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FURTHER COMMENT ON "REPLY TO THE COMMENT BY KOWALSKI ON 'AN ALTERNATIVE APPROACH FOR CO_2 FLUX CORRECTION CAUSED BY HEAT AND WATER VAPOUR TRANSFER' BY LIU"

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The eddy covariance technique defines the turbulent CO_2 flux at the instrument height ($z = z_i$) as the covariance between direct measurements of CO_2 concentration (c) and vertical wind speed (w) in an *Eulerian* frame of reference. If the scalar measured is CO_2 density (ρ_c) rather than c, then turbulent fluctuations in two other *Eulerian* quantities (temperature and water vapour density, each correlated with w) induce scalar variability that, although unrelated to surface CO_2 exchange, can strongly influence the eddy covariance. Correcting for "density effects" (Webb et al., 1980) means artificially eliminating such influences from the (w, ρ_c) covariance in order to determine the turbulent flux at $z = z_i$, and so infer surface exchange. In his reply, Liu (2006) redefines "density effects" to mean ρ_c differences due to parcel expansion during ascent from the surface to $z = z_i$. Such *Lagrangian* parcel adjustments have no direct relevance in the interpretation of eddy covariance measurements at a fixed height.

Liu's puzzle regarding the utility of the boundary condition employed by Webb et al. (1980) can be solved by constructing a budget equation for dry air. Within such a framework, and assuming horizontal homogeneity as throughout this discussion, the assumption of no vertical dry air flux (*total* flux, including mean and turbulent components) is wholly justified. Conversely Liu's proposed existence, if it were true, of "a small negative dry air flux" at $z = z_i$ would imply either dry air accumulation/storage in the layer below, or a dry air sink somewhere below $z = z_i$ (in the air or at the surface). Neither is plausible.

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References

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