Response to reviewer comments for

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

by Katharina Heitmann, Michael Sprenger, Hanin Binder, Heini Wernli, and Hanna Joos

September 29, 2023

In the following, the comments of the reviewers are shown in **black** and our replies in blue.

Reviewer 1

Review of "Warm conveyor belt characteristics and impacts along the life cycle of extratropical cyclones: Case studies and climatological analysis based on ERA5" by Katharina Heitmann, Michael Sprenger, Hanin Binder, Heini Wernli, and Hanna Joos

The paper presents both a climatological analysis and three detailed case studies of the evolution of warm conveyor belt (WCB) characteristics associated with extratropical cyclones in the wintertime North Atlantic. WCBs are first identified from Lagrangian trajectories computed in a systematic way in the ERA5 reanalysis, then different metrics are obtained for the characteristics of the WCB inflow, ascent and outflow using masks based on the trajectories. In agreement with previous studies, the results show a link between WCB ascent and cyclone intensification and precipitation at lower levels, and WCB outflow and ridge building at upper levels, where the cyclonic, anticyclonic and non-curved WCB branches are clearly distinguished.
The text is well written and the figures are of high quality. The merits of the study lie in its systematic and comprehensive approach using numerous metrics applied to about 5'000 cyclones and corresponding WCBs. This allows clear and robust results for the wintertime North Atlantic climatology, which are generalized to some extent to other

oceanic basins. The downside of the approach is that the long description of complex methods and their application to both climatology and case studies may lose the reader. The main ideas and results are blurred in these sometimes lengthy descriptions, which involve many details but somehow miss the general picture.
Thus, I recommend the paper for major revisions before it can be considered for publication. General and specific comments are listed below to help improve the organization of the paper.

Dear reviewer,
We thank you for your constructive feedback. We understand your general concerns regarding the rather detailed description of the methods and case study, which potentially distracts readers from the main results and conclusions. To address these issues, we plan to revise the manuscript in the following way: (i) we will move two of the case studies to the supplementary material to maintain a more focused main text; (ii) we will more specifically and clearly rephrase our research questions to ensure they align better with our study's objectives; (iii) the latter point will also help us to present our key results and conclusions more clearly and concisely. We hope these adjustments will enhance the overall quality of the paper. We appreciate your feedback and look forward to submitting the revised manuscript.
Kind regards,
Katharina Heitmann on behalf of all authors

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).
We thank the reviewer for raising this point and we agree that we can improve the clarity of presenting the main results. As mentioned before, we will ensure that the conclusion section provides clear and concise answers to the refined, specific research questions posed in the introduction, allowing us to present the main conclusions of the paper more effectively. We believe that these adjustments will lead to an overall more coherent and concise presentation of our study's key findings.

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.

We agree that readers could be distracted by the detailed description of three case studies. However, we still believe that the selected case studies are essential to illustrate the developed method as well as the variability of WCB characteristics and impacts. In this sense, the case studies serve two purposes: they allow us to introduce/illustrate the sophisticated methodology, and they also point to the considerable case-to-case variability. Still, to make the main text more concise, we will move the first two case studies to the supplementary material and refer any interested readers to this section. The main text will thus only include case three and a brief description of the differences between the first two cases, as suggested by the reviewer. With this, we are still able to illustrate and introduce the method in detail and hint at the large case-to-case variability, without distracting the reader from the following parts of this paper.

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 thank the reviewer for his/her observation that the developed method might be challenging for readers to grasp. We agree that the developed method is rather sophisticated, involving several steps and 'empirical' thresholds, and is potentially difficult to follow. However we are convinced that the combination of the Lagrangian (trajectories) and Eulerian framework (masks) is highly valuable to quantify the characteristics and impacts of WCBs and it enables us for the first time to describe the WCBs with meaningful metrics along the evolution of the associated extratropical cyclone. We do not see an obvious way to simplify it. However, to help the reader grasping the key steps, we will include a schematic that summarizes the main steps of our method. This visual aid will be applied to all WCB metrics discussed in this study, making the methodological process more accessible to the reader. Furthermore, we will provide a clearer explanation of our choices regarding thresholds and definitions. The chosen thresholds are primarily based on a large number of case studies and previous research related to WCBs. We agree with the reviewer that the selection of these parameters should become more transparent to the reader. We hope that these revisions will make our methodology more comprehensible and easier to follow.

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 appreciate the reviewer’s concern and we acknowledge that the word impact can be misunderstood. We will evaluate different alternatives, such as “implications” to substitute the word “impact” in this study and hope that this better describes that nature of the metrics that we consider. These “WCB-related implications” primarily pertain to precipitation formation and PV modification in both the lower and upper troposphere. This change in terminology should help eliminate any ambiguity and ensure a more precise understanding of the primary focus of our research.

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

We thank the reviewer for this comment. We will clarify that while the WCB masks and their characteristic metrics are available globally, our analysis in the paper will specifically focus on WCBs in the North Atlantic. A comparison to other regions, of course, would be very interesting, but would additionally extend the already rather long paper. Still, to make some first comparison feasible to interested readers, we will make some further, non-North-Atlantic results available in the supplementary material.

l. 5 see above comment on impacts

As mentioned before, we will substitute the word “impacts” with the word “implications” in the entire paper.

l. 34–35 Is there a reference for the second part of the sentence, or is it a hypothesis?

We thank the reviewer for this comment. The described variability of WCB characteristics and implications is an observation from studying previous WCB case studies. To ensure accurate representation, we will rephrase the sentence accordingly to emphasize that this study investigates this potential variability in WCB characteristics and implications for the first time in a very systematic and objective way.

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.

We thank the reviewer for bringing up this point. We hoped that the paragraph in lines 85-92 (after the examples of WCB characteristics and implications) would describe the scope of the paper concisely as “a systematic climatological analysis of WCB characteristics and impacts is currently missing“. We will add a sentence that clarifies that these characteristics and implications of WCBs refer to the previously mentioned examples. This will hopefully help the reader to more clearly understand the scope of this study.

l. 84–85 The distinction between questions 1 and 2 is not obvious
As mentioned before, we agree that the chosen research questions were not perfect in capturing the objectives of this study. Thus, we will rephrase the research questions to: 1. How can we quantify the main characteristics and implications of WCBs? 2. How do the characteristics and impacts of WCBs change along the life cycle of the associated cyclone? 3. How do the cyclonic and anticyclonic WCB branches differ in terms of their characteristics and implications? To make the link between the research questions and the paper sections more explicit, we will note already in the introduction that research question 2 will be addressed through both case studies and a climatological investigation, providing a more comprehensive understanding of WCB behavior during cyclone development. Research question 1 will specifically be addressed in the methodology and case study section, and research question 3 in the case studies and in the climatological analysis.

l. 99–101 What is new compared to the WCB climatologies cited above?
We thank the reviewer for raising this point. The main difference is: (1) the calculation of the trajectories is based on a different data set (ERA5) with higher spatial and temporal resolution; (2) WCB trajectories can overcome the pressure difference of 600 hPa at any point in the period between the start of the trajectory and 48 h later, instead of requiring a strict pressure difference of 600 hPa between the start and end of the trajectory, 48 h later; and (3) in a bundle of WCB trajectories that fulfill the ascent criterion only at least one trajectory must coincide with a cyclone mask at least once. As discussed in the manuscript, these changes allow for a more realistic identification of WCBs.

l. 125 The resolution (6 hourly and 80 km) appears to be taken from ERA-Interim; this is fine but may deserve some comment.
The temporal and spatial resolution of the starting positions of trajectories in ERA5 is consistent with the approach of previous WCB-related studies (e.g. Madonna et al., 2014; Binder et al., 2016; Binder et al., 2023; Joos et al., 2023). Adding these references will help the reader to understand the rationale behind our approach and its consistency with prior research.

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"?
We thank the reviewer for this comment. The adapted WCB ascent criterion also includes WCB trajectories that ascend very fast and descend before the end of the trajectory at t=48 h and which were not included by previous studies of WCBs. This difference is schematically shown in Figure 1a: the green trajectory would not have been taken into

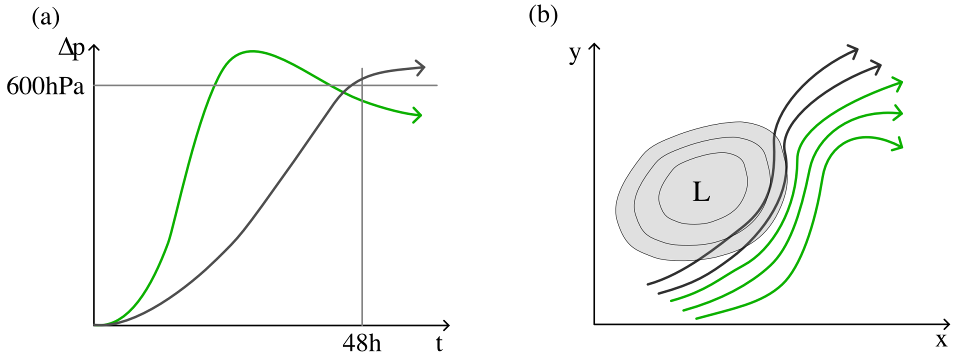

Figure 1: Schematic of WCB trajectories that are considered by Madonna et al. (2014b), shown in black, and those that are additionally considered by the adapted WCB criteria (green) in terms of their (a) ascent behavior and (b) overlap with a cyclone mask (grey area).

account according to the WCB criteria used by, for instance, Madonna et al. (2014), as the pressure difference between t=0 h and t=48 h is less than 600 hPa. The adapted WCB criteria are more flexible and consider both trajectories.

l. 135–136 The sentence is confusing
We agree that this step is complex and we will try to rephrase the sentence so that it becomes more clear to the reader. Figure 1b shows schematically where WCB trajectories ascend relative to a cyclone mask. By defining the WCB as a bundle of trajectories that start at a close distance from one another, we also consider WCB trajectories that ascend at a greater distance to the cyclone and never overlap with it (green trajectories) and that were not taken into account by previous WCB climatologies such as Madonna et al. (2014). Note that this step essentially depends on the way how we identify cyclone masks, i.e., as the area within the outermost closed isobar including the cyclone's center (SLP minimum). By allowing trajectories also to be part of a WCB that fall outside of this cyclone area, we avoid a rather 'artificial' cutoff of the WCB in the East.

l. 142–244 This is interesting but questionable, as several criteria are different, as well as the dataset
We thank the reviewer for this comment. We also found this difference between the WCB trajectories used by Madonna et al. (2014b) and the ones used in this study, based on ERA5 and with the adapted WCB definition, very interesting and investigated it in more detail. We found that the ERA5 WCB trajectories (fulfilling the adapted WCB criteria) ascend more rapidly and are therefore located at higher altitudes at earlier times than the WCB trajectories considered by Madonna et al. (2014), as shown in Figure 2. However,

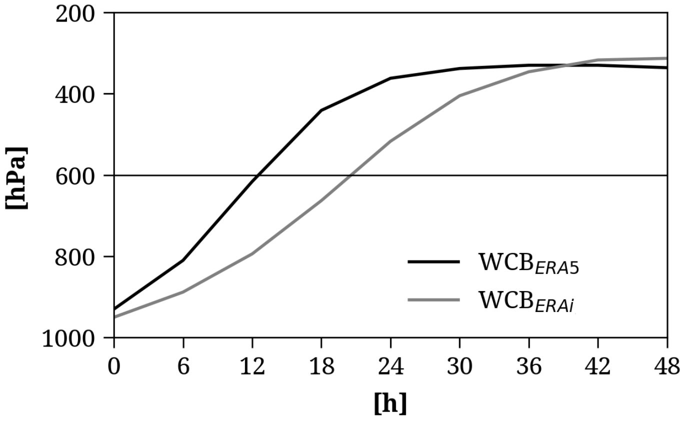

Figure 2: Comparison of the (a) pressure evolution (hPa, median) of WCB trajectories considered by Madonna et al (WCB$_{ERAI}$, grey, 1980–2014) and WCB trajectories considered in the present study (WCB$_{ERA5}$, black, 1980–2022) trajectories that start in the Northern Hemisphere in December–February. Accordingly, (b) shows the region of WCB inflow, ascent, and outflow of the respective set of WCB trajectories.

when we compare the frequency of occurrence of WCB inflow, ascent, and outflow masks (Figure 3, based on the location of WCBs in a pressure range instead of a fixed time after the start of the trajectory, both sets of WCB trajectories agree very well. Although interesting, we refrain from discussing all these aspects in the manuscript to not further extend the paper, but to also to keep a concise scope of this study.

l. 159 Any motivation for this value?
We inflate the WCB trajectory position to a circle with a specific radius so that we get a continuous mask that describes the region of WCB inflow, ascent, and outflow. Especially in the WCB outflow region, where the WCB fans out, the distance between single trajectories can be large, potentially resulting in (artificial) gaps in the WCB mask if the inflation radius is too small. We tested different inflation radii and our findings have led us to conclude that a radius of approximately 100 kilometers is the smallest size that ensures a continuous mask.Note that a inflation radius of 100 km is also in the order of the starting grid's mesh size (80 km). In principle, although not computationally feasible, we could make the inflation radius smaller if the starting grid is much more narrow-spaced. We will incorporate this information into the manuscript to provide a clear explanation of our choice of inflation radius.

l. 168–170 This sentence is disconnected from the rest
We agree with the reviewer and will include this sentence and the references in the

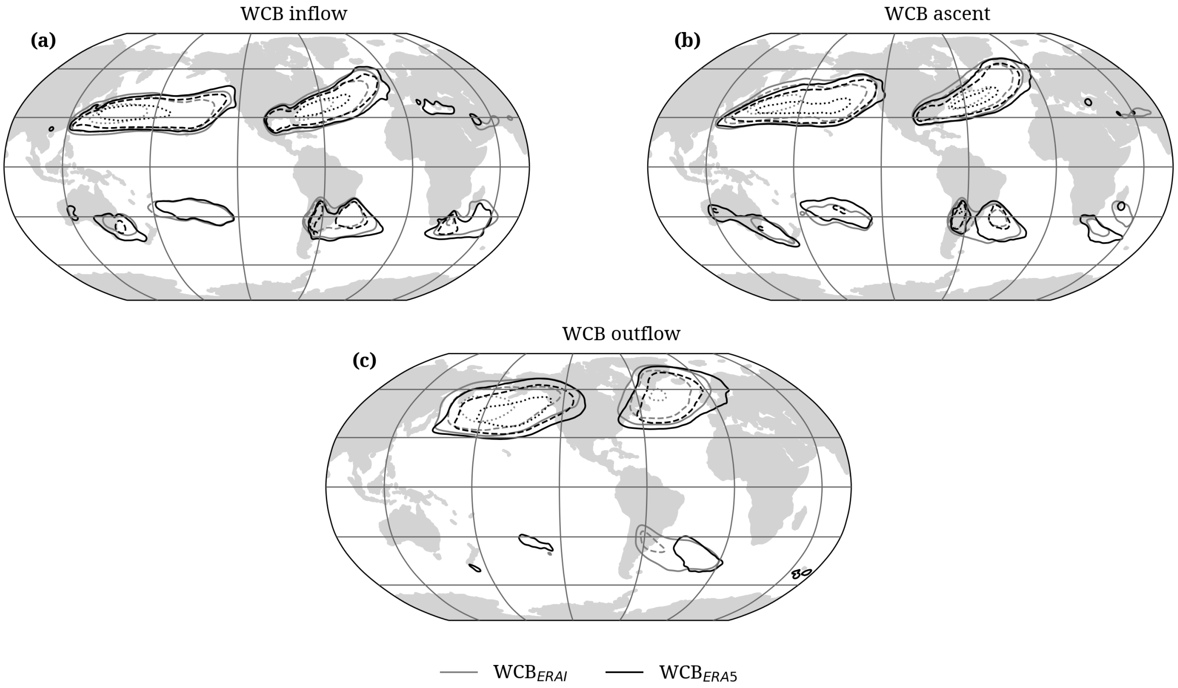

Figure 3: Region of frequent occurrence of WCB (a) inflow, (b) ascent and (c) outflow masks of WCB$_{ERAI}$ (grey) and WCB$_{ERA5}$ (black).

previous paragraph, as it highlights the continuity of our approach with previous WCB-related studies.

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"?
We thank the reviewer for this comment. Yes, per definition, the spatial occurrence of WCBs is strongly linked to the occurrence of an extratropical cyclone. Thus, it is not surprising, that the region of most frequent WCB inflow coincides with the storm tracks. We will mention this more explicitely in the revised manuscript.

l. 176–181 The frequency values require some kind of calibration, otherwise they are hardly usable as such.
We are not sure what kind of calibration the reviewer refers to. If the inflation radius is too small, gaps would result, e.g., in the ascent 0/1-mask for each individual WCB because of the discrete sampling distance of 80 km. With a radius of 100 km the gaps are (sufficiently) filled, and even if a somewhat larger inflation radius was chosen, the percentage values would essentially remain the same. Hence, we need the inflation to

counteract the limitations due to discrete sampling (at 80 km starting mesh size) and potential gaps. In this sense, we would argue that the frequency values can reasonably be interpreted without calibration. They describe how frequently (in percent) a specific latitude/longitude location is part of a WCB inflow, ascent and/or outflow. We will more clearly discuss this point in the revised manuscript. However, note also that the aim of this brief description is mainly to show that the adapted WCB definition applied to trajectories in ERA5 results in a climatology that is qualitatively consistent with previous studies.

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

We agree with the reviewer that the vertical position of WCB trajectories is more meaningful than where the trajectory is located at a fixed time after its start. Thus, the WCB masks, which describe the horizontal location of WCB trajectories in a certain vertical pressure range, are very meaningful and allow for an investigation of the different ascent phases of the WCB. The great spatial overlap between the WCB ascent and outflow masks and the WCB-related implications (precipitation rate and PV modifications in the lower and upper troposphere), as shown in the case studies, motivated us to use these masks to quantify the WCB characteristics and implications in a meaningful way.

l. 197 Lagrangian properties to contrast with the following Eulerian properties?

We thank the reviewer for this comment, indeed the metrics describing the WCB characteristics are Lagrangian properties, while the WCB implication metrics are Eulerian properties of the WCB. We will include this in the text.

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

We agree that a clear separation of the anticyclonic and non-curved branches is difficult and to some extent artificial. Indeed, the eastward direction of the large-scale flow often leads to both branches exhibiting some degree of anticyclonic curvature, albeit at different stages of their ascent. Therefore, non-curved trajectories often turn anticyclonically at the end of their ascent. Thus, we determine the curvature of a trajectory during its ascent (once it has reached 600 hPa and for the following 12 hours) and thereby disregard the curvature of the trajectory at upper levels. This leads to the anticyclonic curvature of the non-curved branch that was observed by the reviewer.

l. 223 Why the asymmetry?

We thank the reviewer for this comment. The threshold for the classification of the curvature has been determined based on a number of case studies. We found that the threshold for the cyclonic branch must be higher, in order to exclude trajectories that initially turn cyclonically but then follow the non-curved and anticyclonic branch to the east. We will include this clarification in the text.

l. 232 Altitude is not the best name for a pressure value
We will change the name of the "Altitude" metric to "Outflow pressure level".

l. 261 The Bergeron unit is not defined
We thank the reviewer for bringing this up. 1 Bergeron corresponds to a deepening rate of 24 hPa in one day at 60°N. We will add this piece of information to the text.

l. 288–289 Repetition of the reference
We will rephrase these two sentences so that the reference to Neiman and Shapiro (1993) is not repeated.

l. 303 The location of the developing cyclone is hardly seen on Fig. 3a
Assuming that the reviewer is referring to Fig. 4a, we agree that the developing cyclone is not visible due to the shown trajectories but also due to the weak signal in the SLP field at this moment in time. We will add a marker as in Fig. 3 that denotes the cyclone center, and this hopefully helps the reader to localize it.

l. 310 Same comment as above, and is the cold front shown somewhere?
We thank the reviewer for this comment. We did not specifically show any fronts, as this would make the plot even more complicated. However, one can infer the location of the cold front by the kink in the outermost isobar as well as by the location of the ascending WCB air masses.

l. 320–330 "almost perfectly", "considerably", "most likely not yet strongly": overstated
We see that this choice of words is somewhat subjective. However, we find that the WCB ascent mask captures practically the entire precipitation signal in the cyclone vicinity, and thus we find the wording "almost perfectly" justified. Furthermore, the more thorough analysis of the temporal TP90 evolution (Fig. 6e) shows that TP90 between 00 UTC on 4 January and 18 hours later increases from $0.4\,\mathrm{mm\,h^{-1}}$ to $4.8\,\mathrm{mm\,h^{-1}}$, which is an order of magnitude larger. Therefore, we find that the phrasing "considerably" is sufficiently motivated. We agree, however, that the phrase "most likely not yet strongly" is unnecessarily complicated, and we will rephrase it accordingly.

l. 337 This is hardly seen on Fig. 5f
We agree that low-level PV is only weakly enhanced, but we still find it important to note that the location of enhanced PV values coincides with the region of precipitation formation as well as the WCB ascent mask. Furthermore, the PV values are averaged between 750 and 950 hPa. As PV is not necessarily enhanced in all considered pressure

levels, the mean PV is stronlgy reduced by the vertical average. However it sill highlights the importance of the WCB and diabatic processes for PV production.

l. 343 Remind the definition of ULPVA?
We thank the reviewer for this comment, and we agree that the definition of ULPVA should be repeated at this point. We will include it in the revised text.

l. 350 What is "because of the low altitude and latitude of the WCB outflow in this region"?
We thank the reviewer for this comment. PV increases in generally at the tropopause and towards the poles. Thus, a WCB outflow located at low altitudes and low latitudes is located in a region of climatologically low PV values. The PV anomaly is defined as the difference between the instantaneous PV value and the climatological mean. Thus, WCBs outflow masks located in a region of climatologically low PV can be associated with only a weak or even positive PV anomaly. We will add a brief note about the climatologically low PV at low altitudes and latitudes to clarify the text and to explain the occurrence of low or positive PV anomalies.

l. 355 In what sense is it similar?
We thank the reviewer for this comment. We noted that both WCB characteristics and implications evolve during the cyclone life cycle. This observation acts as the motivation to study the evolution of these metrics with time in the following. We agree with the reviewer that the word 'similar' is not specific. We will rephrase it as: To conclude, WCB characteristics and implications co-evolve during the cyclone life cycle.

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
We agree that we cannot infer directly a link between the WCB and cyclone intensification from this analysis. We will make it explicit that this paragraph primarily serves to describe the temporal evolution of LLPV in the region of WCB ascent and is not intended to make a general statement about the direct link between WCBs and cyclone intensification. Indeed, to establish/quantify the WCB impact on cyclone intensification would require a more systematic analysis, which however is beyond the scope of our study.

l. 416–418 It is surprising to realize that the chosen case was illustrated above but not mentioned
Unfortunately, we are not exactly sure what this comment means. The second case study was illustrated in Figure 3, which shows the different WCB branches. The caption of Figure 3 mentioned that this is the case study investigated by Martínez-Alvarado et al. (2014). We would gladly add any additional information to clarify the case study selection to the reader.

l. 420 Of which trajectories?

We thank the reviewer for this comment. The phrasing was unclear, we will add the information that the pathway of the WCB trajectories in ERA5 at this moment in time are very similar to the trajectories shown by Martínez-Alvarado et al. (2014). This is not immediately expected, as they were calculated in different models.

l. 425–426 Cyclonic or anticyclonic branch in Martínez-Alvarado et al. (2014)?

We thank the reviewer for this comment. Both the present study and Martínez-Alvarado et al. (2014) found the strongest increase in PV along the cyclonic, W2 branch, which starts at higher latitudes and ascends closer to the cyclone center. We will add the specific branch classification to the text.

l. 487–489 This sounds speculative

We thank the reviewer for this observation. PVOL is determined, on the one hand, by the area of the WCB mask, which in turn strongly correlates with the WCB intensity. Hence, we would expect, in first order, PVOL to coincide with WCB intensity. This, however, is not the case, and can be (somewhat speculatievly) attributed to the timeshift in PQ90. In fact, PVOL also depends strongly on PQ90. Thus, a reduction in PQ90 can explain why the peak in WCB intensity and PVOL are not temporally aligned. We will weaken the statement in the revised text, to make the reader aware that we have not explicitly (and quantitatively) tested this hypothesis.

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

We agree with the reviewer that the discussion regarding the influence of the Coriolis factor on LLPV and the consistency with results from Madonna et al. (2014) should be shifted to the discussion of the results.

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

We thank the reviewer for this comment, we agree that this case study acts as a very good illustration of the archetypal WCB. Hence, as mentioned before, we will move case study 1 and 3 to the supplementary material and only show case study 2, as an archetypical WCB example, 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

We agree with the reviewer that the complex synoptic situation and the mentioned features are difficult to identify in the figure. We will add further information regarding the exact location of each feature to clarify it to the reader.

l. 583–584 Unclear
We apologize to the reviewer for a small, but important typo in this sentence that likely led to the confusion. The correct phrasing would be "The temporal delay is linked *to* the manual attribution of the trajectories to the later emerging cyclone".

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"?
We agree that the sentence should be clarified and rephrased as a hypothesis. We will make the necessary adjustment to present it as a hypothesis. The basic idea behind our hypothesis is: As convection is generally more likely to occur at low latitudes, we suggest that rapid, potentially convective WCB ascent is less frequent at high latitudes, thereby decreasing the overall ascent rate. We will ensure that the text explicitly states that this explanation is one of several possibilities, acknowledging the complexity of the synoptic flow situation and the many (interacting) factors at play.

l. 618 A comparison of the three case studies is expected here
As mentioned before, we will rearange the presentation of the case studies: Case 2 will be discussed in detail in the main text. The discussion of case 1 and case 3 will be kept brief in the main text, focusing on highlighting the differences from case 2 and emphasizing the significant variability of WCBs from case to case. The detailed discussion of case 1 and case 3 will be moved to the supplementary material.

l. 636 Why is it "intriguing"?
We thank the reviewer for this comment. We were positively surprised by the very good agreement of the share of cyclones associated with a WCB in the present data set and results from the study by Eckhardt et al. (2004), which used a different data set, a different WCB definition and a different cyclone identification algorithm.

l. 671 The contrast looks quite weak
We thank the reviewer for this comment. We agree that the difference between the temporal evolution of the WCB intensity and WCB ascent rate is not distinctively different. We will rephrase this comparison.

l. 675–676 Not sure what to learn from this and cyclone intensification lasts for longer than 6h
We thank the reviewer for this comment. The mentioned period of 6 h only describes the maximum intensification phase, we will clarify this in the text. We found it important to highlight that about a third of all WCBs is characterized by a cyclonic branch at this moment in the cyclone life cycle, as this was so far unknown.

l. 683 “very likely”: is it or not related to intense convective precipitation?

We thank the reviewer for this comment. Our analysis showed that the convective precipitation peaks at a very early stage in the cyclone life cycle. We did not specifically investigate if the large-scale precipitation is also enhanced during this time, but we will investigate this further. If indeed mainly the convective precipitation is enhanced at this stage, we will more clearly add this information to the text.

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

We thank the reviewer for this comment. In a separate analysis, we analysed the ULPVA at t=24 h associated with the selected set of WCBs and found that more intense WCBs (more trajectories) are indeed associated with a more intense ULPVA. However, the WCB outflow intensity does not seem to affect the ULPVA during the cyclone life time strongly, as the present study shows. This indicates that other factors than the WCB intensity, like for example the location of the WCB outflow, affect the temporal evolution of ULPVA. It is, however, beyond the scope of this study to investigate and identify these factors.

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

We thank the reviewer for this comment. The mentioned paragraph summarizes the temporal evolution of WCB characteristics and implications associated with bomb cyclones as well as WCBs ascending in different ocean basins. We agree that the second part could be moved to the supplementary material as the present study focusses on the North Atlantic. However, we find it important to note that the cyclone intensity affects the amplitude of the WCB characteristics and implications, as the presented case studies are all associated with extremely strong (bomb) cyclones.

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

We thank the reviewer for this comment. We will rephrase the sentence accordingly.

l. 741 “lowest” is misleading for the highest pressure value

We appreciate the reviewer’s suggestion to clarify the sentence. We will make the necessary adjustment by rephrasing it to: “The cyclonic WCB branch ascends to the lowest altitudes (highest pressures) [...].”

l. 748–750 This very short summary does not support the need for detailed case studies

We thank the reviewer for this comment. As mentioned before, we agree that the discussion of three detailed case studies is, in fact, not needed, and we will reduce it to one. However, we find that one detailed WCB case study shows that the anticyclonic branch arrives at the developing ridge where it can ascend to higher altitudes (lower pressure levels) and is therefore associated with a more intense ULPVA.

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
We thank the reviewer for this comment. We will discuss the differences between the WCB case studies after presenting (only) case 2 and therefore skip the comparison here and mainly present the general conclusions instead.

l. 762 larger but opposite
We agree with the reviewers assessment regarding the importance of whether the cyclone appeared before or after the WCB first appeared and will add this to the text.

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
We thank the reviewer for this comment. Indeed, we focus in this discussion on the importance of the large-scale forcing for the WCB intensity, both peaking during the cyclone intensification phase. We will include a brief discussion adding the link between the WCB intensity and cyclone intensity due to diabatic PV production. As the reviewer already notes in his comment, WCB and their diabatic PV production can contribute to the cyclone intensification, but we would not expect the whole cyclone intensification to be controlled by the WCB. Certainly, upper-level, large-scale forcing also plays a (or even the) crucial role. We will clarify this in the revised text.

l. 814–818 This is also interesting but is not mentioned before, thus does not summarize results
We agree that this hypothesis for the overall decrease in WCB ascent rate during the cyclone life cycle was not mentioned in the climatology, but instead briefly in the discussion of case study 3. Thus, we will add this point to the discussion of the WCB ascent rate in Section 4, where we present the WCB characteristics climatology.

l. 833–834 Any explanation for this?
The total volume of precipitation is largest 10 h after the peak in the precipitation rate. One (partial) explanation for this delay could be the slow decrease in WCB intensity after its peak during the cyclone intensification. Another explanation could be that the WCB becomes less coherent after t=0 h and that the area of the WCB ascent mask becomes larger, thereby increasing PVOL further. We will add these hypotheses to the summary.

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
We thank the reviewer for this comment. We are not exactly sure what is meant specifically with “WCB evolution”, however we agree that this summary mainly focusses on the importance of the Coriolis parameter for LLPV (consistent with Madonna et al., 2014) without mentioning that PV in the cyclone center also increases during the cyclone life cycle and therefore further enhances LLPV with time. Hence, we will state more clearly in the revised text that the latitudinal effect on LLPV is significant, but that there are still other factors influencing LLPV.

l. 848–850 This sounds speculative
We agree with the reviewer that this sentence was phrased vaguely, as we did not specifically quantify the “impact of the WCB on the downstream large-scale flow evolution” and therefore can only suspect when it is largest based on the available metrics. The reviewer is perfectly right that we are mixing concrete results from our study, with more speculative statements how these concrete implications might influence the flow downstream. We will more clearly separate these aspects in the revised text, and we will also mention and briefly discuss the limitation of our approach.

l. 867–869 Why not try them?
This study presents for the first time a systematic quantification of the WCB characteristics and implications, and therefore we acknowledge that there could be different opinions about the best definition of each metric. We tested differed approaches and are confident that the qualitative findings are not dependent on the definition of the metrics. However, it is beyond the scope to conduct a sensitivity analysis of the metric definition. We hope that our first systematic study and definition on WCB metrics will inspire further follow-up studies, potentially also based on more refined metrics.

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)
We agree with the reviewer that the developed methodology was not included in the research questions and therefore understand this comment. Hence, we will rephrase the research questions accordingly (as described before). Actually, we believe that the developed methodology is also a valuable result in its own right, as it allows to systematically and objectively characterize WCBs. This adjustment will hopefully help readers appreciate the significance of the developed method within the context of our study.

l. 874, 878 novel vs new climatology
We thank the reviewer for this comment. We refer to the WCB climatology as “new”, as it builds upon an existing approach that was applied in multiple previous studies and only the used data set and WCB definition have changed. The climatology of the WCB metrics is “novel” because it introduces a new approach to characterize WCBs. It has

not been applied before. We hope this clarifies the choice of words.

l. 882–883 positive PV and negative PV anomalies
We thank the reviewer for this comment and will add the sign of respective PV values in the text.

Figs. 6, 9, 12 Changing scales between figures does not help comparison
We agree with the reviewer that different scales for the same type of figure is not optimal. However, due to large differences in the amplitude of the WCB characteristics and implications between the cases, different scales hopefully help the reader to identify the most important aspects of each case. We will add a note that the scales between the plots change in order to help the reader in comparing the cases.

Fig. 15 When two curves show the same variable, a common scale would be more appropriate
Similar to the previous comment, we agree with the reviewer that different scales for the same variable are not optimal. However, we found that a common scale in Fig. 15a, c and f lead to a reduced readability of the respective subpanel, as the temporal variability of one of the curves becomes very small.

Reviewer 2

SUMMARY

This paper presents an analysis of warm conveyor belts (WCBs) in ERA5. Lagrangian trajectories are used to identify the WCB and a spatial mask is applied to associate the WCB to its impacts. Results are demonstrated in two parts. First, three separate case studies are analyzed, compared, and contrasted. Second, a climatology spanning the 44-year ERA5 data set is presented. The paper is dense and contains many interesting results, but one of the more robust results is that WCBs are typically most intense when the cyclone itself is deepening most rapidly.

Overall, this paper is well written, the figures are well presented and clear, the methodology is appropriate and clearly described, and the conclusions are supported by the evidence presented. This work represents the latest installment in a line of meticulously-conducted studies of WCBs leveraging LAGRANTO. I am eager to learn about follow-up work utilizing the same methodology but applied to climate model simulations.

I am pleased to recommend publication after a minor revision. While I have no major concerns that would merit extensive revision of this paper, I would like to make several points for the authors to consider in future applications of this methodology. These points are expressed below. The paper is long and ambitious in scope. I make this as an observation and not an implicit recommendation to break it up into several shorter papers. That said, this work could probably have been distributed over two papers, although I see no problem fundamentally with long papers.

Dear Prof. Chagnon,
Thank you very much for your positive and constructive feedback regarding the text, figures, methodology, and conclusion of our manuscript. We are delighted to learn of your interest in exploring further applications of the developed method. By addressing the points you have raised, we aim to make the limitations of our study clear to the reader and ensure the overall quality of our manuscript. Your feedback is greatly appreciated, and we are eager to incorporate your suggestions and improve our work accordingly.
Kind regards
Katharina Heitmann on behalf of all authors.

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.

Thank you for raising this point. We agree that the approach to quantify the ULPVA does not isolate the diabatic contribution from other factors and that a WCB outflow in the region of an amplied ridge is very likely associated with a negative ULPVA. However, we still conclude that the chosen approach can give valuable insights in the implications of the WCB at upper levels. For instance, we found that the ULPVA between the cyclonic and anticyclonic branch differs distinctively. Furthermore, a more in-depth analysis of the ULPVA, not included in this manuscript, also showed the the ULPVA correlates with the intensity of the WCB. In order to separate the diabatic contribution to the ULPVA, a separate sensitivity analyis would have been needed, which is unfortunately beyond the scope of this study. We will add these limitations to the description of the definition of the ULPVA and thereby hopefully help the reader to better understand the applicability but also the limitations of this metric.

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?

Thank you for this comment. Indeed, the WCB outflow mask can be located in a region of a PV gradient at upper levels, thereby leading to a more positive ULPVA. By taking the median and not the mean of the PV anomaly in the WCB outflow mask, strongly positive PV values on the poleward side have no impact on the resulting metric. However, we believe that this bias is justifiable, as all WCBs are equally affected by it, and the aim of this study is mainly a qualitative investigation of the temporal evolution of metrics such as ULPVA and the differences between the WCB branches. Furthermore, we tested different radii, but concluded that 100 km is approximately the smallest radius that results in a continuous WCB outflow mask.

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.

Thank you for this comment and we agree that this dataset allows for the first time for an investigation of the variability of WCBs, not *just* their evolution during the cyclone life cycle. Many of the points that you mention were addressed in the PhD thesis that was associated with this study and that will hopefully be published soon. For instance, we investigated the link between the NAO and WCB. While the number of WCB trajectories does not depend on the state of the NAO, we found a distinct shift in the inflow, ascent and outflow regions of WCBs in NAO positive and negative winters. Furthermore, we investigated the link between WCB characteristics and impacts and found, for instance, that the intensity of the ULPVA correlates with the intensity of the WCB and that WCB associated with heavy precipitation are more likely to ascend rapidly. Last but not least, we also investigated how WCB characteristics and impacts are expected to change in the future (using the climate model CESM1) and found a general intensification of WCB intensity, amount of moisture transported by it, precipitation rate and LLPV. We hope that this PhD thesis will serve as a reference for future investigations regarding the characteristics and impacts of WCBs.