Adaptations to

Warm conveyor belt characteristics and impacts along the life cycle of extratropical cyclones: Case studies and climatological analysis based on ERA5

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In the following, the comments of the reviewers are shown in **black** and the corresponding adaptations in the revised version of the text in blue.

Reviewer 1

General comments

The main results of the paper are unclear. This is partly due to a strong focus on the methodology and to the lack of proper conclusions in Section 5, which discusses specific results without much hierarchy and misses more general statements (only one sentence about the results in Section 5.4).
In order to increase the clarity of the paper, we introduced three main changes: 1) We refined the research questions (l.97–99) and specifically added the development of an objective method to quantify the WCB characteristics and impacts, 2) two of the three cases studies were moved to the supplementary material and 3) we restructured the discussion in a way that each research question is answered specifically (Sect. 5).

The structure of the paper is imbalanced. On the one hand, the case studies are too detailed: the description of single panels at multiple times is repetitive and should be streamlined, and similarities and differences between case studies should be emphasized rather than each case described individually. On the other hand, case studies are helpful to illustrate the climatological analysis but it is unclear what should be learned from the

case-to-case comparison beyond the existence of a case-to-case variability. In this regard, the comparison in Section 5.1 is too detailed and appears too late in the paper. An alternative structure would be to present the climatology first, then to (briefly) discuss the case studies in light of the climatology to emphasize their peculiarities.
See previous comment, we moved two of the three case studies to the supplementary material and only briefly discuss the differences between the cases in the main text. This hopefully helps the reader to follow the story line.

The methods are complex, based on several steps and each involving some form of (arbitrary) criterion, which makes results hard to interpret. On the one hand, the methods would benefit from a general summary of the main steps and motivation. On the other hand, the complexity prevents easy interpretation and comparison with previous studies. The numerous metrics (e.g., number of trajectories, low/high-level PV) are defined in a too complex way to be informative per se, thus must be discussed to compare case studies or time steps only. Also, each and every criterion cannot be the subject of a sensitivity test but it must be clarified what is taken from previous studies and what is not (and why). These points are shortly mentioned in Section 5.3 but without much discussion and quite late in the paper.
We hope to reduce the complexity by including a brief summary of the outline of the approach (l.122–128), supported by a schematic of the most important steps (Fig. 1) and added further information regarding some of the metrics.

Specific comments
Title The word "impact" has different meanings and is usually understood as casualties and damages; what kind of impacts is expected here?
We opted to retain the term "impact" but clarify in the first sentence of the abstract (l.1) that this study specifically focuses on *meteorological* impacts.

l. 1 "global investigation": although the approach is global, as illustrated by Fig. 1 and in the supplement, both case studies and climatological results focus on the North Atlantic only
Rephrased: "The approach was applied globally, but this study focuses on the North Atlantic, one of the regions where WCBs ascend most frequently." (l.8–9).

l. 5 see above comment on impacts
Rephrased: "[...] quantify the key characteristics (intensity, ascent rate, and ascent curvature) and meteorological impacts (precipitation and potential vorticity (PV) anomalies) [...]" (l.5–6).

l. 34–35 Is there a reference for the second part of the sentence, or is it a hypothesis?
Rephrased: "[...]which is likely also reflected in a large case-to-case variability of the

characteristics and impacts of WCBs." (l.36–37).

l. 36–38 It is important to define the meaning of "characteristics" and "impacts" for the paper but this short paragraph is rather vague; many examples are mentioned in the next two long paragraphs, after which a clear definition of the scope of the paper would be helpful.
Rephrased to "This brief summary emphasizes the key characteristics of WCBs and their impacts on surface weather, the evolution of the associated cyclone, and the downstream flow." (l.88–89).

l. 84–85 The distinction between questions 1 and 2 is not obvious
Rephrased research questions, the first questions now specifically adresses the development of a systematic approach to quantify WCB characteristics and impacts (l.97–99).

l. 99–101 What is new compared to the WCB climatologies cited above?
No changes made, see reply for comment.

l. 125 The resolution (6 hourly and 80 km) appears to be taken from ERA-Interim; this is fine but may deserve some comment.
Rephrased: "The temporal and spatial resolution of the starting positions of trajectories in ERA5 is consistent with the approach of previous WCB-related studies (Madonna et al., 2014a; Binder et al., 2016, 2023; Joos et al., 2023)." (l.139–140).

l. 132–133 What is the difference between "at any time during the 48-hour ascent" and "strictly between the start and end of the ascent, 48 h later"?
Added: "A detailed description and comparison of the two sets of WCB trajectories can be found in Heitmann (2023)." (l.146–147).

l. 135–136 The sentence is confusing
Rephrased: "Secondly, Madonna et al. (2014) only took WCB trajectories into account that were collocated with a cyclone mask at least once during the ascent. In contrast, the adapted definition considers every trajectory in a bundle of trajectories if at least one of them coincides with a cyclone mask at least once during the ascent (as described above)." (l.151–154).

l. 142–244 This is interesting but questionable, as several criteria are different, as well as the dataset
Added reference to Heitmann (2023) for more indepth investigation (l.160–164).

l. 159 Any motivation for this value?
Added: "The selection of the radius value of 100 km is motivated by the grid spacing of

the trajectory starting position (80 km)." (l.181–182).

l. 168–170 This sentence is disconnected from the rest
The sentence was relocated to an earlier position within the paragraph (l.183–187).

l. 175–176 Is the "enhanced frequency of WCB inflow in the region of the storm tracks" not merely a consequence of "a minimum of one trajectory per WCB bundle must at least once coincide with a cyclone mask during its 48-hour ascent"?
Rephrased: "[...] enhanced frequency of WCB inflow (Fig. **??**a,b) in the region of the storm tracks and on the respective winter hemisphere (linked to the previously mentioned condition that each WCB trajectory bundle must coincide with a cyclone mask at least once)." (l.195–196).

l. 176–181 The frequency values require some kind of calibration, otherwise they are hardly usable as such.
Added: "Directly comparing the absolute values of WCB frequency derived from WCB trajectories or WCB masks poses challenges due to inherent differences in their definition." (l.196–197).

l. 181–187 This supports the use of vertical position instead of relative time but has little to do with the use of WCB masks, which requires more motivation considering the above limitations
Rephrased: "The approach of previous WCB climatologies, which took the position of a WCB trajectory at a fixed time instance, disregards any information about the vertical position and is strongly dependent on the ascent behavior of a trajectory. (l.204–206).

l. 197 Lagrangian properties to contrast with the following Eulerian properties?
Adapted: "In general, WCB characteristics are Lagrangian properties, calculated based on the WCB trajectories defining a WCB mask. In contrast, WCB impact metrics are Eulerian properties, derived from the statistical value of a variable [...]" (l.218–221).

l. 231 The proportion of non-curved trajectories is quite high (two third of the total), while a number of them seems to follow the anticyclonic ones on Fig. 3
Added: "Furthermore, a clear separation between the non-curved and anticyclonically curved branches is often difficult and artifical as the eastward direction of the large-scale flow leads to an anticyclonic curvature of both branches." (l.254–256).

l. 223 Why the asymmetry?
Added: "The asymmetry in threshold values arises from the observation that the trajectories of the anticyclonic branch frequently exhibit an initial cyclonic ascent." (l.247–248).

l. 232 Altitude is not the best name for a pressure value
Changed to “Outflow pressure level” (l.259).

l. 261 The Bergeron unit is not defined
Added: “A deepening rate of 1 Bergeron corresponds to a deepening of 24 hPa in 24 hours at 60°N.” (l.292).

l. 288–289 Repetition of the reference
Adapted: “[...]a deepening rate of 3.4 Bergeron, as described by Neiman and Shapiro (1993). The latter study, along with Neiman et al. (1993), provides a detailed description [...]” . (l.4–5 supplementary material).

l. 303 The location of the developing cyclone is hardly seen on Fig. 3a
Position of cyclone center denoted in all trajectory plots by a letter (’C’).

l. 310 Same comment as above, and is the cold front shown somewhere?
No changes made, see reply for comment.

l. 320–330 “almost perfectly”, “considerably”, “most likely not yet strongly”: overstated
We changed “[...] low PV values at low levels (750–950 hPa), which are most likely not yet strongly enhanced by latent heating.” (l.328–329) to “[...] low PV values at low levels (750–950 hPa), indicating weak diabatic PV production at this point.” (l.45 supplementary material).

l. 337 This is hardly seen on Fig. 5f
Rrephrased to increase clarity.

l. 343 Remind the definition of ULPVA?
Added: “(median of the PV anomaly between 200–375 hPa within the WCB outflow mask, see Sect. 2.2.3).” (l.59–60 supplementary material).

l. 350 What is “because of the low altitude and latitude of the WCB outflow in this region”?
Added: “Weak or even positive ULPVA at relatively low latitudes are linked to the equatorward reduction of the climatological PV at upper levels and are consistent with Madonna et al. (2014).” (l.66–68 supplementary material).

l. 355 In what sense is it similar?
Rephrased to “To conclude, we found that both the characteristics and impacts associated with the investigated WCB co-evolve during the cyclone life cycle.” (l.73–74 supplementary material).

l. 380–382 The WCB impact on cyclone intensification is disputable, as both WCB intensity and cyclonic proportion are delayed compared to the deepening rate
Rephrased to "Considering PV, during the cyclone's intensification phase, LLPV increases to maximum values above 1.5 pvu" (l.98–99 supplementary material).

l. 416–418 It is surprising to realize that the chosen case was illustrated above but not mentioned
No changes made, see reply for comment.

l. 420 Of which trajectories?
Rephrased to "However, the pathway of the trajectories of Martínez-Alvarado et al. (2014) is very similar to the trajectories shown in Fig. 5b." (l.347–348).

l. 425–426 Cyclonic or anticyclonic branch in Martínez-Alvarado et al. (2014)?
Rephrased to "Compared to W2, W1 has higher initial values of q (about 9 vs. $8\,\mathrm{g\,kg^{-1}}$) and experiences stronger latent heating such that its outflow occurs on a higher isentrope (about 310 vs. 305 K), consistent with Martínez-Alvarado et al. (2014)" (l.350–352).

l. 487–489 This sounds speculative
Rephrased to "The resulting PVOL (Fig. 7e, dark blue) peaks 18 h after the peak in PQ90 and a day before the WCB reaches its maximum intensity." (l.410–411).

l. 492–495 This discussion breaks the flow and does not appear too relevant as PV is followed in the WCB mask but not along trajectories here
Discussion of link between Coriolis parameter and PV removed.

l. 503–507 This case study should likely be presented first, as it is discussed and illustrated in Sections 1 and 2 as archetypal WCB
Case study 1 (IOP4 storm, January 1989) and case study 3 (November 1992) were moved to supplementary material, only case study 2 (November 2009) will be discussed in the main text.

l. 525–539 The described features (frontal wave, secondary airstream, trajectories ascending at lower latitudes) are interesting but not easy to identify on Fig. 10
Rephrased.

l. 583–584 Unclear
Rephrased: "The temporal delay is linked to the manual attribution of the trajectories to the later emerging cyclone." (l.184–185 supplementary material).

l. 593 Why does "the movement of the WCB ascent region from low to high latitudes explain the decrease in the WCB ascent rate with time"?
Rephrased: "The decline in the ascent rate of the WCB could potentially be attributed to the generally greater probability of convective and rapid ascent occurring at lower latitudes. However, we did not explore this hypothesis further." (l.195–197).

l. 618 A comparison of the three case studies is expected here
The comparison of the case studies was shifted from the discussion section to after the completion of the case study.

l. 636 Why is it "intriguing"?
Added: "This finding is intriguing as it highlights the robustness of the link between cyclones and WCBs that is seemingly unaffected by the difference in the data set, WCB definition and cyclone identification algorithm employed in Eckhardt et al. (2004) compared to the present study." (l.493–495).

l. 671 The contrast looks quite weak
Rephrased: "Moreover, the WCB ascent rate decreases by about $8\,\mathrm{hPa}\,(3\,\mathrm{h})^{-1}$ between $t_{rel} = -48\,\mathrm{h}$ and $t_{rel} = 48\,\mathrm{h}$." (l.528–529).

l. 675–676 Not sure what to learn from this and cyclone intensification lasts for longer than 6h
Rephrased: "Overall, within the six hours of the most intense cyclone intensification ($t_{rel} = -3\,\mathrm{h}$ to $+3\,\mathrm{h}$)". (l.534–536).

l. 683 "very likely": is it or not related to intense convective precipitation?
"Very likely" removed as a separate analysis showed that the general decrease in total precipitation is mainly linked to a decrease in convective precipitation (l.542).

l. 694–697 This questions the relevance of the ULPVA metric, which likely depends on the number (intensity) of WCB outflow trajectories but also on the extent of the corresponding mask
Rephrased: "A more detailed analysis of the present data set of WCB characteristics and impacts (Heitmann, 2023) showed that the WCB outflow intensity at one specific moment in the cyclone life cycle ($t_{rel} = 12\,\mathrm{h}$) correlates negatively ($r = -0.57$) with the ULPVA. However, other factors, such as the climatological PV at upper levels at the location of the WCB outflow mask seem to exert a more pronounced influence on the temporal evolution of ULPVA." (l.556–560).

l. 698–717 This detailed description of supplementary figures likely belongs to the supplement

Discussion of differences between the temporal evolution of WCB ascending in different ocean basins moved to supplementary material. Added: "Please refer to Fig. S10 in the supplementary material for a brief comparison of WCBs ascending in the North Atlantic, North Pacific, South Atlantic and South Pacific." (l.574–575).

l. 723 Panels g-i in Figs. 5, 8, 11
Adapted accordingly.

l. 741 "lowest" is misleading for the highest pressure value
Rephrased: "The cyclonic WCB branch ascends to the lowest altitudes (highest pressure levels, 345 hPa, median) [...]." (l.599–600).

l. 748–750 This very short summary does not support the need for detailed case studies
Only the second case study (November 2009) will be presented in the main text, the first and third case studies are moved to the supplementary material.

l. 756–799 At that point of the paper, general conclusions are expected about what should be learned from the case studies, rather than a detailed listing of case-to-case comparison
Discussion of differences between the WCB case studies after presenting case 2.

l. 762 larger but opposite
Rephrased: "In the IOP4 storm of 1989, the WCB was first detected in the ascent layer (500–800 hPa) about 12 h after cyclogenesis. In contrast, in the 1992 and 2009 cases, the WCBs ascend 18 h and 60 h before cyclogenesis." (l.433–435).

l. 806–813 This is interesting but contradicts the WCB contribution to cyclone intensification by diabatic low-level PV production discussed everywhere else in the paper
Added: "The findings regarding the WCB ascent appear consistent with previous research that found a positive correlation between WCB intensity and the deepening rate of cyclones, attributed to low-level diabatic PV production (Binder et al., 2016)" (l.647–649).

l. 814–818 This is also interesting but is not mentioned before, thus does not summarize results
Added: "This decrease is potentially associated with convection embedded in the WCB ascent during the early stages of the cyclone life cycle when it is located at low latitudes, as detailed in the third case study provided in the supplementary material." (l.529–531).

l. 833–834 Any explanation for this?
Added: "This temporal shift is potentially linked to the slow decrease in WCB intensity

after the cyclone intensification phase." (l.676–678).

l. 835–840 This suggests that the latitudinal dependence of the Coriolis parameter is solely responsible for the LLPV evolution, while the WCB evolution discussed in this paper does not play any role
Rephrased: "This increase in LLPV is likely the result of strong low-level diabatic PV production caused by the ascent of the WCB, as well as the poleward displacement of the WCB during the cyclone life cycle that goes along with an increase in f and hence absolute vorticity." (l.680–682).

l. 848–850 This sounds speculative
Rephrased: "We found that the ULPVA becomes slightly stronger when the WCB outflow intensity peaks about a day after the strongest cyclone intensification. However, it remains uncertain whether the WCB's most important impact on downstream large-scale flow coincides with this specific moment in time." (l.689–692).

l. 867–869 Why not try them?
Added: "[...] we are confident that the qualitative findings presented in this study are not dependent on the definition of the metric. Furthermore, there might also be other characteristics and metrics that would be worth investigating but beyond the scope of this study." (l.711–713).

l. 870 The purpose of this subsection is unclear, as it summarizes the methodology rather than the results (which are already summarized in 5.1 and 5.2)
Rephrased research question that specifically address the development of a systematic method to quantify WCB characteristics and impacts.

l. 874, 878 novel vs new climatology
No changes made, see reply for comment.

l. 882–883 positive PV and negative PV anomalies
Rephrased: "In terms of WCB-related impacts, we focused on [...] positive PV at low levels [...] and negative PV anomalies [...]" (l.725–726).

Figs. 6, 9, 12 Changing scales between figures does not help comparison
Note added regarding the change in scales in Fig. S6.

Fig. 15 When two curves show the same variable, a common scale would be more appropriate
No changes made, see reply for comment.

Reviewer 2

MAIN COMMENTS

1. Diagnosing upper-level PV anomalies.

On lines 252 – 254, the method for diagnosing PV anomalies is described as follows. " To quantify this impact, we first vertically average PV at all grid points inside a WCB outflow mask between 200– 375 hPa. The monthly 42-year climatology of vertically averaged PV over the same pressure range is then subtracted to get a PV anomaly. The subsequent upper-level PV anomaly (ULPVA) is defined as the median of the anomaly values of all grid points inside the WCB outflow mask." I am concerned that this method does not isolate the diabatic contribution (as implied on line 250). Would not an amplified ridge be guaranteed to host negative ULPVA? It is difficult to see how this metric could distinguish adiabatic Rossby wave amplification from diabatic enhancement. Some discussion and context would be helpful.
Added: "Note that this approach quantifies the total negative PV anomaly in the region of WCB outflow and therefore does not allow assessing the importance of diabatic PV destruction. However, we are confident that the chosen approach can provide valuable insights into the impacts of WCBs at upper levels." (l.284–287)

2. Masking technique

The WCB masking procedure (e.g., as illustrated in Figure 2) identifies the "impact" area to contain all points within a 100 km radius of particle trajectories. I support the rationale for defining an extended "impact" area to associate WCBs to precipitation and PV modification. I have concerns about the appropriateness of using a circular area drawn around trajectories, specifically for PV. Many particles in the WCB outflow are likely to accumulate along the edge of the tropopause (i.e., along the periphery of the downstream ridge). This is a region of very large PV gradient. Is there a concern that the circular mask encompasses a volume of air that is on the poleward side (i.e., above the tropopause)? Wouldn't this create a very large positive bias in the estimated ULPVA? Have the authors experimented with smaller masks? How sensitive is the ULPVA to the radius of the mask? Perhaps the masking is more appropriate for precipitation and less appropriate for PV?
Added: "Furthermore, taking the median of all grid values proves resilient to outliers, e.g. very large PV values close to a strong PV gradient." (l.283–284) and motivated the choice for the inflation radius of 100 km "[...] by the grid spacing of the trajectory starting position (80 km)" (l.181–182)

3. Variance in WCB characteristics

Even a small subset of cases, like that presented in Section 3, demonstrates a large case-to-case variability in WCB characteristics. Despite this variance, this paper also demonstrates that there are some robust similarities (e.g., in the relationship between storm intensification and WCB intensity). While this paper highlights those robust similarities, it devotes less attention to the variance. This is perhaps something for a future study, but I'd be interested to know more about the variance. For example, how much is explained by low-frequency modes of variability (e.g., PNA, NAO)? Is there any clustering of characteristics (e.g., are there distinct groupings of storms with similar cyclonic vs. anticyclonic branch structures)? This dataset is begging for such an analysis to be performed.

Rephrased and added: "First, it could be rewarding to investigate the large variance of WCB characteristics and impacts observed in the present study. Heitmann (2023) analyzed the statistical relationships between WCB characteristics and impacts, e.g., the correlations between WCB intensity and PVOL or between ascent rate and precipitation rates. Further sensitivity studies could employed to validate the identified connections between WCB characteristics and impacts." (l.741–745).