

ACP - 2025 - Galewsky and Los

Galewsky and Los present an analyses of stable water vapor isotope measurements taken during the EUREC4A campaign to demonstrate the role that the mesoscale has to play in modulating moisture heterogeneities near the surface. They show that an isotopic signature is more prominent while diagnosing the mesoscale effect compared to humidity values alone. They use observational analyses and a simple-physics steady state model to demonstrate how the mesoscale might be modulating these moisture and isotopic differences. The study is very timely and address a prominent question that two recent studies (Vogel et al, 2022 and George et al, 2023) have left in their wake while highlighting the importance of the mesoscale. This study also taps into the immense potential of using isotopes for understanding moisture pathways in the atmosphere - something traditional measurements are limited in. If their results can be substantiated (with suggested corrections here to their own analyses) and with more evidence from future investigations, this study lays down important groundwork in showing how the mesoscale mediates moisture - an open question in the shallow cumulus' research community, with important implication for cloud feedback and climate sensitivity studies.

That said, I have a few major as well as minor issues with some aspects of the study, which I have listed here below (and due to a lack of time, have been unfortunately unable to digest down into more concise text).

MAJOR COMMENTS:

1. Apparent AI Hallucination:
 - a. The Galewsky et al (2022) study cited multiple times throughout the paper has an incorrect full reference at L395. I believe the correct citation should be Galewsky, J., Jensen, M. P., & Delp, J. (2022). *Marine boundary layer decoupling and the stable isotopic composition of water vapor*. *Journal of Geophysical Research: Atmospheres*, 127, e2021JD035470. <https://doi.org/10.1029/2021JD035470>. However the study cited, is a fictional one. I could find no study of the given title or with the given combination of authors. The DOI leads to a paleoclimate glaciation study (that the DOI exists probably is why any automated checks from the journal might have missed this). This seems like an unfortunate consequence of using AI-powered tools that are known to hallucinate. However, the authors are responsible for the final product and such mistakes shouldn't be carried over in to it, not least when the full citation in the study includes both the present authors. I haven't made similar checks for all studies listed in the references, but I would request that the authors do so.
2. Data discrepancy and unclear specifications:
 - a. L110: "The measurement uncertainty for 6-hour averaged isotope data (1.24‰ for δD) represents approximately 65% of the observed natural variability, constraining our ability to detect weak atmospheric signals. Consequently, our analysis focuses on the strongest and most robust correlations that exceed this measurement noise threshold" - What exactly does this mean for the data considered / statistics shown in this study? Were only those circles considered whose corresponding δD values lay outside the 1.24‰ range from the campaign mean?
 - b. Figure-3: For these correlations, how were the ship-derived properties sampled? Were these averaged over the time-period that HALO took to fly the circle? Circle-time was ideally obtained through the flight-segmentation files provided via Konow et al (2021) to be consistent with the W measurements.
 - c. Figure-4: (a) It is unclear how the circles have been chosen for the E, W. Most days show fewer than 6 circles, which was the case for a typical HALO flight. However, 0209 shows 7 data-points whereas there were only 6 circles flown for that flight. There was no HALO flight on 0210, but there is a data point in the figure. Such discrepancies are very surprising.
 - d. L121: I don't understand the choice of a 6-hour average centered on circle time. One circle takes one hour. A typical HALO flight had 3 consecutive circles, followed by a roughly 1.5 non-circle excursion, followed again by 3 consecutive circles. So, in a period of 7-8 hours, there are 6 circle measurements available. Having 4-6 data points per day in this plot means that many data points likely have an overlap of 4-5 hours. So, the 6-hourly average doesn't make sense, the autocorrelation between the data-points would be very high.
3. Statistical analyses and interpretations thereof:
 - a. Figure-5 and L129-L138: I can see that the E-W space indeed shows a range of BL moisture states and I understand the physical reasoning put forward by the authors. However, I am unconvinced that there is a "clear diagonal pattern" for δD from high E, negative W to low E, positive W. With a similar level of agreement, one could also argue that ignoring the 5 points of $E > 30$ mm/s, there exists a diagonal pattern from low E, negative W to higher E, positive W with more depleted points and more enriched points in the lower-left and upper-right, respectively. This would go against the relationship that the authors describe in the text. If the authors could please provide some quantification (e.g. a regression coefficient) for these relationships, then it would be more convincing.
 - b. Figure-6: In continuation with my understanding that the correlations are poor in Figure-5, I think that the multi-linear regression (MLR) model also doesn't show convincing results in its agreement with the observed data points. Panel-(B) seems to be doing slightly better, but the large spread in both panels indicates to me that the MLR is not a good predictor for either of the variables. Please also provide the coefficients and the residual errors that come out of the MLR.
4. Counteraction efficiency:
 - a. L147-L164 and Figure-7: I really like the idea and intent behind this analysis, i.e. studying the competing effects of E and W on δD , and is a timely effort indeed. However, the analysis doesn't really separate the effects of E and W. The residuals are computed by first removing the linear regression coefficient of E to explain D. Next these residuals are regressed against W and an r-value of 0.464 is obtained. This residual regression is only applicable if we consider that E and W are uncorrelated. That's not the case. Vogel et al (2022)'s Fig-4g shows that E and W have a negative correlation ($r = -0.25$ at 1h and -0.35 at 3h, and could be stronger if we go with the 6-hour choice in the study). A better way of getting the "true" independent linear relationships that E and W have with δD would be getting their coefficients from the multi-linear regression already done for Figure-6. Here the coefficients for E would assume effect with constant W and that for W would assume constant E.
 - b. I'd also like to add a note about the interpretation of the "counteraction efficiency", which the authors define as the ratio of the slope of regression of W to that of E. In the present analysis, the slope of regression for W is with the residuals whereas for E, it is with δD . At least this is how I understood L150 which follows the description regression with residuals. So, the slopes are not really comparable (note also my previous comment about other inconsistencies in performing such an analysis). However, even if both coefficients are from a regression against δD (say from a multi-linear regression), there should be some normalization done before computing the coefficients so as to account for the mean and range of the predictors E and W (as shown in L366). Otherwise, 1 mm/s of E cannot be comparable to a 1 mm/s of W and the physical conclusions for counteraction interpreted here will not really be that observed in nature.
5. Precipitation events:
 - a. Precipitation and cold-pools were both important mesoscale-modifying events during the EUREC4A campaign as shown by Touzé-Peiffer et al., 2022 and by Radtke et al, 2022 as well as Radtke et al, 2023. Certain flights (evident from Touze-Peiffer's study) during the campaign did sample both precip and cold-pools. These data-points could be ones that will show a contrasting signal in the moisture-W feedback, because with cold-pools you would have higher moisture but low values of W.
 - b. The cold-pool's effect also might be responsible for the vertical localization being around 500 m, because as such in the absence of cold-pool cases, W seems to scale linearly upwards from the surface to around SCL top. This would mean that the correlation shouldn't necessarily change from near-surface to SCL top. However, with cold-pools the values of h will go below 400 m, thus preferentially improving correlation scores above the 400 m which is also what seems to be the case in Figure-3 in the present study. If the correlation is checked by controlling for data-points associated with precipitation and/or cold-pools (as suggested above), then the study can solidify why the vertical localization happens at that altitude.
 - c. The effect of precipitation and cold-pools is missing from the entire discussion, which have a big role to play in setting the BL moisture as well as modulating the properties of the mesoscale. See for example studies like Vogel et al (2021) and Alinaghi et al (2025). Isotope measurements are significantly impacted by the presence of precipitation and resulting evaporation-induced cold pools. Not including a discussion of how such events have impacted these findings would be remiss.

MINOR COMMENTS:

- Throughout the text, consistency between EUREC4A and EUREC⁴A
- L53: "atmospheric properties" instead of "circulations"

- L60: HALO measurements and flight strategies for EUREC4A are described in detail in Konow et al (2021), and hence should ideally be cited here for the reader who might be interested in more details.
- L63: instead of "mesoscale circulation", say "area-averaged kinematic". The latter is more apt and precise.
- L66: and that stationary in the time-period that is taken to sample that space.
- L70: 70 EUREC4A-circles over how many days? The number of days would indicate roughly the different synoptic scale meteorological conditions that were present for the sampling of the mesoscale circulations
- L76 (or L63): At one of the earliest mentions of "divergence" in the text, it should be explicitly stated that this is "horizontal divergence of wind velocity (hereafter referred to simply as divergence)". Otherwise, it remains unclear the divergence of which quantity is being talked about and if the divergence is 3D or horizontal. Same with vorticity, but this term doesn't appear later.
- L78: "h" is later (e.g. Section-3) referred to as SCL height - keep terminology consistent.
- L90: Seeing as these terms (E & h) hold quite some weight in later discussions, it would help the reader greatly to know what were the mean (+/- std dev) values through the campaign. It provides good context for interpreting values discussed later.
- L93: "sampling rate" and not "resolution"
- L108: Please specify where the RH and SST measurements are from. I'm assuming the RV Meteor has in-house instruments, but please specify if this is the case. Also what height and depth were these measured, respectively?
- L118: "vertical velocity exerts its primary influence on surface humidity through processes operating near the top of the subcloud layer." Not enough data yet shown to imply causation.
- L119: How should one interpret the fact that δD shows consistently greater correlation with W compared to q?
- L124 and L125: E values are always positive. The distinction in the text is unclear when the authors refer to the enhanced and weak entrainment periods. Maybe the authors intend to show this as anomaly of E from campaign-mean? If so, that would make sense with the peripheral text in the paragraph.
- L125: No HALO flight on February 1, the text should state January 31 instead.
- L128: Whereas the arguments in this paragraph make sense and could be indicative of what the data shows, there is an inherent assumption here that the surface fluxes are consistent for all the days. This might not be a valid assumption because the estimation of E here considers it directly proportional to the surface buoyancy fluxes (SBF). This means that enhanced E also means enhanced surface fluxes (likely increased wind) given unchanging stability between the sub-cloud and cloud layers. Vogel et al (2022)'s Fig-4b indeed shows that E is very well correlated with SBF and less so with the buoyancy jump. Thus, I believe that the anti-correlation between E and δD cannot be simply explained without controlling for the surface flux conditions, which also play in a role in how fast or slow they can enrich near-surface water vapor.
- Moreover, it needs to be stated that the effect that W has on q or δD of the BL is not a direct physical effect unlike that explained for the effect of E. In assumptions here of a well-mixed boundary layer, whereas both W and E change h, only E can change the BL properties (such as q and δD). W cannot change q and δD because of the well-mixed assumption. Any relationship that is observed between W and the BL properties should be thought of as a consequence of a self-reinforcing feedback.
- Figure-5 caption: Why is 567.7 m the optimal altitude for W? Is that where correlation peaks in Figure-3? If so, please specify either in caption or somewhere in text.
- Figure-6 : The contour line labels in panel (A) don't make any sense with repeating numbers. My first guess that the plot might accidentally contain rounded values for the contour line labels also failed seeing the irregular spacing between those integer repetitions, e.g. one -69, three -70s, one -71 and three -72s... The contour line labels in panel (B) look fine.
- L165 and other mentions of "asymmetry" here on: May I suggest "varying" as a more appropriate term than asymmetric? The relationships are both symmetric wrt E and W, that's what the linear analyses done before in fact assumes, i.e. a symmetric response. The difference is in the strength (slope) of the relationships of q and δD , and hence the suggestion "varying relationship" instead of "asymmetrical relationship". The authors themselves clarify this in L180-182.
- Mixed-layer model:
 - I like the "counteraction efficiency" term that the authors have introduced to try and quantify the competing effects of W and E. The use of it, however, in the mixed-layer model is not the same as that interpreted from the observations (see my comment regarding L147-164 and Figure-7). Also, why not use its value obtained from the regressions instead of $\gamma = 1.0$?
 - The steady-state aspect of the model simplifies calculations indeed, however the authors should verify the validity of such an assumption when comparing to the observations. See Figure-4 where one can see both q and isotopic composition vary quite a lot within just 8-10 hours if the data points correspond to circle times from HALO. Thus, at scales being discussed here, the 0 in Eq A.1 is not valid. The authors should therefore note (preferably around L171) that the MLM helps in crudely placing relative importances of the processes, but there can be major deviations at the observed scales in nature due to the transient state of the boundary layer (i.e. the large spread in Figures-5 and 6).
- L180 - 192: This is indeed an interesting demonstration of possible convective-precipitation processes that the self-reinforcing nature of W modulates. That it comes out of simple physics is a joy to see. :) It shows the potential of how physical hypotheses can be derived from stable isotope measurements.
- L194 : "strongly" is based on the 1.19 counteraction efficiency, I believe. So, this might change.
- L202 - L212: I enjoyed this discussion and the different findings in deep v/s shallow convective regimes, although I did not understand the detailed argument of Risi et al (2020). Regardless, I would request the authors to specify the spatial scales that Risi et al (2020) consider. In the last sentence of the paragraph, I am unsure what is meant by the "vertical structure of vertical motion" setting the contrasting observations. Do the authors mean that the difference in dynamics of shallow convection (keeping BL moist) and deep convection (bringing cold and dry air aloft to the surface) results in the contrasts?
- L244: First sentence of paragraph is essentially a repetition from earlier.
- The discussion misses findings of LES studies such as Bretherton and Blossey, Janssens et al (2022) and Janssens et al (2024) which indicate more of a top-heavy moisture variance and almost no moisture differences due to the mesoscale in the sub-cloud layer. The findings from the present study (if held true) is important observational evidence that contradicts the LES findings.
- L259 - L260. I suggest the following change for more clarification. "... our study shows that mesoscale vertical velocities, through self-reinforcing mechanisms, introduce an additional layer of complexity that fundamentally modulates ..."
- For the discussion and conclusion sections, here I don't point out separately parts that discuss the counteraction efficiency (especially its quantification) or other aspects that might need to change based on the authors' response to my previous major comments.

References:

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- Radtke, J., Vogel, R., Ament, F., & Naumann, A. K. (2023). Spatial organisation affects the pathway to precipitation in simulated trade-wind convection. *Geophysical Research Letters*, 50, e2023GL103579. <https://doi.org/10.1029/2023GL103579>