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Interactive comment on "Organization of convective ascents in a warm conveyor belt" *by* Nicolas Blanchard et al.

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We thank the Referee for his/her time and his/her constructive comments. We have complied with most of the proposed changes. In the following, the comments made by the referees appear in black, while our replies are in blue.

General comments

This is a very interesting contribution that builds upon very recent work (e.g. Oertel et al. 2020, Harvey et al. 2020) on the structure and dynamical importance of embedded convection within the warm conveyor belts in extratropical cyclones. In this contribution the authors make use of convection permitting numerical models and field campaign observations to investigate the convective activity in the Stalactite cyclone

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observed in 2016. This is without doubt relevant research within the scope of WCD. The paper is very well structured and written, and, in my opinion, the description of the methodology is sufficiently complete to allow their reproduction by fellow scientists. Therefore I recommend the article for publication in Weather and Climate Dynamics after revision. I include a list of comments that could be considered by the authors to hopefully enhance the paper. The most important of these is related to the analysis of on-line trajectories. It would be very valuable if the authors can go deeper into this analysis, and in particular of the mid-level convective anticyclonic trajectories and the strong production of negative potential vorticity.

Specific comments

L366-370 and L462-463: I think aspects of the ascent of the anticyclonic trajectories deserve a lot more explanation. The increase and then decrease of PV in WCBs is associated with the transit of the WCB parcels towards a heating maximum (therefore increasing PV) and the away from it (therefore decreasing PV). To my understanding, this is the case by Methven (2015) in his arguments about the matching PV values between WCB inflow and outflow. The trajectory behaviour shown here is the opposite. By what dynamical means is PV decreasing and then increasing. To me it would suggest the presence of strong cooling. However, the trajectories are strongly ascending by about 4km. It would be very interesting to see the evolution of potential temperature along these trajectories. Further details on the evolution of your trajectories would be a great opportunity to confirm the findings in Harvey et al. (2020). We have checked that the evolution of potential temperature essentially follows the evolution of altitude, i.e., an increase during ascent, which excludes a contribution of strong cooling. We specified that the negative peak in PV encountered by mid-level convection trajectories "differs from the evolution at low levels, which matches the typical increase below the heating maximum and decrease above (Wernli et al. 1997). Instead, the evolution at mid levels is similar to that found by Oertel et al. (2020) for trajectories passing through a region under convective influence." Although the topic appears promising, Referee

1 asks for streamlining the PV discussion thus we prefer not to further develop the analysis here and will focus on the link between midlevel convection and negative PV in another study. Please note that our ascents do not perform a full tropospheric rise, and may therefore differ from the theoretical framework presented by Methven (2015), whose reference has been removed accordingly.

L102 and L108: Please include more details on the initial position of the passive tracers to clarify statements such as '...at each grid cell...' or 'they do not necessarily start in the BL'. Thinking about mismatch between modelled prognostic variables (e.g. Whitehead et al. 2015 doi:10.1002/qj.2389, Saffin et al. 2016 doi:10.1002/qj.2729), is there any indication on how accurate these trajectories are? The sentence Line 102 has been rephrased to "Lagrangian trajectories are computed from three online passive tracers defined at each grid cell of the simulation domain (Gheusi and Stein, 2002). The tracers are initialized with their initial 3-D coordinates and are transported by PPM, a scheme with excellent mass-conservation properties and low numerical diffusion."

L110: While I realise that the clustering method is described in Dauhut et al. (2016) it would be useful to have a few more details in this work. E.g. what connectivity rules are being used in this work? The grid spacing in this work is very different from that. Would it be appropriate to use the same rules? We added "Two grid points sharing a common face, either horizontally or vertically, were considered connected, while diagonal connections were considered only vertically. No size criteria were applied". The clustering tool is the same as that applied by Dauhut et al (2016) to identify updrafts, with the exception of the much higher threshold of 10 m s-1, since the focus of this study was on updrafts reaching the stratosphere.

L125-126: 'The WCB ascent region. . .' Is this not just the cyclone's warm sector? Or is there any reason to think that this is just the portion in the warm sector affected by WCB ascent? The warm sector spreads over a much larger area. The area we focused on is really the portion in the warm sector affected by WCB ascent. This result is demonstrated with our trajectory analysis.

L145 and L157 and L445: It is stated that e.g. "[The trajectories] number more than 500000". I'm not clear on how meaningful the number is. As a reference, can you give the initial number of trajectories? While the number is impressive it would be good to have a sense of what it means in physical (mass, volume) terms. We added "(out of nearly 3 million tropospheric trajectories contained in the red box, which means that about one sixth are ascending)".

L161-162: "... corresponding to continuous slantwise ascents in WCBs (i.e., 600 hPa in 48 h..." This is slightly misleading as this is a criteria imposed on the trajectories. It doesn't mean that continuous slantwise ascent has to occur or is even defined in this way. Furthermore in L175 slantwise ascent does not exceed 250 hPa ascent in 12 h. Perhaps you meant 150 hPa? To be more accurate, we changed to "... corresponds to the typical slantwise ascent rate used for the identification of WCBs (i.e., 600 hPa in 48 h...".

L166-168: "Using a high ascent rate of 400 hPa in 2.5 h considered as convective, Rasp et al. (2016) found 55.5% of trajectories meeting the threshold for an autumn storm over the Mediterranean Sea but none for a winter case over the North Atlantic". It's not clear what should be concluded from this. The proportions are different between cases and one didn't show strongly ascending trajectories. How is this to be interpreted? We added "This shows that the proportion of fast ascents and their intensity varies a lot from case to case".

L170-171: "This choice is motivated by the objective of determining the nature and characteristics of fast ascents". Please clarify in what way are the motivation and the chosen threshold linked. The threshold seems justifiable, but arbitrary to me. What would've changed if the definition was different? We added "The specific value of the threshold has been set at a value equal to that used by Oertel et al. (2019) for comparison purposes. The use of another threshold would lead to a change in the proportion between slow and fast ascents."

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L295-297: “This discrepancy shows that the identification of fast ascents based on a pressure criterion focuses on lower levels, so that high vertical velocities at higher levels may not be identified as fast ascents”. I’m not clear on the point that is being attempted here. Does this mean that the clustering analysis is to be preferred to trajectory analysis? There is no preference to be expected here because the approaches are radically different (Eulerian for clustering analysis and Lagrangian for trajectory analysis). The point we wanted to make is the difference in terms of vertical velocity intensity when expressed in m s^{-1} or hPa $(2h)^{-1}$. According to the hydrostatic equation, a vertical velocity of 100 hPa ($2h^{-1}$) (the criterion for fast ascents) is equal to 0.12 m s^{-1} at the surface (using a air density of 1.2 kg m^{-2}) and 0.3 m s^{-1} at 300 hPa (using an air density of 0.45 kg m^{-2}). In the latter case, updrafts with such high values are rare (see Figs. 8 and 10). We added "(a value of 100 hPa $(2h)^{-1}$ is equal to 0.12 m s^{-1} at the surface and 0.3 m s^{-1} at 300 hPa)".

L337-339: Related to the previous comment: I think I’m confused here and I’m sure it’s just a matter of rewriting: “Once again, these high-level isolated convective structures are not co-located with rapid segments (black dots) and thus not further discussed here”. This seems to contradict the point near L295 about preferring clustering analysis! Again, we do not have any preference. See our response to your previous comment.

Technical corrections

L10-12: In the context of the abstract alone, this sentence is a little obscure as it talks about specific thresholds and cyclonic flow at lower levels. Perhaps the authors can expand a bit to explain e.g. the meaning of the threshold and the expected behaviour. Is cyclonic flow what was expected? The sentence was developed for clarity.

L27: ‘WVB’ should read ‘WCB’. Fixed.

L32-33: I wonder whether Joos and Wernli (2012) would be a suitable reference here. Added.

L118-119: The exact value here and the colour bar in the figure are enough in this case as the contrast is clear. Thus, I would delete the vague colour description from the text e.g. ‘reddish colours’. Removed.

L119-120: “The WCB outflow region IS EXPECTED to be located. . .” It is not clear whether this is what actually happens. Perhaps ‘expected’ should be deleted? Removed.

L125: For clarity rewrite the sentence. I suggest “.. some discrepancies in the BT values when compared against MSG observations can be found locally.” Changed to "although with larger extent compared against MSG observations".

L129-130: It’s not clear how comparable the 6-hourly and 1-hour MSLP locations are. Perhaps you can compare 6-hourly versus 6-hourly to then discuss the hourly data once the comparison has been done. Figure 1 shows 6-hourly MSLP locations with dots for ECMWF and the simulation allowing a direct comparison between the two sets.

L134-135: The Meso-NH simulations are driven by ECMWF analysis so that the simulation predicts well the ECMWF analysis track is not that surprising. We agree. This is no surprise because Meso-NH is a state-of-the art model.

L137: Please clarify how the trajectories are counted. Are they counted throughout the 12-h window, or are they counted at 1600 UTC? Changed to "The location of air parcels fulfilling the ascent criterion of 150 hPa in 12 h is shown at 16:00 UTC as their spatial frequency".

L142: Change ‘peaks’ for ‘maxima’. To me ‘peak’ denotes a particularly sharp and spiky maxima. Changed.

L144-145: It is not clear whether the mask is actually the red box in Fig. 2. The sentence has been rephrased to "The red box in Fig. 2 is used as a mask to select the ascents in the WCB region at 16:00 UTC"

L165-166: "... in another NAWDEX cyclone". You can be specific on the cyclone in the Oertel et al. (2019) study. In fact, you are specific in the conclusions (L453), but just not here. Changed to "in the NAWDEX Cyclone Vladiana".

L170: I suggest changing to the following so that the elements in the sentence are consistent: "The ascent of trajectories that do not meet this criterion is defined as slow.". Changed according to your suggestion.

L193: What criteria was used to decide on whether a trajectory was cyclonic or anticyclonic? The definition was given by a sentence L196, which was moved to L194.

L203: Add 'trajectories' after 'slow ascent' or change to 'slowly ascending trajectories'. Changed to "few slow ascent trajectories".

L204-205 and L444: This case has many contrasting features to that described in Martinez-Alvarado et al. (2014). In that work it was the anticyclonic branch that exhibited faster ascent than the cyclonic branch. Accordingly, trajectories ascending faster reached higher isentropic levels. It would be good to know the implications or sources of these contrasting features. Is it just case to case variability or do you think it's deeper than that? We agree. The differences between the case of Martinez-Alvarado et al. (2014) and our study are numerous. We believe that it is a case by case. The study of a larger number of cases is necessary to be able to estimate the general character of these results.

L268: Delete "given by the iso-0 C". Removed.

L314-319: What about the melting level? It doesn't seem to be as clearly defined in the simulations as in the observations. Is this important or not? Why? We added "The bright band is less defined than in the observation suggesting too little simulated melting of snow into rain." This signal is more stratiform than convective. Its importance in relation with the convective updrafts discussed in the paper is therefore small.

L342: Add 'Each one of. . .' at the beginning of the paragraph. I hope this is what you meant here. Ignore if not. Ignored.

L415: It should read ‘overflown’. However the sentence sounds a little odd. It might be worth separating into two sentences. Rephrased.

L441: “WCB trajectories” Are these really WCB trajectories or simply ascending trajectories satisfying the selection criterion? Changed to "ascending trajectories satisfying the selection criterion".

Figure 1: If possible, choose a different colour for the theta_e contours so that the colour is not part of the colour scale. The color scale includes many colors, except white. This is what motivated our choice to use of white to draw the theta_e contours.

Figure 4: It would be helpful to have an altitude v time plot. In the current panels it’s difficult to get a clear picture on where the rapid ascent occurs or the differences in altitude between the two types of ascent. Figure 4 shows the trajectories, but for a random sample only, while Fig. 6a shows the plot of the altitude as a function of the time you request and this for all trajectories.

Figure 6: A way of presenting this that has proven useful in other studies is aligning the trajectories according to their time of maximum ascent to highlight the PV behaviour for a typical trajectory. Thanks for your suggestion. However, we removed the time evolution of PV.

Figure 7 and other figures showing vertical sections: In general, the features below 2 km are very difficult to distinguish. The double hatching is very difficult to see. It might be worth including separate figures for the lowest levels. Somehow the black dots do not look like dots but like incomplete symbols, so I’m not sure whether the image is displaying properly on my screen or not. We improved the readability of these figures. In particular, double hatching and yellow contours have been removed in the vertical sections.

Figure 7 caption: Change ‘iso-0degC’ for ‘0degC’ or simply ‘melting’. Changed to "melting".

Figure 11b: It's very difficult to distinguish the overlapping shading. It might be worth separating into four or perhaps two panels for clarity. We decided to group the trajectories into only three categories. This improves the readability of overlapping shading.

Figure 13: This figure is very difficult to read. Any attempt to simplify it would be much appreciated. We improved the readability of this figure.

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