We have addressed the remaining reviewer's comments by adding in the supplemental material a comparison between the decoupling index used in this work and one proposed in Jones et al., 2011and showing specific cases in which the 2 indexes agree and disagree. We also addressed the minor revisions proposed.

I thank the authors for addressing my comments. However, in my view the revised version does not show convincing evidence that the issue of well-mixedness is resolved. A lot of questions remain open for me:

- Has this Decoupling Index been successfully used in other studies? If so, reference to previous work should be given.

Thank you for pointing this out, the following reference has now been added to the discussion (Line 204): Sena, E.T., McComiskey, A., Feingold, G.: A long-term study of aerosol–cloud interactions and their radiative effect at the Southern Great Plains using ground-based measurements, Atmos. Chem. Phys., 16, 11301–11318, doi:10.5194/acp-16-11301-2016, 2016.

- To help the readers' intuition, could the authors show example radiosonde profiles for the different DI thresholds? This could convince me that a DI of 0.25 is indeed a coupled boundary layer.

We agree with the reviewer. Examples of profiles are now added to the supplemental material and mentioned in lines 207-211

- When I look at Fig. 2 of Albright et al. 2022, I find that the pdfs of LCL and the sub-cloud layer top (indicative of cloud base) are usually very close together also in the decoupled boundary layers of the downstream trades near Barbados, with DI's between about 0.03-0.15 (as estimated by eye). Furthermore, Albright et al. 2022 only apply the ML framework to the sub-cloud layer, and not the entire trade-wind boundary layer (i.e. excluding the cloud layer). How does this go together with your study?

There are indeed decoupled cases in which the LCL and the cloud base are close, however these constitute a minority of the cases. When we compare the index used with the index in Jones et al., 2011 they disagree in about 12% of the cases. We have added more analysis in the supplemental material. In our case the ML approximation is necessary as there is no straightforward way to understand the controls of boundary layer water vapor apart from the mixed-layer budget framework. In the manuscript we have mentioned that this assumption is flawed and have discussed the ramifications of such approximation.

- How do the DI's depend on the averaging scale? LCL and CB might be closer together for individual radiosondes compared to averages over multiple sondes / longer times.

The classification of coupled and decoupled cases is not the main focus of the paper but is used here to evaluate the uncertainties due to the assumption of a mixed layer framework at the site. In this perspective a detailed analysis of the decoupling index is beyond the boundaries of this analysis, and we have not investigated these aspects.

- How is cloud base estimated from the Radiosondes? What formula / air parcel properties are used for the LCL computation?

The cloud based was determined from radiosondes relative humidity with a threshold of 0.95% and the LCL was computed from radiosondes using the approximation (eq.2 in Romps, 2017):

$$z_{LCL} = z + \frac{c_{pm}}{g}\left\{T - 55K - \left[\frac{1}{T-55K} - \frac{\log(RH)}{2840K}\right]^{-1}\right\}$$

with z=100 m.

Romps, D. M.: Exact Expression for the Lifting Condensation Level, *Journal of the Atmospheric Sciences*, *74*(12), 3891-3900, 2017.

- For evaluating the influence of the degree of decoupling on the budgets in L327-330, why use the subset with cloud base > 1.2 km, rather than a certain range of the DI?
Unfortunately, we can't calculate the decoupling index of the entire hourly dataset due to a lack of accurate thermodynamic profiles, that's why we calculated it exactly for the radiosondes and used the cloud base height as a proxy for the entire dataset.

-Overall, I wonder whether other decoupling indicators could be more helpful to justify the assumption of coupled well-mixed boundary layers.
Thank you for the suggestions, we repeated the calculations using the index developed in Jones et al., 2011 and added a comparison in the supplemental material.

Other comments/typos:
- L66: mention that Albright et al. 2022 focused on the downstream trades / near Barbados. -Done
- L73: LWP acronym not introduced - Done
- L114f: Say 'Quantities from ERA5' here and in the following, rather than 'from ECMWF'. -Done
- L116: of about -> by about-Done
- L148: better discussed -> discussed in more detail-Done
- L208: large-scale instead of mesoscale processes?- Done
- L210: q_L -> q_l - Done
- L337: in within -> to within -Done
- L341: the moistening from the surface is primarily balanced by advection drying (~50%) precipitation, and entrainment drying (~25%) with the rest compensated through precipitation removal -> should precipitation be removed in the first place? -Yes, thank you-Done
- L380: one-minute -> 1-minute (or just consistently throughout the manuscript)-Done
- L387: is -> are-Done
- Table S2: I think the table would be more useful if some associated mean meteorological conditions would be shown along with it. E.g. T, q, wind, zLCL, zCB, zPBL, cloud cover, precip, LWP... Done

- Fig. 6: 'duration' is actually just wind speed. So why not write wind speed directly? And # of perturbations is just # of samples, as no minimum difference from the mean is specified for defining perturbations. I think it would be better to write explicitly what is shown, as otherwise readers might think it shows sth. different.
The y axis is the time that takes for the 10 km water vapor perturbation to pass over the site. The time depends on the wind speed but does not coincide with it. For example, if the wind speed is 5 m/s a 10 km perturbation lasts about 33 minutes. The perturbation is defined as a deviation from the background state defined with a 100 km lowpass filter. We added the correspondence to wind speed in the figure caption. Duration, rather than wind speed, may be more intuitive on a time/space reference.

- L414: why not sticking with the 10-km scale, why using 10-min now?
The perturbations are still defined using the same 10 km window and are shown in Fig. 6. From these we excluded those perturbations that lasted less than 10 minutes. From Fig. 6 these correspond to PWV deviations of less than 0.01 cm from the background state.

- Sec 5.2: would be valuable to mention / discuss the George et al. 2022 study here (https://doi.org/10.1002/essoar.10512427.1)
Thank you we had missed this relevant reference

- L461: 10^3 and 10^4 what? Updrafts per case/day? Then write e.g. '...in a total of 10^3 and 10^4 updrafts per case/day'

Changed to: "total number of updrafts"