}{2}\overline{q^2})=-\frac{\partial}{\partial z}(\tfrac{1}{2}\overline{q^2w}+\rho^{-1}\overline{pw}). \tag{2}$$

Here I have included the pressure-flux term (an inadvertent omission in my 1973 paper); however, I continue to neglect the dissipation term because at the top of a boundary layer without an inversion lid that term should be small compared to the others. The approximate energy budget in (2) corresponds to the one in Tennekes and Lumley (1972, p. 123), where it is stated explicitly (though not exactly in the same words) that the flux convergence of kinetic energy near the entrainment interface is used primarily to bring turbulent energy into the just-entrained fluid. I should not have missed that point.

In his comments, Zilitinkevich shows clearly that his generalization does not change matters very much for most practical purposes, because situations with an extremely small inversion strength are relatively rare. However, since the publication of the recent group of inversion-rise papers (Betts, 1973; Carson, 1973; Tennekes, 1973) there has been a lively discussion about the numerical value of the entrainment constant [the factor c in Zilitinkevich's Eq. (1)]. The values reported in the literature range all the way from 0.1 (Lilly, 1968; Deardorff *et al.*, 1969) to 0.5 (Carson, 1973; Tennekes and van Ulden, 1974), and I am grateful to Zilitinkevich for providing a foundation for further research into this issue. Incidentally, Carson (1973) apparently was the first to suggest that there might be a diurnal cycle of c, with small values in the first few hours after sunrise and a maximum in the early afternoon. Such a cycle seems to be in rough agreement with Zilitinkevich's equations.

In order to evaluate the potential impact of Zilitinkevich's correction term, it is necessary to make preliminary estimates of the various numerical coefficients involved in his Eqs. (7) and (9). A first estimate can be obtained from a careful inspection of Zilitinkevich's Eq. (3). Near the inversion base, the temporal rate of change of kinetic energy can be estimated as

$$\frac{\partial}{\partial t}(\tfrac{1}{2}\overline{q^2})=\frac{\partial}{\partial z}(\tfrac{1}{2}\overline{q^2})\frac{dh}{dt}, \tag{3}$$

where the vertical gradient of kinetic energy is evaluated over a thin layer near the base of the inversion. Following Zilitinkevich, we approximate this gradient by $c_2\sigma_w^2/h$, where σ_w is the vertically averaged standard deviation of vertical velocity in the mixed layer. We obtain

$$\frac{\partial}{\partial t}(\tfrac{1}{2}\overline{q^2})=c_2\frac{\sigma_w^2}{h}\frac{dh}{dt}. \tag{4}$$

Because the kinetic energy gradient near the inversion base is quite steep (Willis and Deardorff, 1974), we should expect that c_2 is substantially larger than 1, in contrast with Zilitinkevich's rather casual assumption.

In this context, it should be pointed out that the temporal rate of change of kinetic energy is likely to be rapid only near the inversion base. The average kinetic energy in the interior of the mixed layer changes much more slowly with time. This distinction was overlooked by Willis and Deardorff (1974), who used a quasi-stationary energy budget everywhere, and therefore attributed the difference between the flux convergence near the inversion base and the locally negative buoyancy flux entirely to the (not independently measured) dissipation term. The large dissipation rates near the inversion base in the experiments of Deardorff and Willis raise another problem, however. Because that problem is related to a different limit process, we postpone a discussion of it to the final paragraphs of this reply.

Returning to the limit as $(\overline{\theta w})_i\rightarrow 0$ and $\Delta\rightarrow 0$, we find that Zilitinkevich's Eq. (5) reduces to

$$c_2\frac{\sigma_w^2}{h}\frac{dh}{dt}=c_3\frac{\sigma_w^3}{h}, \tag{5}$$

so that

$$\frac{dh}{dt}=\frac{c_3}{c_2}\sigma_w. \tag{6}$$

This result (which was hinted at in the last section of my 1973 paper) is a very attractive consequence of Zilitinkevich's analysis. It states, simply and convincingly, that in the absence of an inversion lid the boundary layer grows at a rate proportional to the turbulence intensity in its interior.

Eq. (6) allows a quick estimate of c_3/c_2. If the surface heat flux is zero, the average value of σ_w in the boundary layer over a rough surface is roughly equal to 0.7 u_*, where u_* is the surface friction velocity (Hinze, 1959, p. 490). Also, in the absence of a capping inversion the entrainment rate is approximately 0.25 u_* (Tennekes and Lumley, 1972, p. 191). Therefore, a preliminary estimate for c_3/c_2 is

$$c_3/c_2 = 0.35. \tag{7}$$

The constant c_3 can be found by taking another extreme case, in which the mixing height is so large that Zilitinkevich's correction term is negligible, and with the surface heat flux large enough to maintain a state of free convection. In that case, Zilitinkevich's Eq. (5) reduces to

$$-\frac{g}{T_0}(\overline{\theta w})_i = c_3\frac{\sigma_w^3}{h}. \tag{8}$$

The recent laboratory data of Willis and Deardorff (1974) suggest that in a state of free convection $\sigma_w^2 \approx 0.3\ w_*^2$. Here, w_* is the convective velocity scale (Tennekes, 1970; Deardorff, 1970), which is defined by

$$w_*^3 = \frac{g}{T_0}(\overline{\theta w})_0 h. \tag{9}$$

Incidentally, this relation is based on a quasi-stationary energy budget. Because the kinetic energy in the interior of the mixed layer varies quite slowly, a diagnostic estimate of σ_w is appropriate in all cases of practical interest.

Using the relation $\sigma_w^2 = 0.3\, w^{*2}$ and substituting (9) into (8) yields

$$-\frac{(\overline{\theta w})_i}{(\overline{\theta w})_0} = 0.16 c_3. \tag{10}$$

We now refer to the investigations of Carson (1973) and Tennekes and van Ulden (1974), which suggest that the downward heat flux near the inversion base may be as large as one-half of the surface heat flux if the height of the convective mixing layer is large. Assuming that these data (which, needless to say, refer to a fictitious heat flux at the idealized inversion, chosen in such a way that the predicted entrainment rate is in accordance with observations) are reliable, we have to conclude that

$$c_3 = 3.1. \tag{11}$$

Because $c_3/c_2 = 0.35$ by virtue of (7), this yields

$$c_2 = 8.8. \tag{12}$$

This value is large enough to warrant the conclusion that Zilitinkevich's correction term will have considerably more impact than he has anticipated.

A few additional calculations show that the constants c and c_1 in Zilitinkevich's Eq. (7) are probably about 0.5 and 2.6, respectively, and that preliminary values for the constants in his Eq. (9) are $A = 1.1$, $A_1 = 4.3$. All of these estimates are based on the assumption that c_2 and c_3 are independent of conditions in the mixed layer and of those in the inversion that caps it. That assumption may not be justifiable: further studies are very much in order.

This brings me to another problem that requires more research. Several of my colleagues have raised objections to the assumption that the dissipation rate is small near the inversion base. I believe that this assumption is a fair approximation for all cases in which the inversion strength is relatively small, but I do concede that it raises a serious problem if the mixed layer happens to be capped by a strong inversion. The laboratory data of Willis and Deardorff (1974) suggest that the "constant" c_3 in (10) decreases as the lapse rate γ of potential temperature in the stable air above the mixed layer increases. Their data also show that the nondimensional dissipation rate near the inversion base increases as the lapse rate becomes more stable. This effect must be even stronger than the data suggest, because the correction for the temporal rate of change of kinetic energy is smaller when the entrainment rate decreases due to greater stability in the air aloft.

All of this suggests that I did not treat the energy budget correctly in the limit as $\Delta \to \infty$. In that limit, it is obvious from (1) that $dh/dt \to 0$ because $(\overline{\theta w})_i$ cannot increase indefinitely. However, Eq. (1) cannot specify how $(\overline{\theta w})_i$ behaves in the limit as $\Delta \to \infty$. Since the largest value of $-(\overline{\theta w})_i/(\overline{\theta w})_0$ reported to date is about 0.5, while the value appropriate to strong inversions seems to be about 0.1 (Lilly, 1968; Willis and Deardorff, 1974), there is some evidence to support the speculation that $-(\overline{\theta w})_i/(\overline{\theta w})_0 \to 0$ as $\Delta \to \infty$. This idea is supported by the intuitive notion that entrainment should cease altogether if the inversion strength is large enough. In that limit, we cannot avoid the use of an approximate energy budget in which the flux convergence is balanced by the dissipation rate. This calls for an appropriate parameterization of the dissipation term. I do not know of any successful attempts to solve this problem, and I suspect that it will be very difficult to find a suitable model because it has to take into account the fact that the efficiency of conversion of kinetic energy to potential energy decreases as the strength of the inversion lid increases.

Acknowledgments. This research was supported by the Atmospheric Sciences Section of the National Science Foundation under Grant GA-35422X, and by the Environmental Protection Agency through its Select Research Group in Air Pollution Meteorology at The Pennsylvania State University.

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