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

Anonymous Referee #1

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Review of wcd-2020-25

"Organization of convective ascents in a warm conveyor belt"

by Nicolas Blanchard et al.

Paper in review in Weather and Climate Dynamics Discussion

1 General Comments

The paper presents a detailed analysis of a case study of ascending motion within the warm conveyor belt region associated with a strong extratropical cyclone in the North Atlantic. The case study occurred during the NAWDEX field campaign which allows for the analysis of rare airborne radar observations with the RASTA system, which is accompanied by online Lagrangian trajectory analysis and a 3D clustering of updraft objects in a convection-permitting simulation. The study, with a strong focus on two individual points in time, confirms recent results from previous case studies that fast ascents can be an integral part of the mostly considered slow and slantwise WCB ascent region. The analysis of online trajectories centered around the RASTA observations and the 3D clustering provide evidence for the occurrence of shallow faster ascents in the cyclone. In particular, the combination of observations, online trajectories and the 3D clustering provide a comprehensive view on fast ascents based on different diagnostics. The occurrence of fast ascents is further divided into different categories of convection, e.g., frontal, banded and mid-level convection. The analysis of fast ascents within the cyclone is complemented by a description of PV evolution along fast and slow ascents and examples of PV distribution associated with rapid ascent.

I recommend publication of this manuscript, but I have several major concerns that should be addressed beforehand as well as specific comments and questions listed below:

1 **Identification of WCB trajectories**
While I agree that the considered case study is indeed a WCB (e.g., Maddison et al., 2019), I do not agree with the here applied identification of individual WCB trajectories with an ascent criterion of 150 hPa in 12 h (l. 106). This performed downscaling of the ascent criterion from the mostly used 500-600 hPa ascent

in 48 h (l. 107) captures the mean ascent rate of what is typically considered a WCB, however, it does not ensure that the trajectories actually perform a full ascent from the lower to the upper troposphere. The latter is a defining characteristic of the WCB, as the airstream connects the lower with the upper troposphere (which is correctly mentioned in the introduction). In contrast, the 150 hPa ascent in 12 h, can also include air masses that only rise a little bit but do not perform a substantial cross-isentropic ascent. Indeed, previous studies showed that the WCB airstream is often accompanied by air masses that are only lifted a little (e.g., Wernli et al., 2016; Binder et al., 2020), however, do not themselves define the WCB airstream. I would ask the authors to discuss this topic in section 2.3, and rephrase the sections where the selected trajectories are referred to as 'WCB trajectories' (e.g., l. 136). Please be specific about what is indeed considered as WCB trajectory (i.e., deep cross-isentropic ascent), such as the trajectories performing the actual 600 hPa ascent (e.g., l. 173) and what is slow/fast ascent within the overall extended "WCB ascent region" or within the extratropical cyclone (but not necessarily considered a WCB trajectory). In particular, trajectories with fast ascents that remain only in the lower and mid-troposphere should not be considered a WCB trajectory. This is a major concern and should be resolved before publication of the manuscript.

To illustrate my point, Fig. 11, for example, shows the ascent of some selected trajectories. The ascent of categories "frontal convection" and "banded convection" appears to flatten out at 3 km height, and hence, would not be considered as WCB trajectory. Instead, it resembles shallow convection in the extratropical cyclone.

Finally, the authors themselves mention in the conclusions (l. 441) that WCB trajectories are identified outside of the WCB ascent region, which is contradictory and suggests that the selected trajectories may not all be WCB trajectories: "Contrary to what one might expect, WCB trajectories are identified not only in

the WCB ascent region but also in the cloud head and along the warm front of the cyclone". I hence suggest that the authors do not name their selected trajectories per se as 'WCB trajectories' and rephrase the according passages in the manuscript.

2 Trajectory computation
This point is related to the above comment. Have the authors tried to use a longer time window than 12 h for the trajectory computation? A longer time window (even if some trajectories leave the domain boundary) might allow for a larger number of trajectories that actually perform a WCB-like deep ascent from the lower to the upper troposphere. See also comment to l. 103. Did the authors also consider trajectory computation centered around other times? Do the authors find similar structures and distinction of slow versus fast ascents as for trajectories centered at 16 UTC? How do these structures evolve with time?

3 RASTA observations
The study uses rare campaign observations to analyse the radar reflectivity structure of fast ascents in the WCB ascent region. Instead of providing a lengthy comparison of the capability of MESO-NH to simulate the overall radar reflectivity structure (sections 4.1 and 4.3), I would recommend focusing on the region of fast ascents and provide a more detailed analysis and description of the fast ascent regions based on the observational evidence. The availability of such measurements is a great opportunity to obtain more observational evidence of these embedded fast ascents and deserves more detailed consideration.

4 Separation of anticyclonic and cyclonic branches
The performed analysis focuses on the distinction between anticyclonic and cyclonic branches in several sections (e.g., 3.3, 3.4, 3.5). What is the exact reasoning behind the separation into these categories? In Figs. 4, 6 and 11 it appears that the separation is to a large extent determined by the altitude of the trajecto-

ries. As expected, trajectories at a higher altitude experience the strong winds from the upper-level jet, and are hence advected anticyclonically. Besides, WCB branches have so far been mostly considered in the upper tropospheric outflow (e.g., Martínez-Alvarado et al., 2014). I am, thus, not sure if the cyclonic trajectories (which mostly remain below 3-4 km height, e.g., Fig. 4) would be considered a cyclonic WCB branch. Did the authors check, if these trajectories continue their ascent after dt=6 h? Moreover, the separation of "mid-level convection" in cyclonic and anticyclonic subsets (Fig. 12) is not convincing in its current state and additional clarification is needed. Did the authors check if the "cyclonic" trajectories do not turn anticyclonically within the next couple of hours? In the beginning, both clusters overlap, and only at around 20-22 UTC the orange trajectories perform a slight anticyclonic turn. It appears as if the green trajectories could theoretically follow the path of the orange "anticyclonic" trajectories if extended by a few hours. Did the authors consider this possibility?

Similarly, the authors define many categories and sub-categories of fast ascents based on the 3D clustering approach. I appreciate that this analysis shows the coherent nature of the individual (shallow) convective regions. Do these categories differ substantially in terms of characteristics and impact? How do these different categories evolve?

5 Lagrangian versus Eulerian perspective
In some parts of the study, it is not clearly stated if the Eulerian or the Lagrangian perspective is considered. For example, it is unclear to me if the "WCB frequency" (Figs. 2 and 5) is computed as frequency of trajectories all centered around 16 UTC and following a certain path or if it represents the frequency of the location of trajectory air parcel positions at 16 UTC. The differentiation between this is quite important. If it is the latter, please specify more clearly. "The frequency of trajectories" (l. 137) sounds like it is the first. Please note that a direct comparison of the "trajectory frequency" with Eulerian fields is not valid, as the trajectory

frequency spans a 12 h window, while the Eulerian field is only valid at one point in time. See also specific comment to l. 138. In addition, in Figs. 2, 4, and 5 it would be insightful to show the location of the rapid segments which occur at 16 UTC. This would enable a direct comparison of where relative to the fronts the rapid ascent takes place. This type of analysis would complement the 3D object clustering analysis.

6 Relation of fast ascents and PV
The PV evolution along trajectories and the discussion section (section 5) about the relation of PV and rapid ascents is in its current form not convincing. I would suggest to either remove these sections or substantially shorten them. In general, I would recommend streamlining the manuscript and focusing on the organization and structure of convective ascents as suggested in the title. The major concerns about the analyses including PV include (i) the robustness and significance of the results with mostly small differences in mean and large interquartile ranges and (ii) the purely descriptive character of the PV signals, i.e., the lack of explanations for the described PV evolution and PV features. See also the specific comments below.

2 Specific comments

ABSTRACT

1. l. 8-9: "The simulation reproduces well the mesoscale structure of the cyclone shown by satellite infrared observations". This information might not be relevant in the abstract.

2. l. 9-10: " the location of trajectories rising by 150 hPa during a relatively short 12 h window matches the WCB region expected from high clouds". This sentence is unclear to me. How do the authors link ascent of 150 hPa with the WCB? It sounds as if the authors identify the WCB from "high clouds", however, the WCB is more than just a "high cloud" layer.

3. l. 12: This sentence is a bit confusing. Are the "convective updrafts" identified directly from the radar or identified in the simulation? Please clarify this sentence.

4. l. 16ff: The presented results about the PV objects and the lower- and upper-level jet are not convincing and very speculative. Please remove this part from the abstract. See also major comment 6 and specific comments below.

5. l. 19: The last sentence is repetitive.

1 INTRODUCTION

6. l. 24: The authors could replace "lower layers of the troposphere" by "lower troposphere" to streamline the text

7. l. 30: See comment to l. 24. The authors could replace "lower layers of the troposphere" by "lower troposphere".

8. l. 43: What do the authors mean with "isolated clouds"? If convection is embedded in a larger cloud system such as the WCB as was described, the convective clouds do not appear to be isolated?

9. l. 52: Why also winter, if the field campaign took place in Sep/Oct?

10. l. 54: Maybe replace "well sampled" by "well observed"?

11. l. 56: "More specifically, the onset of a blocking situation over Scandinavia was found unpredictable in the medium-range forecasts." This information is not relevant at this point. Please explain the relevance for this study in more detail or omit.

2 DATA and METHODS

12. l. 77: It would help the reader if the authors added the reference directly "(flight 7 of the Falcon 20 aircraft, Schäfler et al. 2018)".

13. l. 93-96: These sentences are a bit confusing as it is initially unclear, which data comes from the model and which are MSG observations. Please improve this paragraph.

14. l. 100: For simplification, the authors could replace "the temperature of clouds at their top" by "cloud top temperature".

15. l. 103: Why did the authors chose a 12-h window? To actually capture WCB trajectories, wouldn't it be more meaningful to chose a longer time window that would actually capture the WCB ascent from the lower to the upper troposphere with ascent depths that are representative of WCBs (e.g. 500-600 hPa)? See also major comment 1. Did the authors check which percentage of trajectories leaves the simulation domain if the trajectories are actually computed for a longer period? The later mentioned "banded" and "frontal" convection do not appear to ascend above 3 km. If these trajectories were run forward for several more hours, would they continue their ascent? See also major comment 2.

16. l. 105 ff: I do not agree with the adapted criterion of 150 hPa ascent in 12 h to identify WCB trajectories. See also major comment 1.

17. l. 110 ff: Do the 3D objects need to have a certain size to be identified as a cluster? Could the authors elaborate a bit more on the clustering approach?

18. l. 112: The threshold of 0.3 m s-1 is based on the identification of the so-called "fast ascents". Until here, the fast ascents have not been defined. Please add a short explanatory sentence for clarification at this point.

19. l.112: The applied threshold of 0.3 m s^{-1} appears low at first sight. The authors could add an estimation of 'typical' ascent velocities of a WCB (approx. 10 km in 48 h, i.e., $\approx$0.05 m s^{-1}), which would emphasize the selection of ascent rates that are an order of magnitude larger than what would be expected from the widely-used WCB criterion. The 600 hPa ascent in 12 h discussed in section 3.3 would correspond approximately to such an ascent rate.

20. l. 114: Similar to comment above: Do the PV objects need to have a certain size to be considered a cluster?

3 General characteristics of the WCB

21. l. 117: "is expected to be" sounds vague. Please clarify.

22. l. 118-119: The specification of the colors in brackets is not needed here, because the colorbar in the figure is self explanatory.

23. l. 120ff: Again "is expected to be" sounds vague. Is the WCB outflow there or not? I find it difficult to distinguish the two branches based on BT alone. How do the authors distinguish that WCB trajectories are ascending into the cloud head? Please make sure to be concise with what is referred to as WCB and how it is identified.

24. l. 112: The authors could add Martínez-Alvarado et al. (2014) as a reference for anticyclonic and cyclonic branches.

25. l. 125: Could the authors describe where the discrepancies in the BT values between MSG and the satellites are found?

26. l. 132: "In the simulation, the track shows much more detail with hourly resolution." This is expected in a simulation with higher temporal and spatial resolution. Please streamline this paragraph.

27. l. 137: "The frequency of trajectories fulfilling the WCB criterion of 150 hPa in 12Âÿ,h". As mentioned before, I don't think the applied criterion is appropriate to identify WCB trajectories.

28. l. 138: "It is integrated on all vertical levels and calculated on coarse meshes of 20 km x 20 km for better visibility." (i) Please specify how it is "calculated" (e.g. interpolated). (ii) Did the authors simply compute the frequency of the Lagrangian trajectories? Or does it show the Eulerian perspective of air parcel trajectory ascent? If it is the first, the "frequency" does not show the actual frequency at 16 UTC, but integrated over the full 12 h window. I.e., it is difficult to combine the Eulerian θ_e field with the trajectory maxima, because the trajectories at t=-6 h can be located somewhere else relative to the cyclone; similar for the position of trajectories at t=6 h. It could also be meaningful to show the trajectory position at t=0 h (i.e., at 16 UTC). See also major comment 5.

29. l. 138: Please remove "equivalent potential temperature", as it has been introduced before.

30. l. 143: "Few or no WCB trajectories are detected in the dry intrusion". This is expected, because the dry intrusion is a descending airstream, i.e., dry intrusion and WCB cannot co-occur. Please clarify this part.

31. l. 160: For clarification, please include "maximum" pressure variation.

32. l. 173: Although ascent rates of at least 600 hPa in 48 h is often used, previous studies have already shown examples of WCB trajectories that are characterized by faster averaged ascents similar to what is shown in the manuscript (e.g., Fig. 7 in Martínez-Alvarado et al., 2014).

33. l. 174: I agree that convective motion can occur for a shorter period of time. However, in particular deep convective motion is often characterized by deep ascents from the lower to the upper troposphere. Do the authors here refer to shallow convection? Can the authors please elaborate and set it into perspective?

34. l. 176: Heading "3.4 Location of slow and fast ascents in the WCB". I suggest to rename the heading to something like "trajectory/path of slow and fast ascents in the WCB", because the evolution of the entire trajectory is shown.

35. l. 178: Are the selected samples chosen randomly? Are they representative for the entire ensemble of trajectories?

36. l. 178: How do the authors define the "core of the WCB"? While reading the manuscript, I realized that it is explained below (l. 202). Please define it when it is first mentioned.

37. l. 182ff: See general comment 4 for the distinction between anticyclonic and cyclonic trajectories and its dependence on the height level.

38. l. 187ff: It appears as if the majority of fast ascents starts in the lower troposphere and only reaches 3-5 km height. Did the authors check if these trajectories remain at this elevation or if they continue their ascent? Is this some kind of boundary layer triggered convection? Where relative to the fronts do the rapid segments occur?

39. l. 206: "Fast ascent are mainly located behind the surface cold front and more particularly in its southern part". What do the authors mean with "behind" the cold front? East or west of the front? It is difficult to exactly see the location of the cold and warm fronts in Fig. 5. This could be enhanced by using appropriate colors for the θ_e-contours or drawing the frontal surfaces. Does Fig. 5 show the frequency of the selected trajectories all centered around 16 UTC or the frequency of air

parcel trajectory positions at 16 UTC? See also comment to Fig. 2 and general comment 5. If it shows the frequency of selected trajectories all centered around 16 UTC, it is problematic to directly relate it to the position of the fronts, which is a Eulerian field and only valid at 16 UTC. In contrast, the frequency of trajectories would be valid for the full 12 h period.

40. l. 217: The authors state that the interquartile ranges show a lot of overlap between the fast and slow trajectories and the mean does not differ substantially either. Fig. 6 suggests that there is almost no difference between slow and fast ascents. Does the averaging along trajectories smear out the signal or is there indeed very little difference between the slow and fast ascents? Instead of simply averaging over all the fast and slow ascents, did the authors consider to analyse the rapid segments in more detail? See also comment to l. 353-354.

41. l. 227: The anticyclonic trajectories overlap in altitude (Fig. 6a). But do the corresponding air parcels also overlap in space and time? Fig. 5a,b suggests that there is only partial overlap between fast and slow anticyclonic ascents with the slow ascents mostly north of 60°N and the fast ascents south of 60°N.

42. l.232: I would have expected the fast ascents to have a larger vertical velocity as the slow ascents. Why isn't this the case? Could the authors go into more detail to further clarify their observations?

43. l. 234: Why did the authors chose to show only graupel mixing ratio, if there is very little graupel actually produced? How about snow and rain? Why is the graupel mixing ratio larger in the slow (cyclonic) ascents than in the fast (cyclonic) ascents? Isn't this counter intuitive?

44. l. 239-249: The authors discuss the mean evolution of PV values along the different trajectory clusters. What are new insights gained from this analysis? Does the PV structure of the slow and fast ascents differ? It seems as if the PV

values are more strongly influenced by the trajectories' height (which is already known, e.g., Wernli et al., 1997; Madonna et al., 2014) than by the distinction between slow versus fast. Please streamline and emphasize what is new.

4 Fast ascents in the region of observations

45. l. 258: What do the authors mean with "absence of reflectivity values"?

46. l. 271: I think the authors mean that the black dots show the air parcel positions based on the trajectories, and not the trajectory positions themselves (which would not be a dot only). Are these air parcel positions obtained from the trajectories centered around 16 UTC or did the authors analyse trajectories centered around 15 UTC, too?

47. l. 272-273: In my understanding the dry intrusion is a descending airstream. How can the selected ascending trajectories be located "within the dry intrusion"? Do the authors mean below the dry intrusion? The dry intrusion has been mentioned several times before, how do the authors identify it? See also comment to l. 143.

48. l. 287: What do the authors mean with "topping in the dry intrusion"?

49. l. 296-297: Can the authors please elaborate on this? Why would a pressure criterion focus only on lower levels?

50. l. 297-300: The description about the tropopause and jet structure is a general description of the basic synoptic situation and does not fit in this section about "fast ascents".

51. l. 300ff: Where exactly are the PV dipoles located? The rapid segments are located near a positive PV anomaly (above 2 PVU), but not all rapid segments also coincide with negative PV features (Fig. 8b). In particular, I cannot clearly see dipoles of PV.

52. l. 333ff: I am not sure, if I can correctly identify the second cell. Do the authors refer to the rapid segments located at 60°N, too? If yes, could this also be considered as one object or are these clearly separated structures?

53. l. 335: I cannot clearly identify the mentioned PV dipole around both convective cells in Fig. 10b. Small-scale negative PV features are present, but where is the positive pole? It seems as if the PV signal is not very pronounced. See also comment to l. 300ff.

54. l. 337ff: I think that these upper-level convective structures are very interesting, especially because they occur in both the observations and the simulation. Do the authors have any idea why the trajectory analysis does not identify them? Is the rapid ascent in this region too localized or too transient to enable the maintenance of a deep ascent of at least 100 hPa? Do trajectories in this region not meet the ascent criterion of 150 hPa?

55. l. 351-353: It appears evident that mid-level convection is located in the middle troposphere. Please streamline.

56. l. 353-354: I find the results in Fig. 11a much more convincing than results in Fig. 6. Did the authors consider streamlining the manuscript and avoid simple averaging over all fast and slow ascents (as in Fig. 6), which does not produce convincing results. Instead, a more detailed analysis of rapid segments would shed more light into the actual convective ascents.

57. l. 365ff: Can the authors please elaborate on the different PV evolution along the cyclonic and anticyclonic mid-level trajectories (Fig. 11b). What are the mechanisms that lead to these differences? Are these typical characteristics or only valid for trajectories at 16 UTC? Please clarify this part.

58. l. 367: I don't agree that the PV values of all categories are approximately the

same in the beginning and end. For example, frontal convection starts with on average ≈0 PVU and ends with ≈1 PVU. Please clarify or avoid this part.

5 DISCUSSION

59. l. 382-430: Why did the authors choose to name this section "discussion". As this section shows entirely new results and not a discussion of the previous results, I would suggest to rename it accordingly.
I appreciate that the authors show additional times of rapid segments, however, I think that the PV discussion is not yet fully mature and the relation of rapid segments, PV structures, and the low- and upper-level jet is unclear and speculative for the following reasons:
(i) "The results also suggest a link between convection and negative PV production": This appears speculative because negative PV structures frequently occur without rapid segments. Besides, the rapid segments coincide with high positive PV values at 11 and 21 UTC (Fig. 13b,f), while at 16 UTC they coincide with negative PV values (Fig. 13d). Hence, the effect of the rapid segments on PV appears unclear to me;
(ii) "the clustering approach shows that elongated negative PV bands persist for about 10 h": Did the authors track the individual PV bands or PV objects? How did the authors estimate the lifetime of negative PV bands? Are the negative PV bands simply advected or did new PV bands form between the different times?;
(iii) "locally intensify the jet stream": I cannot clearly see this relationship in Fig. 13a,c,e. While in Fig. 13a negative upper-level PV objects indeed coincide with a local jet maximum, this is not the case in Fig. 13c, where distinct negative upper-level PV objects at 61/62°N do not coincide with a local jet maximum. Instead the jet maximum at 9 km height (Fig. 13c) is located in a region where the top altitude of negative PV objects is mostly below 9 km height.
I would kindly ask the authors to clarify the analysis and/or avoid such detailed

conclusions.

60. l. 408: "The absence of rapid segments in the negative PV tower at 11:00 UTC suggests that it formed earlier and upstream". Without more detailed information this statement appears speculative. Please clarify.

61. l. 428: "Although a cause and effect relationship cannot be proven, the common shape, location and timing of the identified structures and fast ascents suggest that the organization of negative PV depends on the organization of convection." I do not disagree with this statement, however, I think that the presented results are not yet fully convincing and the conclusions appear rather speculative. Please shorten/remove this discussion about PV.

6 CONCLUSIONS

62. l. 433: "investigates a possible impact on the associated mesoscale and large-scale dynamics". I think that this aspect is too speculative and contributes only a minor part to the study, and thus, should be avoided in the conclusions.

63. l.438-439: "thanks to an online tool implemented in the Meso-NH model". This is rather technical and belongs in the methods section.

64. l. 439ff: Please see major comment 1 for my concerns about the WCB trajectory selection.

65. l. 441ff: This is contradictory (see major comment 1) and should be removed.

66. l. 446ff: Please consider the previous comments about the distinction between cyclonic and anticyclonic trajectories and adjust the conclusions accordingly (see major comment 4).

67. l. 458ff: "Finally, potential vorticity increases along cyclonic ascents – located mainly in the lower troposphere - and decreases along anticyclonic ascents - located mainly in the mid and upper troposphere." Here several aspects are mixed. Are the PV values to a first order determined by cyclonic versus anticyclonic or by low-level versus upper-level? I think it is the latter - please clarify. Besides, is there a clear effect of slow versus fast ascents on PV? Please streamline the conclusions and focus on the main topic of this study, which is organization of convective ascent".

68. l. 465: "understanding of their formation". How do radar observations provide a better understanding of the formation of fast ascents? I think, they rather provide additional evidence of the existence of fast ascents.

69. l. 476ff: Please streamline this part. In the conclusions it is off little relevance what was not analysed in detail.

70. l.484: "and should therefore have an impact on the upper-level jet stream". I think this is speculative and should therfore be avoided in the conclusions.

FIGURES

71. Fig. 1: caption: Replace "the position" by "cyclone track" or "evolution of MSLP".

72. Fig. 1a: It would be helpful if the authors would also add MSLP and θ_e from the ECMWF analysis in Fig. 1a for comparison with MESO-NH (similar to a comparison of the cyclone track). It might be helpful to color the θ_e contours to better see the fronts.

73. Fig. 2: caption: Please rephrase the caption and replace WCB frequency by "frequency of trajectories fullfilling the 150 hPa ascent in 12 h" or similar. See major comment 1. It would be helpful to color the θ_e contours. Besides, is the

trajectory frequency shown or the frequency of air parcels at 16 UTC. See also major comment 5.

74. Fig. 3: caption: "WCB trajectories" (see major comment 1).

75. Fig. 4: It is difficult to see the trajectory locations at the specified times in the figure (in particular the black dots at 16 UTC). Did the authors consider to show the location where the fastest ascent takes place, i.e., the so-called "rapid segments" (location where $\Delta P(2h) < -100$ hPa), too? Especially for the "fast ascents" in the lower troposphere it is difficult to see where the actual ascending motion takes place, as they mostly remain at low levels for the first 6 h (dark blue colors until 16 UTC).

76. Fig. 6: Could the authors please specify what is meant with the following sentence, I am not sure I understand it correctly: "The median and the 25th–75th percentiles for the 2 h rapid segments are shown with boxplots." Did you average over all rapid segments for each hour and regardless of which category they belong to? The anticyclonic slow and fast ascents (Fig. 6b) have very similar median vertical velocities? Is this meaningful? I would expect to see a difference between fast and slow, however, the difference between anticyclonic and cyclonic appears to be larger. Moreover, considering the median evolution of altitude versus time (Fig. 6a), there seems to be little evidence that fast versus slow ascents are characterized by very different averaged ascent behaviour (especially for the anticyclonic trajectories).

77. Fig. 7: The presentation of this figure could be improved. In (c,d) the "double hatching for values greater" is hardly visible. In (a,b) instead of using hatching for wind speed, could the authors add colored contours to highlight the region? The trajectory positions are difficult to see. In addition to all WCB air parcel positions, it would be interesting to highlight the location of rapid segments (as in Fig. 8).

78. Fig. 8: This figure is very busy. Could the authors somehow reduce or condense the content shown in the figure? For panel (a) a zoom on the target region where most of the updraft objects are located might improve the visualization. In (b) the grey and light blue contours are very difficult to see in the lower troposphere. Moreover, it is difficult to see the updraft objects, because the many lines cover the shading (e.g. at 23°W).

79. Fig. 9: See comments to Fig. 7.

80. Fig. 10: See comments to Fig. 8.

81. Fig. 12: The following sentence is unclear to me: "Only samples of 10 categories are plotted." Do the authors mean "Only 10 samples of the 4 categories are shown"? Moreover, it would be helpful to show θ_e contours to the see the frontal structure, especially for the category "frontal convection".

82. Fig. 13: This figure is very busy and it is difficult to see the individual negative PV objects. Can the authors zoom in and focus on a smaller region? In the regions with many rapid segments the dots sometimes cover the top altitude of negative PV objects. Moreover, I cannot clearly identify the mentioned PV dipoles in panels (b,d,f).

3 Technical corrections

1. l. 27: Typo: "WVB"

2. l. 103: Please replace "centered on the time" by "centered around the time"

3. l. 119-120: The word "troposphere" in "Mid-level troposphere clouds" is not needed.

4. l. 126: Please add a bracket here: equivalent potential temperature (θ_e)

5. Please replace "convection cells" with "convective cells" and try to be consistent with the wording. It is mixed throughout the text. Similarly, please consistently replace "potential vorticity" by "PV", equivalent potential temperature by θ_e, etc., once it has been introduced.

6. l. 160: What is meant by "upward trajectory"? Do the authors mean upward motion or ascent?

7. l. 169: I think a "**-**" is missing in "below **-**100 hPa 2h^{-1}".

8. l. 203: Please add a "s" in "few slow ascent".

9. l. 347: Please be consistent and use "frontal convection".

10. l. 351: Please replace "Lagrangian trajectories" by "trajectories". Trajectories are per definition Lagrangian features.

11. l. 456: Please rephrase the following sentence: "during which fast WCB ascents rise above the pressure threshold of 100 hPa (2h)$^{-1}$".

12. l. 476: Please replace "several hundred km" by "several hundreds of kilometers".

4 References

Binder, H., M. Boettcher, H. Joos, M. Sprenger, and H. Wernli, 2020: Vertical cloud structure of warm conveyor belts – a comparison and evaluation of ERA5 reanalyses, CloudSat and CALIPSO data. Weather Clim. Dynam. Discussions, 2020, 1–28, doi:10.5194/wcd-2020-26.

Maddison, J. W., S. L. Gray, O. Martiíez-Alvarado, and K. D. Williams, 2019: Upstream Cyclone Influence on the Predictability of Block Onsets over the Euro-Atlantic Region. Mon. Wea. Rev., 147 (4), 1277–1296, doi:10.1175/ MWR-D-18-0226.1.

Madonna, E., H. Wernli, H. Joos, and O. Martius, 2014: Warm conveyor belts in the ERA-Interim dataset (1979-2010). Part I: Climatology and potential vorticity evolution. J. Climate, 27, 3–26, doi:10.1175/JCLI-D-12-00720.1.

Martínez-Alvarado, O., H. Joos, J. Chagnon, M. Boettcher, S. L. Gray, R. S. Plant, J. Methven, and H. Wernli, 2014: The dichotomous structure of the warm conveyor belt. Q. J. R. Meteorol. Soc., 140, 1809–1824, doi:10.1002/ qj.2276.

Wernli, H., M. Boettcher, H. Joos, A. K. Miltenberger, and P. Spichtinger, 2016: A trajectory-based classification of ERA-Interim ice clouds in the region of the North Atlantic storm track. Geophys. Res. Letters, 43, 6657–6664, doi: 10.1002/2016GL068922.

Wernli, H., and H. C. Davies, 1997: A Lagrangian-based analysis of extratropical cyclones. I: The method and some applications. Q. J. R. Meteor. Soc., 123, 467–489, doi:10.1256/smsqj.53810.