Reply

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Lewellen *et al.* (1985; hereafter referred to as the authors) show that their relatively simple second-order closure model can qualitatively reproduce the asymmetry between top-down and bottom-up diffusion. I had not noticed this, and I thank them for pointing it out. This provides another illustration of how second-order closures can sometimes give useful insights into constitutive relations in turbulence. I gave several other examples in a recent survey paper (Wyngaard, 1982).

The authors mention that their result is sensitive to the closure for turbulent transport, and that while it agrees qualitatively with the LES result, a better closure would presumably improve the agreement. This reflects an attribute of this type of turbulence modeling: its closures (which are usually all but impossible to formulate from first principles, and difficult to develop from existing data) are often crude, and yet the resulting model predictions can sometimes be strikingly realistic. This has long perplexed turbulence researchers, as evidenced, for example, by Bradshaw's comment at the Stanford Conference (Kline *et al.*, 1981, p. 709) on the modeling of engineering flows:

> The present position, therefore, is a not unfamiliar one in turbulence research; some calculation methods reproduce some of the experimental results, but the process by which they do it is unclear or unrigorous.

One challenge, clearly, is to enhance the reliability of these models (which are far cheaper to use than LES codes) in a reasonably wide range of applications. At the Stanford Conference, Lumley (Kline *et al.*, 1981, p. 767) suggested how to meet this challenge:

> I believe it makes sense to search for models that cover as large a domain as possible on the basis of better physics; I base this belief on the success that has been experienced in the last decade in extending existing models *on a physical basis* to new situations: transport, return-to-isotropy, and buoyancy are now much better understood by some workers, and models for these terms are under development that have every appearance of being much more nearly universal. The last serious hold-outs are the rapid terms, which are still poorly understood. In addition, I feel that . . . if we are to extend the existing models, the only hope is by consideration of the physics.

With that motivation, I'll discuss briefly some of the physics of the closures on which the authors' development rests. I'll extend their own discussion of the transport closure, and then focus on the closure for the pressure correlation.

The authors use a gradient-transport closure for third moments (Lewellen, 1977),

$$\overline{w'w'c'} = -D\frac{\partial}{\partial z}\overline{w'c'}$$

$$\overline{w'c'\theta'} = -D\frac{\partial}{\partial z}\overline{c'\theta'} \qquad (1)$$

where D, the diffusivity, is of order qL where q and L are turbulent velocity and length scales. Data from studies in Minnesota (Kaimal *et al.*, 1976) and in the AMTEX experiment (Wyngaard *et al.*, 1978; Lenschow *et al.*, 1980) indicate that over most of the convective PBL this closure is reasonable in the sense that D is positive as assumed in (1). However, the data also indicate that the magnitudes of D for $\theta'q'$, $w'\theta'$, and $w'q'$ (where q is water vapor mixing ratio) differ substantially. For example, values of D, scaled with w_*z_i and averaged over the layer between $0.2z_i$ and $0.8z_i$, range from about 1.0 for $w'q'$ to about 0.1 for $\theta'\theta'$ and $\theta'q'$, an order-of-magnitude variation. Thus, the simple closure (1) for transport could be improved, as the authors suggest. One route is the third-order closure of Zeman and Lumley (1976) mentioned by the authors; it allows for the inclusion of the explicit buoyancy effects which presumably cause some of these differences in D. Perhaps this closure could be developed further through LES-generated data bases, as suggested by a recent LES study group (Wyngaard, 1984).

The authors' closure for the pressure correlation in their (1) is

$$\frac{\overline{c'}}{\rho}\frac{\overline{\partial p'}}{\partial z} = \frac{\overline{c'w'}}{\tau_1} \qquad (2)$$

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where τ_1 is a time scale, taken to be a property of the turbulence and, hence, the same for top-down and bottom-up diffusion. Moeng's LES results shown in Fig. 8 of Moeng and Wyngaard (1984) suggest that this closure is qualitatively adequate (in the absence of significant mean wind shear) in the sense that τ_1 is positive throughout the PBL. Her results also suggest, however, that in the upper portions of the PBL the τ_1's for top-down and bottom-up diffusion differ by a factor of 2.

One can obtain some insight into pressure correlations from the Poisson equation for pressure in incompressible, Boussinesq turbulence:

$$\frac{1}{\rho}\nabla^2 p' = -(u'_{i,j}u'_{j,i} - \overline{u'_{i,j}u'_{j,i}}) - 2U_{i,j}u'_{j,i} + \frac{g}{T}\theta'_{,3} - 2\epsilon_{ijk}\Omega_j u'_{k,i}. \quad (3)$$

Here a comma denotes differentiation, and repeated indices are summed. The forcing terms on the right side represent turbulence, mean shear, buoyancy, and Coriolis effects. The authors' closure is usually considered to represent only the first of these; the rest give the "rapid" terms mentioned by Lumley. Lumley (1979), Zeman (1981), and Wyngaard (1982) among others discuss closures for the rapid terms.

Moeng and Wyngaard (1985) have decomposed the resolvable-scale pressure field from her LES runs into these four components. They find that the rapid buoyancy contribution cancels about 50% of the direct buoyant production term in the authors' (1), and that mean wind shear effects also enter that equation through the pressure correlation term. We conclude that the closure in (2) does qualitatively represent some of the physics of the pressure correlation term, but also omits some.

In summary, it is intriguing that a relatively simple turbulence model can display the asymmetry between top-down and bottom-up diffusion. I commend the authors for pointing this out, and at the same time suggest that they might have oversimplified the second-moment physics. Nonetheless, their finding provides hope that if even more physics is put into these models they can ultimately be used with confidence in a range of geophysical applications.

The authors' second point is that my parameterization scheme (Wyngaard, 1984) is not sensitive to the asymmetry in diffusion. In some of my numerical experiments I found I could create "unmixed" layers if I increased g_t by a factor of 2–4. This is interesting because some recent measurements (Zhou and Lenschow, personal communication, 1984) indicate that g_t can be larger than we found in our LES studies, by about that factor. Clearly, we have much to learn about the physics of the entrainment region, as the authors suggest.

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