Review of Galewsky and Los 2025

November 11, 2025

This article documents how humidity and water vapor isotopic composition of the subcloud layer in the trade-wind region vary with entrainment and mesoscale vertical motion. It uses measurements from the EUREC4A campaign and a simple analytical model of the subcloud layer.

The motivation for this study is to contribute to the debate on the validity of the mixing-desiccation mechanism for the sub-cloud layer in the trade-wind regions (Sherwood et al 2014), especially regarding the role of mesoscale circulations (Vogel et al 2022). This is an important topic for the scientific community interested in cumulus cloud feedbacks.

The article argues that the isotopic composition is more sensitive to mesoscale vertical motion than to entrainement, whereas the humidity is more sensitive to entrainment than to mesoscale vertical motion. I found that the argument for this based on EUREC4A observations was convincing. However, I was not convinced by the physical mechanisms to explain this behavior. I think the analytical model fails to capture this behavior, and this is my major comment.

Except from this comment, my comments are only minor. The paper is well written and illustrated.

1 Major comment: the analytical model fails to capture the observed behavior at the core of the article

The observed behavior at the core of this paper is that the isotopic composition is more sensitive to mesoscale vertical motion than to entrainement, whereas the humidity is more sensitive to entrainment than to mesoscale vertical motion. Using the notations of the article, it means that:

$$d\delta_{BL}/dW > d\delta_{BL}/dE$$

$$dq_{BL}/dW < dq_{BL}/dE$$

Fig 6 is very convincing to prove this.

Fig 7 also confirms this behavior (if I understand well, "Eff." is a non-dimensional parameter that reflects $(d\delta_{BL}/dW)/(d\delta_{BL}/dE)$ or $(dq_{BL}/dW)/(dq_{BL}/dE)$?)

Any successful analytical model should capture this behavior. However, this is not the case for the analytical model proposed here. Rather, an analysis of the equations yields $d\delta_{BL}/dW)/(d\delta_{BL}/dE) = (dq_{BL}/dW)/(dq_{BL}/dE)$.

The article argues that δD_{BL} responds 7.5 times more to W than q_{BL}. However, this is an artifact of normalizing by the observed standard deviation. There is no good reason to do this normalization, because the standard deviation may have nothing to do with the impacts of W and E that we are investigating. In addition, I don't think that just showing that $(d\delta_{BL}/dW)/\sigma_{\delta BL} > dq_{BL}/dW)/\sigma_{qBL}$ is useful for the whole argument of the paper, which is to investigate the interplay between E and W. So we really need to compare the response to E and to W.

Therefore, I recommend not artificially normalizing by the observed standard deviation. Non-dimensional numbers can be obtained by calculating rations such as $(d\delta_{BL}/dW)/(d\delta_{BL}/dE)$ or $(dq_{BL}/dW)/(dq_{BL}/dE)$, which roughly correspond to the slopes of the diagonal contours on Fig 6.

Showing the E-W plot for the modeled q_{BL} and δ_{BL} as in Fig 6 would be sufficient to show the failure of the model.

The failure of the model is expected from its equations: q_{BL} and δ_{BL} depend on ϵ_{eff} which depends on $E - W$. Therefore, this model does not allow to separate the impacts of E and W.

I had some fun playing around with equations to try to come up with equations that separate E and W and in which $d\delta_{BL}/dW)/(d\delta_{BL}/dE)$ would be different from $(dq_{BL}/dW)/(dq_{BL}/dE)$. Actually, I couldn't. I even tried a two-layer model, it failed as well. Maybe I didn't try hard enough, but I expect that making q_{BL} and δD_{BL} respond differently to E and W is not so obvious.

Maybe some alternative hypotheses should be considered to explain that δ_{BL} responds more strongly to W than to E whereas q_{BL} responds more strongly to E than to W. One hypothesis: mesoscale ascent favors the development of cumulus clouds, whose rain drops evaporate in the subcloud layer? It has a strong enriching effect, but only a moderate moistening effect. A simple analytical model that includes this effect would capture the observed behavior.

2 Minor and detailed comments

- I don't agree with the sentences arguing the model captures the asymmetric behavior, consistent with my major comment: l 9, most of the discussion and conclusion
- l 109: "similarly constrained": what does constrain mean here?
- l 111: "constraining" -> limiting?
- l 121: remove one δ
- Fig 3: could it be useful to also plot h as a function of time? I was wondering to what extend variations in h alone could contribute to the observed correlation pattern. Would it be useful to plot some W profiles as well? To see where the W extrema are relative to h?
- Fig 4: make larger labels
- l 136: "clear diagonal pattern": I cannot see it clearly on Fig 5. Rather, I can see a q gradient with E, i.e. horizontal pattern. This impression is confirmed by the nearly vertical lines on Fig 6b.
- l 155: "more equitable": I disagree, Fig 5 and 6 rather show that E dominates for W, whereas it is more "equitable" (i.e. diagonal pattern) for δ.
- l 163: "moisture characteristics" -> "moisture δD" only?
- Fig 7: Δ-> δ in the caption
- l 176: I recommend not doing this normalization, consistent with my major comment. Same Fig 8. Replace by something more comparable to Fig 6.
- l 201: FT: I think earlier it was written that the entrained air comes from the CL. Make notations consistent? Same l 264 and throughout appendix A.
- l 209: "large-scale ascent" -> "convective updrafts": in Risi et al 2020, the export of enriched air is through convective scale updrafts.
- l 222: "bottom-heavy variance": what does it mean? Same l 246
- l 250-252: I disagree with (4) consistent with major comment. I'm also not convinced by (1) and (2). For (1): can you come up with a number? For (2): can you be more specific?
- Appendix A3: if R_{flux} is given by Craig and Gordon, then it should depend on R_{BL}. It's important to account for this feedback because it attenuates δ variations in the boundary layer, so that you need a more strongly enriching process when $W > 0$ in your model than if you ignore this feedback. When deriving the equation, you will end up with a Merlivat-like equation for R_{BL}, as in Risi et al 2020.