Review of: Lagrangian formation pathways of moist anomalies in the trade-wind region during the dry season: two case studies from EUREC4A
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Manuscript #: 10.5194/wcd-2021-42

recommendation: minor/major revision

This is a thorough study of two types of moist anomaly intrusions seen during the EUREC4A campaign, with the effort made to assess their climatological representativeness. The work is novel. I enjoyed reading this, and appreciate the effort the authors made to provide a polished document that could be easily reviewed along its way to publication.

My main comment is that I wished for more discussion on how well ERA5 can be used to assess the atmospheric aspects the authors most drew on. We are told on page 5 that the ERA5 variables of most import are IWV, precipitation, and CRE. There is no discussion here on their representativeness. Comparisons of ERA5-IWV to the radiosonde-derived values at BCO are shown in Fig. 6, along with precipitation. The IWV comparison clearly indicates moister radiosondes, most likely in the boundary layer (based on fig. 5b and fig. 12b). How about showing IWV comparisons for multiple layers? And why no comparison to CERES-derived CRE values? How much faith should we have in the ERA5 CRE values given the large spread in values reported by the authors - could any of this reflect a systematic over/underestimation by ERA5 of low/mid/high clouds? There is some discussion on p. 8 on how the ERA5 CRE compare to the satellite-derived values in the Bony 2020 study, but to place that discussion there feels adhoc. Better is to devote a section to a more cohesive discussion of the ERA5 data strengths/weaknesses. Is there any literature to draw on that has compared ERA5 variables to data (e.g. AIRS?)? In addition, further along in the manuscript, we see the ERA5 humidity variables, evaporation and liquid water along the trajectories. Why are these not listed on p.5? Surely there is more the authors can say about the strengths and weaknesses of the ERA5 physical variables for their study. And if not, the authors need to at least mention this shortcoming of their analysis.

A further comment is, in attempting to reconcile how an EDI can suppress/encourage shallow convection, it seems to me that there must be some modulation of the temperature profile that either encourages or discourages a (dis)stabilization of the atmosphere. Or is this not the case because the temperature profile equilibrates so quickly in the tropics? I would appreciate seeing some analysis/discussion of the moist anomaly intrusions impacts on the temperature profiles somewhere, primarily for the EDI case as the impact of the shelf clouds detrained as part of the TMD case is easier to understand.

Minor/specific comments:

Abstract: line 10: the abstract jumps into describing the low-level moist anomaly here. Would suggest labeling this as 'case 1' to distinguish this one from the other.
Line11: "Its" I believe would refer to the negative CRE of the previous sentence, as written. Is this the intent?
Line 21: "of the long-range transport" -> "of long-range moisture transport"

p. 3, lines 78-79: I don't quite follow how an EDI is already interacting with PBL air on day 2, is this implying the trajectory is starting at 500-600 hPa? (since they tend to descent 400 hPa in 2 days?)

p. 6, section 2.2: I presume the Lagranto trajectories rely on the ERA5 subsidence velocities. Have these been compared to the EUREC4A dropsonde-circle-derived vertical velocities - is there any assessment known of the ERA5 values?

p. 7 section 4.1: The ‘Fish’ cloud structure described here, similar to others, is bordered by cloud-free regions, suggesting (to me) an enhancement to the subsidence at the mesoscale. Do the authors see cloud features like this in the ERA5 data as well? Would a spatial plot of ERA5 liquid water path provide a useful comparison to the MODIS image shown in FIg. 5?

P. 12 bottom - p. 13 top: this discussion on future work seems a better fit for the end of the manuscript.

P. 14, line 434: the references provided are all to low-level mixed-phase clouds I believe, for which the shortwave CRE will be more dominant. If the authors are able to draw in literature more pertinent to the mid-level mixed-phase cloud CRE that would be more pertinent. I provide some suggestions below but am not sure if those also evaluate the CRE.

Figures:
Fig 2: I believe this is entirely ERA5, regardless it would be useful for the figure caption to clarify.

FIg 3: I do not see the red shadings.

Other potentially useful references:
Casey, S. P. F., Dessler, A. E., & Schumacher, C. (2009). Five-Year Climatology of Midtroposphere Dry Air Layers in Warm Tropical Ocean Regions as Viewed by AIRS/Aqua, *Journal of Applied Meteorology and Climatology*, *48*(9), 1831-1842.
Is their data description consistent with your ERA5 inferences?

Zuidema, P., B. Mapes, J. Lin, C. Fairall, and G. Wick, 2006: The Interaction of Clouds and Dry Air in the Eastern Tropical Pacific. *J. Climate*, **19**, no. 18, pp. 4531-4544.
Their discussion of how the latent cooling from evaporating falling ice particles helps maintain a stability structure that can help sustain the upper-level stratiform cloud deck seems relevant to the TMD discussion on p.13.

Relevant to the radiative impact of mid-level mixed-phase clouds:
Bourgeois, Q., Ekman, A. M. L. L., Igel, M. R., and Krejci, R.: Ubiquity and impact of thin mid-level clouds in the tropics, Nat. Commun., 7, 12432, https://doi.org/10.1038/ncomms12432, 2016.
Sassen, K. and Wang, Z.: The Clouds of the Middle Troposphere: Composition, Radiative Impact, and Global Distribution, Surv. Geophys., 33, 677–691, https://doi.org/10.1007/s10712-011-9163-x, 2012.
Adebiyi, A. A., P. Zuidema, I. Chang, S. P Burton and B. Cairns, 2020: Mid-level clouds are frequent above the southeast Atlantic stratocumulus clouds. *Atmos. Chem. Phys.*, **20**, p. 11025-11043, 10.5194/acp-20-11025-2020