Review for Atmospheric Chemistry and Physics: ACP-2022-543
"Dependency of vertical velocity variance on meteorological conditions in the convective boundary layer" - *by Noviana Dewani et al.*

General Comments

This manuscript examines daily profiles of normalized vertical velocity variance to explore where observed residual/unexplained variance originate. The authors focus on moisture effects and conclude that a new scaling parameter is needed for convective boundary layer parameterizations that account for such effects. The topic is relevant and the paper is generally well-written.

My first concern is that I believe the authors' formulation for w_* is incorrect as presented in their Eq.(2). The Deardorff (1970) formulation is

$$w_* = \left[\frac{g}{\theta_{vr}} z_i \left(\overline{w'\theta_v'}\right)_o\right]^{\frac{1}{3}}.$$

If we focus on the virtual heat flux (drop the subscript and assume surface values), we can expand as

$$\overline{w'\theta_v'} = \overline{w'\theta'} + 0.61\theta_{vr}\overline{w'q'}.$$

Using the authors' notation, we can define

$$\mathsf{SHF} = \rho c_p \overline{w'\theta'} \qquad \text{and} \qquad \mathsf{LHF} = \rho L_v \overline{w'q'}.$$

Substitution yields,

$$\begin{aligned}\overline{w'\theta_v'} &= \frac{\mathsf{SHF}}{\rho c_p} + 0.61\theta_{vr}\frac{\mathsf{LHF}}{\rho L_v} \\ &= \frac{1}{\rho c_p}\left(\mathsf{SHF} + \frac{0.61\theta_{vr} c_p}{L_v}\mathsf{LHF}\right).\end{aligned}$$

If we take $\theta_{vr} = 300$ K, $c_p = 1004$ J kg^{-1} K^{-1}, and $L_v = 2.5 \times 10^6$ J kg^{-1},

$$\overline{w'\theta_v'} = \frac{1}{\rho c_p}\left(\mathsf{SHF} + \frac{0.61(300\text{ K})(1004\text{ J kg}^{-1}\text{ K}^{-1})}{2.5 \times 10^6\text{ J kg}^{-1}}\mathsf{LHF}\right).$$

Solving and substitution yields:

$$w_* = \left[\frac{g}{\theta_{vr}}\frac{z_i}{\rho c_p}\left(\mathsf{SHF} + \mathbf{0.07}\mathsf{LHF}\right)\right]^{\frac{1}{3}}.$$

Note that the coefficient is 0.07, and not 0.7 as listed in the manuscript's Eq.(2). The authors should check which version they used, because the use of 0.7 would give latent heat flux an order-of-magnitude more importance to the normalizing value w_*, which would perhaps lead to the observed lack of profile collapse after scaling and may call into question the entire premise of the paper.

Next, I believe there are issues with the choice of considered fields and conclusions drawn from their use. The use of BR and RH are relative terms. Why did the authors also not look at more absolute terms such as mixing ratio or dew point temperature? I worry that the use of relative terms hide analysis of other important terms contained within. For instance, the authors note that in the cases where BR was lowest, the LHF was highest and so was the normalized variance. Just looking at Eq. (2), an increase in LHF would presumably lead to a larger w_* in the absence of a known change in SHF. That in turn would lead to a lower value of the normalized variance values without a known change in the absolute variance, which is the opposite of the findings. In other words, it is hard to gauge any conclusions without knowing how related terms are affected in the presented scenarios. SHF is especially ignored throughout by the confusing justification that SHF is already accounted for in w_*. However, LHF is also contained in the equation, as shown above and in the authors' own Eq. (2). I think a more advanced multivariate analysis technique is needed to establish whether the stated conclusions are valid, especially in light of the potentially wrong form of w_* as described in the paper.

Based on the above considerations, I believe this paper requires enough work that it would look substantially different than it does in its present form. Additionally, assuming that my mathematics are correct, this paper cannot be published nor conclusions trusted without knowing whether Eq. (2) contains a simple typo or the authors used an incorrect coefficient. In addition to these broad issues, I have a few specific issues that are listed below. Accordingly, I recommend that the manuscript be **rejected** for publication in *Atmospheric Chemistry and Physics*.

Specific Comments

- **Line 54** "*Large Eddy Simulations*" need not be capitalized here.
- **Eq. 2** See above for my comment, but I believe the expression for w_* is wrong. There is also no citation or explanation of how the authors arrived at this expression.
- **Fig. 6** The caption should read "The outliers *are* denoted ..."
- **Line 150** In Figure 10, the authors present correlation coefficient with two digits to the right of the decimal. For Bowen ratio, the value is shown as 0.28, yet the text says "*an absolute correlation coefficient equal to* 0.3 *...*" The authors should either present the correlation coefficients as rounded to the tenths spot in the figure, or write that the value is approximately equal to 0.3 in the text.
- **Figure 10** Temperature has a similar absolute correlation coefficient as Bowen ratio. Why not examine that as well?
- **Line 162** The authors state that since "*the sensible heat flux is already taken into account in the convective velocity scale, the* σ_w^2/w_*^2 *dependency is now examined based on the surface latent heat flux classification.*" I am confused by this reasoning to avoid examining SHF because LHF is also accounted for in the convective velocity scale (per Eq. 2).
- **Sect. 4.2.3** Given that there is no analysis for rainy days, this short section seems unnecessary. The authors even allude to this on Line 110.
- **Line 195** Again, I am confused why the scaling by w_* means that the dependency of the normalized variance on the Bowen ratio is attributable only to LHF. Both SHF and LHF are contained in w_*.