

Weather Clim. Dynam. Discuss., referee comment RC1
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review of wcd-2021-42

Anonymous Referee #1

Referee comment on "Lagrangian formation pathways of moist anomalies in the trade-wind region during the dry season: two case studies from EUREC⁴A" by Leonie Villiger et al., Weather Clim. Dynam. Discuss., <https://doi.org/10.5194/wcd-2021-42-RC1>, 2021

Review of the manuscript "Lagrangian formation pathways of moist anomalies in the trade-wind region during the dry season: two case studies from EUREC4A" by Villiger et al.

The manuscript provides a comprehensive overview of the variability of moisture and clouds over Barbados during the EUREC4A campaign and the underlying processes that govern this variability. Particularly, combined Lagrangian-Eulerian analysis of two moist anomalies during the campaign span the extratropical and tropical systems and flow conditions that influence the BCO site at the trades region. In addition, the authors provide the climatological context for the campaign, allowing to generalize the findings and provide an estimate for the climatological occurrence frequency and magnitude of such moist anomalies.

The topic is of high interest not only for better understanding the observations carried out during the campaign, but for providing important insight on the influence of the extratropics as well as ITCZ deep convection on the trade wind region, i.e., a non-local influence. The combined Eulerian-Lagrangian analysis for the region of Barbados is novel. The paper is exceptionally well written, and exciting to read, despite its length. The figures are excellent and mostly provide full evidence for the written text. However, I have two main exceptions to the last remarks which I would like the authors to address.

1. Quasi-climatological identification of TMDs: the motivation behind finding a similar pathway to the rather peculiar case is not clear. In principle, there can be detrainment from cumulonimbus anywhere in the tropics, not necessarily in the southern hemisphere, and indeed many of the M-local and M-trades of the February 2020 case trajectories do not originate from the southern hemisphere. Please explain why you focus on this specific pathway (which is indeed very specific, as the ice content value shows), or alternatively generalize the tropical detrainment criteria. I actually find the results in section 5.3 to be

somewhat trivial, as they result directly from the definition of TMD days by their trajectories (e.g., pressure and latitude evolution, relation between minimum pressure and IWV). Also given the less robust relation to CRE, and the already many figures, I suggest to skip this subsection along with Figs. 17 and 18, and summarize the quasi-climatological context with less detail and focus on the mid-level anomaly only.

2. The analysis concerning EDIcon/div shows interesting and coherent results regarding the precipitation response to EDI events. However, it raises some doubts regarding the position of the EDI trajectories with respect to the slanted frontal surface. Since the front is identified on the 850-hPa surface and divergence is considered at 950 hPa, it is possible that the EDI airmasses seem to reach ahead of the front to its warm side, but are in fact still at the cold sector since they are located closer to the surface. For example, the bottom panels of S2.4 suggest that there is divergence directly below the warm side of the front over Barbados, where the trajectories reach (or is this partly hidden by the large marker?). It is clear that the con/div conditions prevail locally at the BCO and those are directly related to the IWV and precipitation there. However, the statements about the position of the EDI trajectories with regards to the front need more evidence. As the mesoscale variations of the divergence field near the frontal region can have sharp variations, there is more direct evidence needed to state that the EDIcon trajectories indeed reach the warm side of the front by the ageostrophic circulation. Furthermore, if indeed the EDI trajectories – front location be substantiated, can the discussion on the EDI-front interaction be further related to existing knowledge on kata/ana fronts and precipitation in the midlatitudes?

Additional remarks:

- Line 201-202: it is not immediately clear which balance is referred to here. Please explain.
- Line 205: is the inversion represented also in the temperature profile?
- 8d and accompanying text: it may not be clear that here you refer to surface evaporation since you also mention the evaporation of hydrometeors into the dry airmass. Please clarify this in the text and caption.
- What is the significance of L-DIwcb? Is it needed to separate the analysis?
- Line 275: actually, the specific humidity increases to easily above 10 g/kg, roughly double the typical North Atlantic values (~6 g/kg). Is this due to the relatively high temperature in the tropical region and thus higher specific humidity at saturation?
- 4.3: what is the sensitivity to the 5% EDI criterion? Does the event during 8-9 February 2020 qualify as an EDI event using a lower percentage or a different time span?
- How well is the vertical velocity represented at the BCO region in ERA5 compared to observations from BCO? This is especially important under convective conditions e.g. in Fig. 16d.

Technical corrections:

- Line 273: delete "in".
- S2.4 caption first line: delete "on".
- Line 319: add "y" to "dail"
- 9 and 17 captions: replace "geographical" by "geometrical"
- 14d: mark the location of BCO, as the domain is shifted compared to the other panels.
- 17: correct the pressure levels of the IWV on the y axes.
- Line 489: replace "to" with "from".