Manuscript WCD-2021-76

"Diabatic processes modulating the vertical structure of the jet stream above the cold front of an extratropical cyclone: sensitivity to deep convection schemes"

Authors' response

Dear Editor,

We would like to thank first the two referees for their deep analyse and their relevant remarks that helped to improve the quality of our manuscript. Please find below our point by point answer to the reviewers' comment. Replies to reviewers are in blue, while reviewers' comments are in black.

Reply to referee 1

This study presents a detailed analysis of the influence of using two different deep convection parameterization schemes on the wind speed in the mid-troposphere and associated PV structure above the cold front of an extratropical cyclone. Therefore, two simulations with different convection schemes are compared to each other, as well as to three (re-) analysis data sets and airborne observations of ice water content and wind speed. Furthermore, backward trajectories are used to show that differences in the PV structure in both simulations are related to diabatic processes behind and ahead of the cold front. The authors find that using different convection schemes results in differences in the representation of diabatic heating ahead of the cold front, which modifies diabatic PV modification and finally influences the tropopause structure, associated PV gradients, and the jet in the middle troposphere. Although various different datasets are employed in this study, it remains elusive as to which convection scheme is more realistic, as both model simulations are in between the (re-) analyses, both models strongly underestimate ice water content, and both show a bias in the jet structure. While this analysis focuses on one specific time and vertical cross-section only, the (systematic) impact of the different convection schemes is a timely question and fits the scope of Weather and Climate Dynamics. I recommend the publication of this manuscript, however, I have several comments and questions that should be addressed before publication and are listed below.

We would like to thank the referee for accepting to review our second paper and for his comments that helped to improve the content and clarity of the paper. The point by point answers to the comments are hereafter provided.

General Comments

1. Direct impact of convection schemes for observed differences
   While the differences between both simulations with PCMT and B85 in terms of air mass transport and diabatic processes are described in detail, I would appreciate if the differences could be related more closely to both convection schemes throughout the manuscript. For the mid-tropospheric jet, a clear difference in air mass origin is found between PCMT and B85. Could the authors explain in this section how the different heating patterns and thus trajectory pathways are related to the convection schemes? I understand that the companion paper part I deals with this topic more closely and it is briefly mentioned in the introduction, however, I would appreciate a reminder of the actual cause of the differences in the according paragraphs.

   The origin of the differences in heating and trajectory pathways are closely linked to the deep convection closure as shown in the supplementary figures. All the simulations run with the CAPE closure (e.g., PCMT) resemble the simulation where no deep convection is activated and behave differently than the simulations run with the moisture closure (e.g., B85). Our conclusion is that the CAPE is not strong enough along such warm conveyor belts (WCBs) to trigger the convection scheme while the moisture convergence contains a synoptic-scale pattern that triggers the convection scheme more systematically in the WCBs. These results were already found when looking in the WCB outflow region in the first paper and are recalled in the introduction. In the present paper, we show similar behavior above the cold front in the ascending branch of the WCB which has some impact on the representation of the vertical structure of the jet.

   In future studies, sensitivity experiments will be performed to more systematically investigate the sensitivity to the closure and get a deeper insight of its effect along WCBs and the jet stream.

2. Focus on one specific location and time
   The paper focuses on the differences between the simulations at 15 UTC and at one specific location. I assume this is motivated by the availability of observations at that time and location. However, I was wondering if the observed differences are somewhat representative of the evolving tropopause and jet structure? How do the differences evolve with time? I understand that the trajectories provide a temporal evolution of diabatic processes, but they are still specifically related to the single time step and cross-section. Considering that the simulations are already at a lead time of more than 24 h, how relevant are small differences in timing and spatial shifts between the simulations when the differences for one specific cross- section are evaluated? Do the differences consistently grow with increasing lead time?

Even though the differences between simulations are shown for a given time and a specific location along the cold front they are representative of systematic differences. Indeed, similar differences appear when looking at different lead time and for other hindcast simulations starting at different initial times. Figure 1 (below) shows the wind speed at 600 hPa for B85 (first column) and PCMT (second column) and also the difference of the PV between the two runs (third column) by initializing the simulations at three distinct initial times: 00 UTC 1 Oct, 12 UTC 1 Oct and 00 UTC 2 Oct. In each case, the wind speed is stronger in PCMT than B85 and is associated with a band of positive PV difference to the west and a band of negative PV difference to the east along the cold front. For the run starting earlier (00 UTC 1 Oct), a third band is visible with positive values of the PV difference. According to figure 1, differences seem to increase with lead time. Interestingly, hindcast simulations of another cyclone (see figure 2 below) show the same difference between PCMT and B85 and a clear tripolar PV anomaly is visible (Fig. 2c). Therefore these differences seem to be systematic and are not specific to the time and location presented in the paper. In the revised paper, we mention these different hindcast simulations to underline the recurrence of these differences.

3. Mid-tropospheric jet
The major part of this manuscript analyses the difference in wind structure at approximately 600 hPa, and the presence of a secondary jet at that level in some datasets. While the term 'jet' is not necessarily limited to maximum wind speed in the upper troposphere, it is common to define the jet in the upper troposphere (e.g., https://glossary.ametsoc.org/wiki/Jet stream). While the introduction provides a detailed review of upper-level jet literature, I would appreciate if the authors could also include some introduction about the relevance of these mid-tropospheric jets (e.g., Georgiev and Santurette, 2009, https://doi.org/10.1016/j.atmosres.2008.10.024; Kaplan et al., 2009, https://doi.org/10.1175/2009JHM1106.1). Furthermore, it might be helpful to slightly adjust the title and add that the mid-level jet is one of the foci. This would also more clearly distinguish this manuscript from the companion paper RW21 ('The impact of deep convection representation in a global atmospheric model on the warm conveyor belt and jet stream during NAWDEX IOP6').

We would like to thank the referee for mentioning these two references that we were not aware of. However, the main band of wind speed maximum discussed in the present paper (see e.g., Fig.1 of the present document or Fig. 3 of the paper) corresponds to the lower part of the upper-level jet and not to a mid-tropospheric jet per say. It is only the secondary band of wind maximum to the northeast of the main one that appears to be a mid-tropospheric jet in some simulations (e.g., see Fig. 5a of the paper near the trajectory index 70). But this secondary jet is not the focus of the present study. For these reasons, we think it is better not to go into the details of mid-tropospheric jet literature in the introduction and we decided not to change the title of the manuscript. In the revised paper, the two references will be cited when the secondary jet is described but no deep investigation on that jet is provided.

4. Length of the paper
The manuscript in its present form is rather long and I think it could be shortened. I would appreciate if in particular parts of sections 3 and 4 could be re-phrased and streamlined to improve readability with a focus on the relevant processes. Please see also specific comments below.

As suggested in the following specific comments, some paragraphs have been shortened and some sentences deleted. In particular, the text describing Figs. 2 and 3 has been reduced and provides less details to more directly go the point.

Specific comments

ABSTRACT

1. l. 7: 'jet core in middle troposphere': Based on Fig. 2, I would place the 'jet core' rather between 300 and 400 hPa. How do the authors define 'jet core' ?

By 'jet core', we mean area with high values of wind speed. However that formulation brings confusing information and thus has been removed.

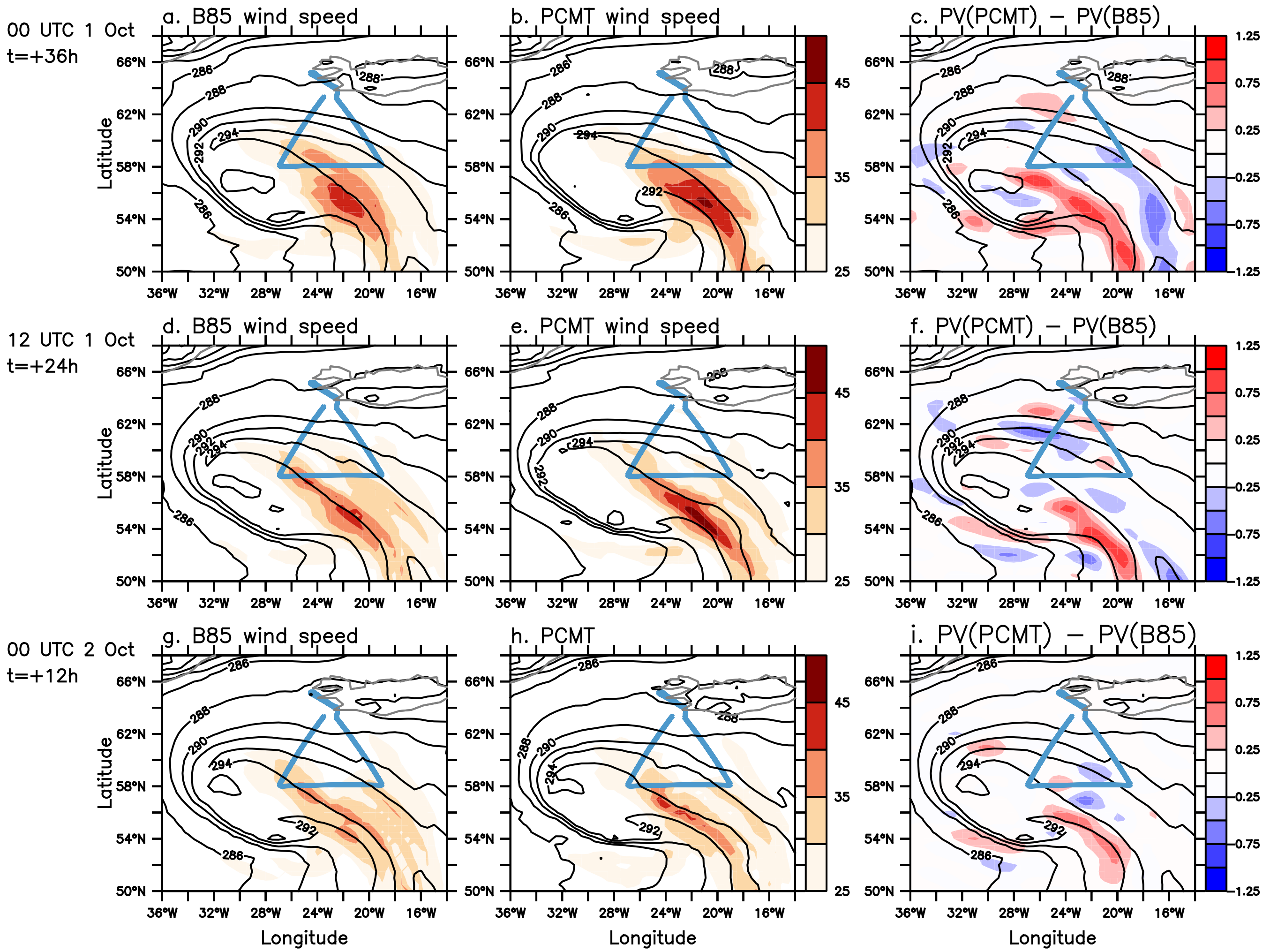

Figure 1. First and second columns show the 600-hPa wind speed for B85 and PCMT respectively at 12 UTC 2 Oct. The third column correspond to the PV difference (PCMT-B85) averaged between 550 and 650 hPa. The black contours represent the potential temperature averaged between 750 and 850 hPa. Each line correspond to a different initial time: (a)-(c) 00 UTC 1 Oct, (d)-(f) 12 UTC 1 Oct, (g)-(i) 00 UTC 2 Oct. The lightblue segments correspond to flight F7.

1 INTRODUCTION

2. l. 26 Please define NWP when it is first introduced.

   NWP stands for Numerical Weather Prediction. A definition of that acronym has been added.

3. l. 40f: 'very short-term forecasts'. Please specify the lead time.

   In that study, Schäfler et al. (2020) showed an underestimation of vertical wind shear in ECMWF-IFS and MetOffice MetUM forecasts from +2 to 4h, and from +8 to +10h respectively. Thus, we added a '(up to 10h)' in the sentence : 'Using NAWDEX observations as a reference, Schäfler et al. (2020) showed underestimation of vertical wind shear in the vicinity of the tropopause in very short-term forecasts (up to 10h)'

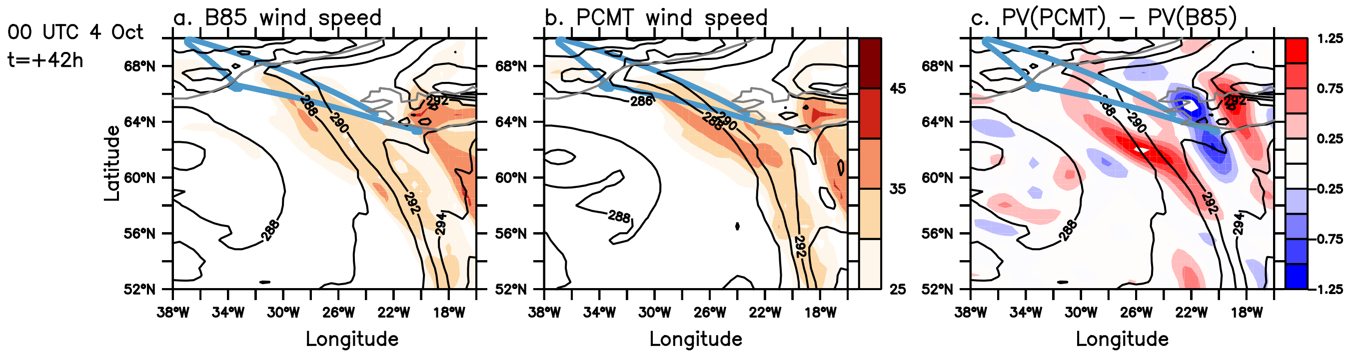

Figure 2. Same as Fig. 1 but for the initial time at 00 UTC 4 Oct and a valid time at 18 UTC 5 Oct, hence a forecast of 42 hours. The cold front belong to the IOP7 cyclone (the cyclone that deepened just after the Stalactite cyclone). The lightblue segments correspond to flight F9.

4. l. 41: ’analysed this could affect Rossby wave propagation’: This sounds speculative. Please clarify if it was analysed or hypothesized.

   The underestimation of the vertical wind shear has been diagnosed in analyses and short-term forecasts in Schäfler et al. (2020). The end of their study discusses the potential implication of that underestimation for Rossby wave propagation and in that sense it is more an hypothesis. To be more precise on their reasoning we complete the sentence as follows: "Schäfler et al. (2020) showed underestimation of vertical wind shear in the vicinity of the tropopause in very short-term forecasts and analysed this could affect Rossby wave propagation by altering the strength of the PV gradient."

5. l. 45ff and l. 53ff: It seems as if the authors split the ’PV tracer’ and the ’Lagrangian PV framework’ in two separate methods (I may have misunderstood these two paragraphs). If the authors would like to keep these methods separated, please double-check the referenced literature and which of the methods was applied for which study. Some of the mentioned studies apply both methods. Low-level PV anomalies in extratropical cyclones have also been analysed systematically in Attinger et al. (2021; https://doi.org/10.5194/wcd-2-1073-2021).

   The ’PV tracer’ and ’Lagrangian PV framework’ are not really two distinct methods. The ’PV tracer’ technique can be seen as one specific technique embedded in the broader ’Lagrangian PV framework’ as it decomposes the PV tendency into distinct nonconservative processes using the advection scheme of a model. But we may have a Lagrangian PV approach without necessarily using such a ’PV tracer’ technique. Thus, we change the beginning of the second paragraph as follow: ’The PV and potential temperature Lagrangian framework in general can be also used’

   In order to add the Attinger et al. (2021) reference, we modify the paragraph as follow : ’The PV tracer technique that decomposes the PV rate of change into different model processes has been widely used during the last decade, mainly to study the near-tropopause PV anomalies associated with the jet stream (Chagnon et al., 2013; Martinez-Alvarado et al., 2014; Saffin et al., 2017; Spreitzer et al., 2019; Harvey et al., 2020). Such technique has also been used to study low-level PV anomalies associated with extratropical cyclones (Crezee et al., 2017; Attinger et al., 2021)’.

6. l. 56ff: I appreciate the comprehensive overview of literature here, however, I think re-phrasing this paragraph would help the reader. Instead of writing ’author 1 et al. showed’, I would suggest summarizing the existing results in terms of impact on the PV and jet structure.

   We summarize the paragraph as follow : ’Joos and Forbes (2016) and Mazoyer et al. (2021) used this approach to analyse the sensitivity of the jet stream structure and WCB to distinct cloud microphysics schemes at 2-3 days lead times. Both found some effects of the microphysics representation on the WCB and the tropopause position along the edge of the ridge building, using the ECMWF-IFS global model in Joos and Forbes (2016) and using a regional convection permitting model in Mazoyer et al. (2021).’

7. l. 73ff: The description of the results of the companion paper is essential for this study. I suggest using a separate paragraph for the description of the relevant main results from RW21. Please also clearly state the key results relevant for the study at hand.

   We are not entirely sure to understand the referee's suggestion. The paragraph from line 73 to 90 in the initially submitted version is precisely dedicated to summarizing the approach and the main results obtained in RW21. The main objective of the present paper is to focus on another region of the extratropical cyclone, that is along the cold front and the ascending branch of the WCB. In the revised version, we modify the last sentence to make our objective clearer.

8. l. 85: 'the heating extends further upward'. Does this relate to total heating from microphysics and convection scheme or convection scheme only?

   This is related to the total heating calculated by finite differences but also the total heating calculated from the addition of all parameterization heatings. However, as that total heating is mainly due to the large-scale cloud scheme, it is mostly the large-scale heating which extends further upward. The sentence has been changed as follow : 'RW21 showed that the run in which deep convection is more active (B85) is also the one for which the total (from all parameterization) heating extends further upward above the warm front of the extratropical cyclone and has a stronger PV destruction at upper levels. '

9. l. 86: 'This leads to a distinct location of the jet stream'. Please clarify 'distinct location'.

   Comparing the three simulations (B85, PCMT and NoConv), the jet stream has a different position: in B85, the jet stream is located few hundred kilometers west to the one in PCMT and NoConv. This implies a different horizontal position of the jet stream. We change the sentence to 'This leads to a shift of the jet stream of about hundred kilometers to the west, compared to the other two runs.'

2 DATA and METHOD

10. l. 144 and 148: 'Figure 1b gives an insight of the cloudy region sampled by the flight.' and 'During this flight, different measurements have been made.' I think these sentences are not necessary, and the manuscript could be shortened by avoiding such sentences. I would kindly ask the authors to streamline these paragraphs.

    These two sentences have been removed as they do not bring key information.

11. l. 150: 'LATMOS and DT-INSU': Please specify if relevant, else remove.

    As it is not very important and references related to the lidar and radar are already cited, that information has been deleted.

12. l. 158: The in-situ wind measurements were already mentioned in l. 154.

    We change the sentence to 'Additional wind measurements were made by dropsondes too.'

13. l. 165: Why did the authors decide to use only every 2nd grid point for the ERA5 dataset, instead of also interpolating the data as done for the other datasets?

    All the operational analyses and reanalysis are considered at the same 0.5° grid. As the ARPEGE and IFS model are post-treated (interpolation) to get output on a regular grid of 0.5°, the same resolution is used for ERA5. As ERA5 has a 0.25° resolution, we just use one every 2 grid point to get a 0.5° resolution. This solution, contrary to interpolation, has the advantage to be closer to the original ERA5 data. Furthermore, it leads to exactly the same grid than ARPEGE and IFS model grids.

    The paragraph is now : 'ERA5 reanalysis (Hersbach et al., 2020) and operational analyses of the ARPEGE and Integrated Forecasting System (IFS) models are used at same vertical resolution of 50 hPa and horizontal resolution of 0.5° than the ARPEGE simulations outputs.'

14. l. 169ff: Why did the authors decide to select trajectories that ascend 300 hPa only? In which vertical levels were the trajectories initialized?

    The common criterion to select WCB trajectories is an ascending of 600 hPa in 48 h (Joos and Wernli, 2012). As computed trajectories last 48h, a selection of an ascending of 600hPa in 48h leads to a too small number of trajectories. In order to get a more populated set of trajectories, a similar but less restrictive criterion of an ascent of 300 hPa in 24 h is thus considered.

    Trajectories are seeded between 1000 hPa and 800 hPa with a vertical resolution of 20 hPa. Thus, the paragraph is now: 'They are initialized at 12 UTC on 1 October in the warm sector of the extratropical cyclone and last 48 hours. These trajectories are seeded in a box from 50°W to 20°W, 35°N to 56°N and 1000 hPa to 800 hPa, with a horizontal resolution of 0.5° and a vertical resolution of 20 hPa. To select WCB trajectories, a criterion of an ascent exceeding 300 hPa within 1 day during the period between 12 UTC on 1 October and 12 UTC on 3 October is applied. This is a less selective criterion than the more usual ascent of 600 hPa in 2 days but has the advantage of selecting a larger set of trajectories.'

15. l. 175: 'over the whole vertical'. Is there a word missing?

    The word 'axis' is missing and has been added ('over the whole vertical axis').

16. l. 175f: 'trajectories are seeded on a vertical regular grid spacing of 12.5 hPa from 975 hPa to 200 hPa and a horizontal grid spacing of about 0.3 in longitude and latitude': Is it meaningful to seed trajectories at a higher vertical and horizontal resolution than the actual data they are computed from (which is 0.5 in the horizontal and 50 hPa in the vertical if I'm correct)?

    It is meaningful as trajectories belonging to the same bin (i.e in between two adjacent grid points) may deviate with time. Indeed, the advection wind components used to compute the next trajectory position are calculated with a bilinear interpolation and will be different for trajectories with close seeding points.

17. l. 178ff: I appreciate the detail concerning the starting times for the trajectories. Just out of curiosity, did the authors check if the timing is critical for the differences described later? The simulations are already at a lead time of approx. 27 h, and I was wondering how important small shifts in timing are. If trajectories are started a little earlier or later, does PCMT still show two different clusters while B85 only shows one (e.g., Fig. 7)?

    We tested two other initial times for trajectory seeding. Instead of using the mid-time of each leg, we use the initial and final time of each leg. This leads to very small differences in the results.

18. l. 200ff: Do I understand correctly, the PV tendencies for friction and diabatic heating are computed differently (method 1 for friction vs. method 2 for diabatic heating)? How large are the differences between methods 1 and 2? If they are large, doesn't this introduce uncertainties if the PV tendencies from friction and heating are compared?

    If method 1 is applied for both friction and heating, the qualitative picture does not change because the two types of trajectories for the two runs behave very differently and our PV budget is already good enough to explain such large differences. However, the sum of these two terms less accurately follows the diagnosed PV tendency than when the heating is computed using method 2. This is the reason why we chose to use method 2 for the computation of the heating to show the PV budget. The difference between the two methods has been discussed in our previous paper where Figures S1a-b and S2a-b in RW21 compare the heating patterns obtained by the two methods.

3 SECTION 3

19. l. 213ff: Interestingly, ERA5 shows more negative PV in the cross-section shown at 15 UTC, while in the companion paper RW21 Fig. 8 suggests that the ARPEGE analysis is characterized by enhanced occurrence of negative PV at 12 UTC. Are these differences related to specific timing, location, or selected vertical level?

These differences are related to the specific time and location of that different PV anomalies. When looking at other cross sections of the cold front, such a behavior for ERA5 is not systematically found. We just observe that the PV gradient is a bit stronger in ERA5 above the cold front than in the other two analyses. Since this section is a bit lengthy as already mentioned by the referee and the purpose of Figs. 2 and 3 is more about the difference between the 3 simulations, we do not put an emphasis on these aspects of analysis/reanalysis.

20. l. 233: Please mention which datasets.

    The datasets are ECMWF-IFS, ERA5, B85 and PCMT.

21. l. 256: Is it correct that member 3 uses B85 but with a CAPE closure? Please clarify in the manuscript.

    Member 3 uses B85 but instead of the moisture convergence closure, it has been modified to use a CAPE closure. The sentence is now : ' In the other group, all members share a CAPE closure : members 6, 7 and 8 use the PCMT scheme and member 3 use a modified version of B85 with a CAPE closure too.'.

22. l. 256ff: I find the results in this section very interesting, because it provides a more systematic assessment and does not only show two different simulations. In the supporting material, it is difficult to compare all of the members. Did the authors try to compute vertical cross-section composites for the members in both clusters (i.e., moisture convergence vs CAPE closure)? I understand that averaging may smooth out some differences, but I would be interested if systematic differences remain between both clusters.

    Figures 3 and 4 of the present document are the equivalent of Figures 2 and 3 of the manuscript but show the composites of both clusters, one made with members having the moisture convergence closure and the other with members having the CAPE closure. The jet stream extends more deeply in the mid troposphere for the CAPE closure composite than for the moisture closure composite (compare Fig. 3a with Fig. 3b and Fig. 4a with Fig. 4b). The difference thus remains between the two clusters. We prefer to keep the supplementary figures S1 and S2 as they are in the initial version of the paper where the individual members are shown.

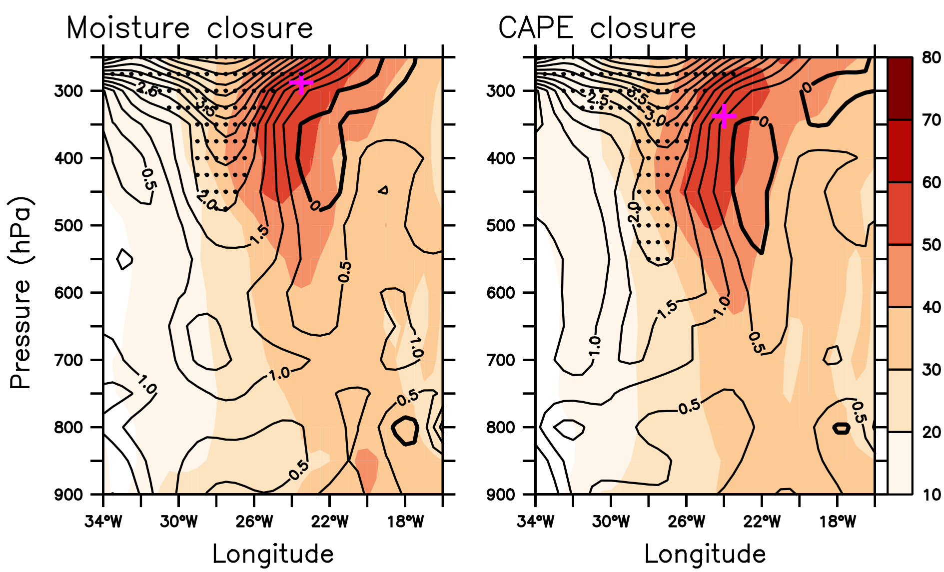

Figure 3. Vertical cross section at 58° N of the mean zonal wind (shadings) and mean Potential Vorticity (black contours with hatched areas for values superior to 2PVU and bold contour for 0 PVU) at 15 UTC, 2 October 2016 for a) all PEARP members using a moisture convergence closure for deep convection scheme and b) all PEARP members using a CAPE closure.

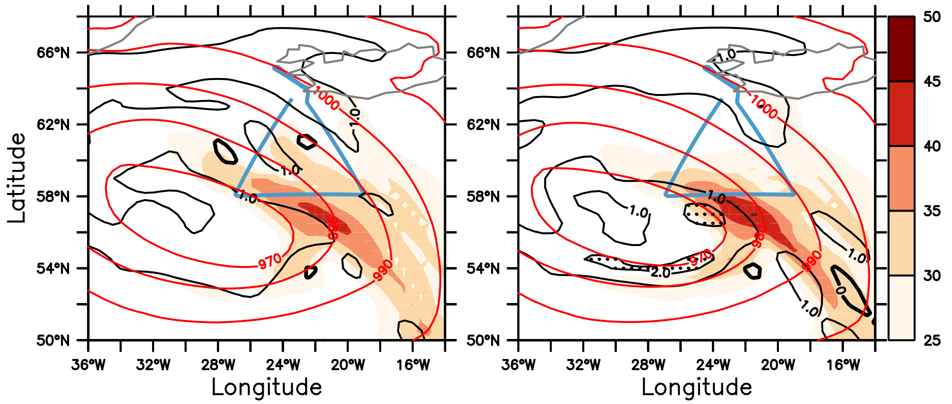

Figure 4. Mean Wind (shadings) and mean Potential Vorticity (black contours with hatched areas for values superior to 2 PVU and bold contour for 0 PVU) at 600 hPa with sea level pressure (red contours) at 15 UTC, 2 October 2016 for a) all PEARP members using a moisture convergence closure for deep convection scheme and b) all PEARP members using a CAPE closure.

23. l. 261: 'smaller negative PV values to the east in the mid-troposphere around 600 hPa': I cannot see a systematic difference of neg. PV between the datasets in Fig. 3. It appears very patchy and non-systematic in the simulations and the (re-)analyses. Is this a relevant difference?

    Even though the negative PV values ahead of the cold front are patchy, this is a systematic behavior of the less active scheme (i.e CAPE closure) compared to the most active scheme (i.e moisture closure). This does not necessarily occurs at the same place along the cold front but we do see systematically more negative PV patches for the less active scheme ahead of the cold front (see members 3, 6, 7 and 8 on the one hand and members 1, 2, 4, 5, 9 on the other hand in Figure S2). Also see the systematic negative values of the difference in PV between the two schemes in Figures 1 and 2 of the reply document by changing the initial conditions or even by changing the case study.

4 SECTION 4

24. l. 273ff: What is the relevance of the curvature of the trajectories, in particular, if there is no clear separation? As this is not used in the following, I would suggest removing this paragraph.

    As the curvature of the trajectories is not relevant, the paragraph has been removed as suggested.

25. l. 282ff: Please streamline this short paragraph.

    The paragraph is now : 'The pressure along these trajectories, represented in color, shows two ascending regions in the vicinity of the flight (e.g., near 25° W; 50° N and 40° W; 62.5° N). This confirms the flight clearly occurs in the main ascending region of the WCB.'

26. l. 299: Why was neither time nor longitude used for the abscissa? I'm not sure, I fully understand what exactly 'number of trajectory seeds' refers to? Is this a pseudo-longitude?

    Trajectories are seeded on a grid with 63 points on the vertical axis and 84 points on the horizontal one. On the vertical axis, trajectories are seeded between 975 and 200hPa every 12.5hPa.

    On the horizontal, seeds follow the flight travel. Firstly, 32 seeding point are defined for the first leg, from 22.5°W 63.3°N to 19°W 58.6°N. For the second leg, 20 seeding point are defined between 19°W 58.6°N (last position of leg1) to 26.9°W 58°N. For the third leg, 32 seeding point are defined between 26.9°W 58°N (last position of leg2) to 22.5°W

63.3°N (first position of leg1). Thus, this lead to 84 seeding points approximately separated by 0.3° in longitude and latitude, and ordered in the same way than the flight. They define the trajectory index.

As these seeding points does not fit perfectly the flight position, we do not consider flight position as abscissa. Time cannot be used either as all trajectories from a same leg are initialized at the same time. The geographical position has been avoided as trajectory seeds have different latitude and longitude positions and the use of such position may be too heavy in the figures. In opposition, this trajectory index, as trajectories are seeded in the chronological order of the flight, can be associated to the time along the flight.

27. l. 323: It is clear from Fig. 5, but I think it would be helpful if the authors mentioned that the jet between 500 and 700 hPa is analysed.

The paragraph is now : 'To better explain the deeper jet stream in PCMT, the next section focuses on the jet between 500 hPa and 700 hPa, where differences between PCMT and B85 are the highest. Particularly, the origins of the positive PV difference (black dots in Figs. 5a-b) and negative PV difference (green dots) located on the warm side of the jet stream are studied.'

5 SECTION 4.1

28. l. 325ff I would suggest to restructure this section and to discuss the origin of trajectories first, instead of directly starting with PV evolution and detailed tendencies. While reading, I immediately wondered if the large difference in PV results from different ascent pathways, which is discussed in the manuscript at a later stage. I would find it more intuitive and easier to follow if switched around. This way, it would also be more straightforward to understand the PV evolution.

Our reasoning relies on the following steps:

(a) Wind differences are described in Figs.2 and 3 of the paper and are found to be related to PV gradient difference.

(b) The explanation of the PV differences is provided by computing Lagrangian backward trajectories starting from the large PV differences in mid-troposphere (Fig.5 of the paper).

(c) Analysis of the PV evolution along trajectories is then made to identify the time when the PV values diverge between the two types of trajectories (Fig.6 or Fig.8).

(d) Finally, a focus is made at the time when the PV values diverge (Fig.7 or Fig. 9).

We do think that such an order is relevant and we do not see the need for a re-formulation of this section. However, to make clearer our reasoning these different steps will be more clearly stated when introducing sections 4.1 and 4.2.

29. Fig. 6: I assume Fig. 6 without considering the ascending trajectories in PCMT would result in a similarity between B85 and PCMT? Is this correct, i.e., are the major differences only caused by the ascending portion of trajectories?

Figure 6 without the ascending trajectories in PCMT would not result in a similarity between B85 and PCMT. The PCMT ascending trajectories only explains the positive PV differences between 44 and 47 trajectory index in Figure 5a and b. While the constant-level trajectories, which pass over a different cooling between B85 and PCMT, undergo different PV tendencies. This cooling difference explains the positive PV difference between 47 and 59 trajectory index in figure 5a and b (see lines 362-363 of the initially submitted version).

30. l. 336ff: How close do the individual PV tendencies get to closing the budget, i.e., if the tendencies are summarized, how large is the deviation between a) and b)?

As already emphasized in Spreitzer et al. (2019, their section 4b), there are different reasons why the sum of the diabatic and friction-related PV tendencies is not necessarily equal to the net PV tendency: 1) the dynamical core of the model does not necessarily conserve PV due to numerical diffusion, 2) many interpolations intervene, 3) offline Lagrangian trajectories are computed on a coarse grid 4) finite-differences schemes are applied on coarse-grid model outputs. The fact that the sign of the sum of diabatic and friction-related tendencies provides a good approximation for the sign of the net PV tendency is already something satisfying. It does not mean that the residue is small. But such a budget is enough to clearly identify the origins of the PV differences between the two types of trajectories belonging to these two distinct runs. It would be more problematic to use such a budget to compare runs and trajectories with smaller differences.

31. l. 342ff: Although a little smaller than the heating induced PV tendency, I think the frictional PV tendency is still relevant for the PV budget in PCMT. Is the difference between the frictional PV tendencies caused by the ascending trajectories in PCMT (Figs. S3 and S4 suggest that at least turbulent heating is most important below 600 hPa)?

    Indeed, the frictional PV tendencies are not negligible for PCMT and their negative values have much stronger amplitude than for B85 because of ascending trajectories from the boundary layer in PCMT. However, this cannot provide an explanation for the larger PV obtained in PCMT than B85 because it goes in the opposite direction. So it is the heating that makes the difference in PV at the end between the two runs but is partly compensated by differences in frictional processes.

32. l. 344ff: Considering that all trajectories in B85 and a large number of trajectories in PCMT remain at the same pressure level and do not ascend, I find it critical to define these as WCB trajectories. After these trajectories crossed the flight track, do they continue to ascend? I would suggest to re-name this cluster of trajectories or thoroughly discussing this topic.

    Trajectories that do not satisfy 300 hPa ascent in 24 h should not indeed be referred as WCB trajectories. This is modified in the revised paper.

33. l. 347f: The PV framework and diabatic PV modification have not been explicitly introduced in the introduction. I think it is also not necessary in this manuscript, in particular, because it is introduced in RW21. However, I think some references and one explanatory statement may be useful here for readers who are less familiar with this concept.

    We added that short summary of this concept: 'Indeed, as the PV tendency is proportional to the heating vertical gradient (see e.g., Fig. 4 of Wernli and Davies, 1997), the PV tendency is positive under the heating and negative above.' Additionally, reference to the PV tendency equation shown in RW21 is provided in subsection 2.5.

34. l. 359f: 'the two PV start to get away from each other': Please re-phrase.

    We change to : 'when PV values of each simulation are enough different to each other'

35. l. 364: I find it interesting that the convection schemes impact substantially the turbulence scheme and the structure of large-scale cloud heating (Figs. S3 and S4). Do the authors understand why and how?

    The heating term due to turbulence is large in the boundary layer behind the cold front where the ocean is warmer than the air and surface heat fluxes are upward oriented. As shown in Fig. S3 and S4, the heating due to parameterized convection is large between the top of the boundary layer and the mid troposphere suggesting that the warming of the boundary layer in the cold sector destabilizes the lower free troposphere and activates the convection schemes. The fact that the heating patterns due to parameterized convection and turbulence are not located at the same heights create opposite-sign PV tendencies that largely compensate with each other as already pointed out by Spreitzer et al. (2019, J. Atmos. Sci., see Figs. 4b and c). The cold sector being not the main focus of the paper the heating budgets are not discussed in that area in the main text.

    Another point raised by the reviewer concerns the interplay between the convection schemes and the large-scale cloud heating. This is more something that we describe in the first paper. In the case there is no deep convection scheme activated, the release of convective instability occurs at resolved scales and this forms isolated patches of strong heating ahead of the front and this appears as the contribution of the large-scale cloud heating. In other words, there is a strong dependence of the large-scale heating on the triggering of the deep convection scheme.

36. l. 372ff: I appreciate the summary paragraphs at the end of each subsection. Could the authors discuss additionally how the observed differences are related to the different convection parameterization schemes? It summarizes the difference

in terms of large-scale heating and PV modification, however, it lacks to discuss the underlying causes (i.e., different convection parameterization).

As suggested by the referee, we will add an interpretation for such PV and heating differences. So far we do not have a full explanation. Our interpretation is as follows. As PCMT is less active than B85 (closer to NoConv), the convective instability is released at resolved scales generating heating over numerous grid points. This leads to an overlapping of the heating area with the front itself in PCMT and NoConv (see figure 5 of the present document) and ascending air masses on both sides of the jet and not only on the warmer-air side. The separation between the frontal zone and the heating area ahead of the front is clearer in B85 (compare Fig.5a with Fig.5b,c). This does not provide a full chain of causes but might be worth investigating in the future.

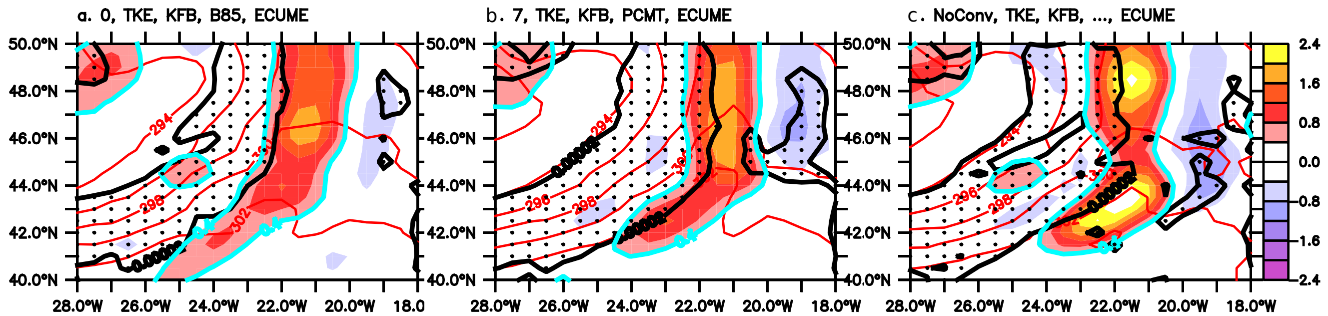

Figure 5. Averaged heating between 700 and 850 hPa (shading, 0.4K/h in cyan contour), potential temperature (red contours, interval 2K) and potential temperature gradient over 2×10^{-5}K/m in (stippled areas) for a) B85, b) PCMT and c) NoConv

6 SECTION 4.2

37. l. 382: I assume the backward trajectories considered in the following are the green dots in Fig. 5. If this is correct, it may be helpful to mention here.

    It has been added between parenthesis : 'The same approach based on backward trajectory (seeded at the green dots in Figs. 5a-b) is adopted to better understand the origin of the negative PV difference to the east of the jet, which is mainly embedded in the WCB region.'

38. l. 389f: I agree, but I think that the frictional PV tendency for PCMT is non-negligible as it strongly reduces the differences between PCMT and B85.

    Here, the higher PV tendency values in PCMT is due to the heating PV tendency. We agree that the frictional PV tendency is non negligible, but does not explain the higher PV tendency value. Thus, the sentences has been changed : 'This difference is due to the higher PV tendency in PCMT which results from the higher heating term in the PV tendency budget. The friction contribution only partly offsets the differences due to the heating terms during the PV increase phase. '

39. l. 402f: The authors describe an interesting systematic tilt of the heating in PCMT. Do the authors understand or have a hypothesis what causes these differences in heating pattern between PCMT and B85?

    This tilt is also a systematic feature seen in the members having the CAPE closure but we do not have hypothesis for the origin of such a tilt. This aspect will be investigated in future studies when the effect of closure will be more deeply analyzed.

40. l. 410: 'heating which is more confined at lower levels': this could be an option to link the general WCB analysis (section 4) to the specific analyses in sections (4.1 and 4.2) through refering back to Fig. 4 and dicussing the differences in heating pattern.

The more confined heating in the lower troposphere in PCMT can be indeed linked to results from Fig. 4: part of the differences comes from the liquid phase as seen below the iso-0°C in Figs.4c-d. Thus, that information has been added in the discussion: 'Such vertically heating difference has already been observed in Fig. 4 and is partly linked to a different behaviour of deep convection schemes in the liquid phase'.

7 SECTION 5

41. The large IWC differences in magnitude between simulation and observations are interesting. Were such large differences expected by the authors? Do the authors think that this is disconcerting? How does IWC from the (re-)analyses compare with (i) the simulations, and (ii) the observations? How reliable are the retrievals? Apart from (Mazoyer et al., 2021), have other studies compared the estimated IWC with model results?

Such underestimation of IWC by models can be expected as it has also been noticed in other studies with other models (see for example, Heymsfield et al. (2017) and Flack et al. (2021) with global climate models, Rysman et al. (2018) or Mazoyer et al. (2021) with NWP limited area model). As already said in the paper, we tested different snow fall velocities but the retrieved IWC still does not reach observed values. Furthermore, (re-)analyses also underestimate IWC compared to observations (see figure 6b). The underestimation of ERA5 IWC relative to the observations is more important than the one mentioned in Binder et al. (2020). Hence, the underestimation of IWC in models and (re)-analyses seems systematic even though the factor of underestimation varies from one study to another. That must be deeply analysed in future studies but it is not in the scope of the paper. The retrieved IWC obtained from RALI measurements using the VARCLOUD algorithm can be considered as reliable as it has been favourably compared to in-situ measurements in several past studies (see Cazenave et al. (2019) for a recall of such comparison studies). Furthermore retrievals using the radar measurements only and using both lidar and radar measurements respectively (Figs. 10a-b of the paper) are quite close to each other and well above the IWC values of the models. It suggests that uncertainties related to the retrievals are small compared to the differences between models and retrievals.

8 CONCLUSIONS

42. l. 469f: I suggest to add 'mid-level' jet or similar for clarification for the reader and to distinguish this manuscript from the companion paper RW21.

As already mentioned at the third general comments, we prefer not to add 'mid-level', as the studied jet is the mid-tropospheric signature of the jet stream at 300 hPa which falls deeper in the troposphere with PCMT. We do not consider it as a mid-tropospheric jet because its maximum is not located in the mid troposphere.

43. l. 490: Here only the second ascending group of trajectories is referred to as WCB trajectories, whereas above, all trajectories are refered to as WCB. Please be consistent and adjust throughout the manscuript. See also comment to l. 344.

Trajectories staying at the same level are not considered as WCB trajectories. As already mentioned, the word WCB has been suppressed when considering these trajectories.

44. l. 491ff: I find this discussion very interesting and would appreciate if theses differences between heating in B85 and PCMT are more thoroughly also discussed throughout section 4 in the appropriate places.

As suggested, the discussion has been moved to the subsection 4.1 conclusion (line 376).

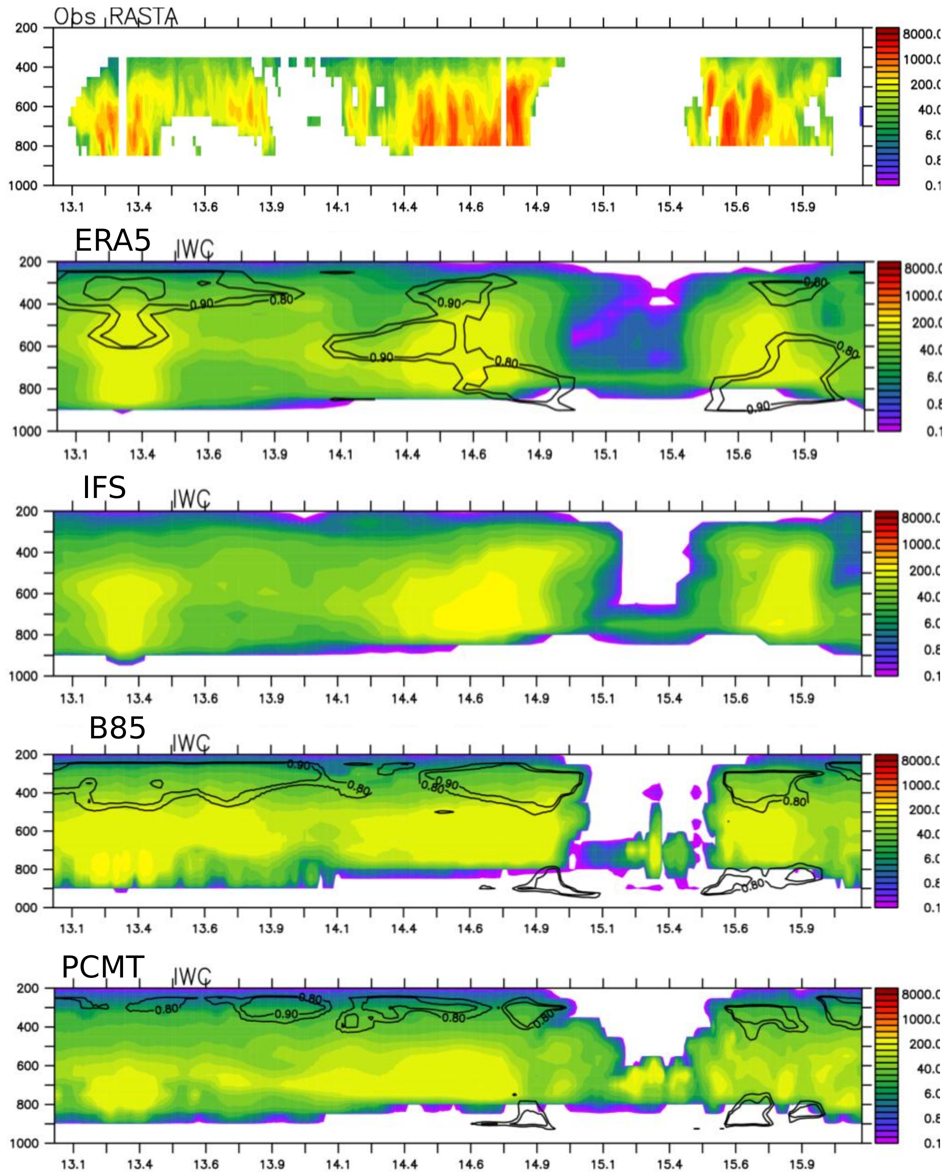

Figure 6. Vertical cross section of IWC (mg/m3) along flight F7 for a) RASTA observations b) ERA5, c) ECMWF-IFS, d) B85 and e) PCMT

45. l. 494ff: The differences between trajectories and PV modfication are clearly summarized here. I would appreciate if section 4 could be rephrased and streamlined to highlight the important differences.

The conclusion of section 4 (from L406 to 411) has been rewritten in order to better highlight this differences : 'On the warm-air side, PCMT trajectories undergo a more rapid PV decrease because they already pass over the heating which is more confined at lower levels and appears sooner than in B85. This leads to a negative PV differences in mid-troposphere in PCMT while this anomaly appears more in upper-level with B85. This induces a PV difference between PCMT and B85 that reinforces the PV gradient at mid-levels and thus the jet in PCMT relative to B85.'

46. I appreciate the comprehensive summary in the conclusions and the schematic in Fig. 12.

Thank you

9 FIGURES

47. Fig. 1: It would be nice to additionally show the location of the jet (e.g., wind or selected PV contours) because the manuscript focus on the jet and PV structure. Is it correct that Fig. 1b does not show the same region as Fig. 1a? I would appreciate if Fig. 1b shows at least the approximate longitude and latitude coordinates. I was additionally wondering why 12 UTC is shown in this figure although the following figures all focus on 15 UTC.

Figure 1 has been modified in consequences, arrows indicating the jet stream location have been added. Fig1a and b have also been changed and have the same projection and area (see Fig. 7 of the present document). The choice of 12 UTC is because the ARPEGE analyses are only available at 00 UTC, 06 UTC, 12 UTC and 18 UTC. As no ARPEGE analysis is available at 15UTC, we choose the 12UTC analysis to get closer to the flight time.

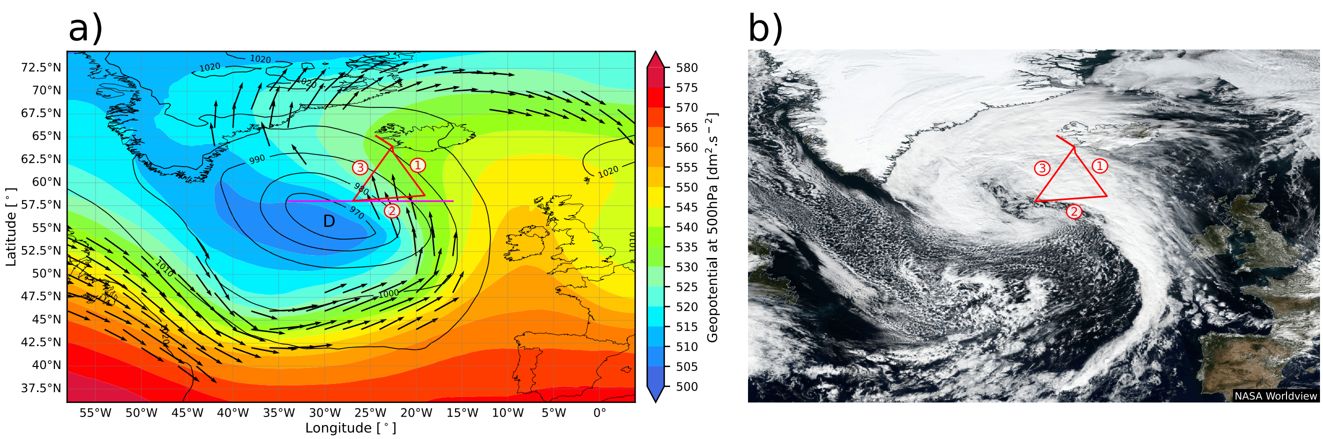

Figure 7. Visualisation of the Stalactite cyclone during its mature stage, the 2nd of October 2016: a) Geopotential at 500 hPa (shading), sea level pressure (black thin contour) at 12 UTC, Falcon flight (red bold line) and vertical cross section at 58° N (magenta bold line) b) visible picture from VIIRS of the Suomi NPP satellite (NASA Worldview) with the Falcon flight in red.

48. Fig. 2: right bracket is missing in caption after 'in Fig. 1'

The bracket has been added.

49. Fig. 3: Please add that the 0 PVU contour is shown by bold contour (if this is correct?).

The formulation is now 'Potential Vorticity (black contours with hatched areas for values superior to 2 PVU and bold contour for 0 PVU)'.

50. Fig. 5: How do the authors define positive and negative 'anomalies' along trajectories? I would find it helpful if in a) and b) at least one PV contour of PCMT and B85 would be included in both panels. This would help the interpretation of the differences shown in shading.

PV anomalies which are either positive or negative are calculated at the flight position, not along trajectories. As trajectories are seeding along the flight, the PV is interpolated at the location of trajectory seedings. Thus, since PV value is known at the intersection between Lagrangian trajectories and flight for the two simulations, we can compute the PV differences between the two simulations along the flight. Trajectories for which ones that differences (PCMT-B85) is positive (higher PV in PCMT than B85, in red colors in Fig5) are represented by black dots. In the opposite, trajectories whose the differences is negative (higher PV in B85 than PCMT, in blue colors in Fig5) are represented with green dots. The other WCB trajectories are represented with grey circles.

As discuss in the second technical corrections, to better explain definition of 'PV anomalies', we refer to 'PV differences' between the two simulations.

As there is a lot of information in the Figure 5 (PV differences in shading, wind field in black contour and trajectories intersection with flight legs represented with different colors dots), we prefer to do not add another field (PV) to do not overprint the plot.

51. Fig. 7a,b: Is the heating vertically averaged? Please clarify in the caption. Note also the typo in K h-1. Please also note comment to l. 344 about WCB trajectories. Please re-consider if the quasi-isentropic trajectories should be refered to as WCB trajectories.

The heating is vertically averaged between 300 and 900hPa. The information has been added in the caption. Thank you to point out the typo. As already mentioned in the answer of the l. 344 remark, we refer trajectories instead of WCB trajectories. Consequently, the caption is now : 'Pressure (shadings) along trajectories reaching the positive PV difference in leg 2 for a) B85 and b) PCMT respectively, vertically averaged heating between 300 and 900hPa (black contours ; units : 0.4 K h^{-1}) and trajectories position (blue crosses) at 03 UTC, 2 October; Vertical cross sections of heating averaged between 45° N and 49° N (black contours; second method of computation), potential temperature (red contours) and trajectories positions (blue crosses) at 03 UTC 2 October, for c) B85 and d) PCMT.'

52. Fig. 11: Please clarify the caption.

The caption is now : '(a) Wind speed observations from RASTA and aircraft at full resolution; Wind speed anomaly with respect to observations interpolated at model resolution (shadings) and wind speed (black contour) for B85 (b ; Difference B85-Obs.), PCMT (c ; Difference PCMT-Obs.)'

Technical corrections

1. general comment: I would suggest to avoid rather technical terms in the manuscript, such as 'plot', 'vertical point of view', '3D picture'.

Other formulation such as 'meridional section', 'horizontal section' or 'zonal section' have been preferred.

2. Please try to be consistent with the wording. I think starting from l. 301 the authors refer to the differences between the simulations as 'anomalies'.

We agree the formulation is not well chosen and we change all the 'anomalies' to 'differences'.

3. l. 37: I'd suggest to rephrase to either 'NAWDEX's objective' or the 'objective of NAWDEX'

We follow your second suggestion.

4. l. 92: 'number of respects'. I think you mean 'number of aspects'

Yes of course. The modification has been made.

5. l. 115: 'an horizontal resolution': Change to 'a horizontal resolution'. See also l. 154.

The correction has been done at the two lines.

6. l. 141: 'Figures 1a and b show the position of the flight according to the Stalactite cyclone': Please re-phrase this sentence, e.g., 'Figure 1 shows the position of the flight in relation to the cyclone'

We change to 'Figure 1 shows the position of the flight in relation to the Stalactite cyclone'. To avoid repetition in the following sentence, the formulation 'Stalactite cyclone' is kept for the first sentence, but in the second one, only a 'cyclone' is used : 'Figure 1 shows the position of the flight in relation to the Stalactite cyclone. The aircraft took off at Keflavik, went south, realized a clockwise loop triangular in shape to the northeast of the cyclone.'

7. l. 269: I would replace ’modelized’ by ’modeled’.
   We replace by ’represented by’.
8. Table 1: Typo in unit for wind speed.
   The dot has been removed.
9. Fig. 3: I’d suggest to change ’without any deep convection representation’ to ’explicit deep convection’.
   The correction has been made in the caption.

Reply to referee 2

General comments

This paper is effectively the second part of a previously submitted and published paper, which I also reviewed. As the latter this paper consists of a comparison between three simulations (among a whole ensemble of simulations) of a mid-latitude storm using two different convection parametrisations and no parametrisation at all. The focus in this new paper is specifically on the jet stream. The analysis consists of trajectory analysis combined with the analysis of observations collected during one flight of the NAWDEX field campaign. While no conclusions are drawn as to e.g. which parametrisation yields more accurate results, the work is useful to understand the variety of responses expected from different parametrisations on very specific details in a simulation (the jet stream in this case) rather than on statistical quantities such as forecast skill. As the previous paper, this article is definitely in scope for Weather and Climate Dynamics. It is also well-structured and well-written. I do not have any specific comments, but I do include a set of technical comments that can be considered by the authors to hopefully improve the manuscript. Other than this, I can fully recommend the paper for publication in WCD.

We would like to thank you for your review of our second paper and your improving and interesting comments of the manuscript. Thank you also for all noticed typos. They have been taken into account. A point by points answer to your comments is listed below.

Technical comments

- L15: Remove the acronym WCB from the abstract as it's not used there.

  That acronym has been removed from the abstract.

- L26: Change 'skills' to 'skill'.

  It has been corrected.

- L35: Change 'This' to 'It'.

  The correction has been made.

- L36: Change 'Downstream and impact' to 'Downstream impact'.

  The word 'and' has been removed.

- L110-111: Change 'its Ensemble Prediction System associated' to 'its associated Ensemble Prediction System'.

  The word 'associated' has been moved.

- L114: Change 'Models' to 'Model'.

  The 's' has been removed.

- L132: I don't see the need for the word 'hereafter'. Can it be removed?

  That word 'hereafter' is effectively not needed and has been removed in consequence.

- L159-161: The text in these three lines is slightly repetitive. I believe it could be rewritten in a clearer way. If left as is, change 'which it is close' to 'which is close', in L161.

  That paragraph has been rephrased as following : 'To better compare observations with model outputs, in-situ and RALI measurement have been averaged over intervals of 180 s as the Falcon 20 (with its mean speed of about 200 m s^{-1}) travels, in that time, a distance of 36 km corresponding approximately to the horizontal grid spacing of the model outputs.'

– L201: I suggest changing ‘made available’ to ‘available’.

The word ’made’ has been removed.

– L201-202: Why was this preferred instead of computing the numerical derivatives in the native model grid? Is the advantage of the high resolution grid not lost by the smoothing associated with the interpolation?

This has been preferred for practical reasons as the native model grid is a stretched Gaussian reduced grid. With such grid, the calculation of numerical derivatives is tricky. However, by interpolating on 0.5° regular grid, the calculation is easier. However, it is true, as mentioned by the referee, it has the disadvantage to lose the high resolution information.

– L210: I suggest changing from ‘case to case’ to ‘dataset to dataset’.

As suggested, the modification has been done.

– L220-223: How sensitive are these results to the exact location of the vertical cross-section? This is, what is the length scale of the features discussed here (for example, the negative PV region or the tropopause fold). Even though Fig. 3 shows horizontal sections, the question remains. For example, are the high PV regions joined in the vertical in all simulations or not?

The higher PV values slightly to the west of the cold front in NoConv and less activated scheme are not necessarily homogeneously distributed all along the cold front as shown in Figures 3 and S2. They form some patches of high PV values. Therefore not all cross sections along the cold front show such high PV values for the less activated schemes, but most of them do. Also, the formation of such patches of high PV values just behind the cold front is systematic of the less activated scheme whatever the initial conditions (see figure 1). This reflects the release of convective instability occurring at resolved scales within localized cells with few degrees extent in longitude and latitude along the front as described in the first paper (Rivière et al., 2021).

– L227: The cold front is indeed noticeable by the change in mslp contour curvature, but perhaps a more direct indication of the front would be worthwhile (for example, low-level moist potential temperature).

As suggested, to better visualize the position of the cold front, we change the mean sea level pressure field to the potential temperature, averaged between 750 and 850hPa to get a less noisy field (see figure 8).

– Figure 2: There is no need to include the colour bar twice.

The upper colour bar has been removed.

– L237-238: I suggest joining these two paragraphs for better text flow.

The line break has been deleted.

– L253-254: I suggest joining these two paragraphs for better text flow.

The two paragraphs has been concatenated.

– Figure 3a: The red sea level contours are missing in this frame.

It has been corrected. But, as suggested in the comment about on line 227, to better visualize the cold front, the red sea level contours has been changed to the potential temperature, averaged between 750 and 850hPa (see figure 9).

– L269: Change ‘modelized’ to ‘modelled’, or ‘analysed through’ or ‘represented by’.

We adopt your suggested phrasing : ’represented by’.

– L291: Can the strong heating of 2 K per hour be attributed to a particular parametrisation?

That strong heating is due to the large-scale cloud scheme. On Figure S4, the large-scale heating coming from the cloud parameterization is the preponderant one. Heating from the other terms (parameterized heating from deep convection scheme, turbulence and radiation) are quite small, or more localized, compared to the one coming from the large-scale cloud scheme.

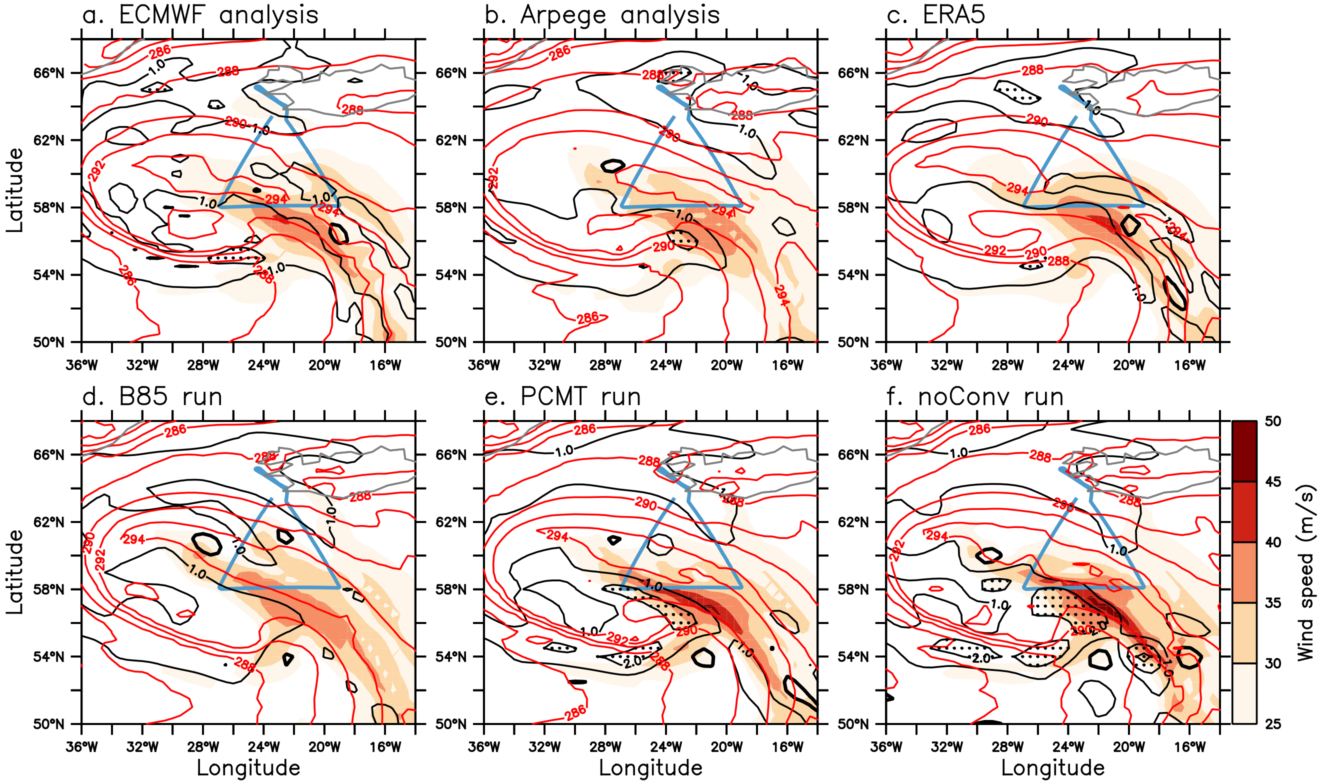

Figure 8. Wind (shadings) and Potential Vorticity (black contours with hatched areas for values superior to 2 PVU and bold contour for 0 PVU) at 600 hPa with potential temperature averaged between 750 and 850hPa (red contours) at 15 UTC, 2 October 2016 for a) IFS analysis, b) ARPEGE analysis, c) ERA reanalysis, d) simulation with B85, e) simulation with PCMT and f) simulation with explicit deep convection. The Flight F7 of the SAFIRE Falcon appears in blue line.

- L291: I hope I'm understanding correctly, but I would call this 'below the freezing level'. Positive or negative only apply to the Celsius temperature scale.

  You correctly understand. We mean, as you suggest, 'below the freezing point'. As all temperatures are in Kelvin in this paper, we modify 'in the positive temperature area' to 'below the freezing point'.

- L299: Thank you for using the correct name (abscissa) instead of x-axis! Nothing to change here.

  Thank you.

- L300: Number of seeds? Why would you have an increasing number of seeds between leg 3 and leg 4? I'm not sure I understand this. Can you expand on the explanation of the trajectory index meaning, or perhaps refer back to the methods section? Furthermore, why do you need to plot against a trajectory index and not against some more physically meaningful quantity such as distance along flight or geographical position?

  In the subsection 2.4.2, trajectories are seeded on a grid with 63 points on the vertical axis and 84 points on the horizontal one. On the vertical axis, trajectories are seeded between 975 and 200 hPa every 12.5 hPa.

  On the horizontal, seeds follow the flight travel. Firstly, 32 seeding points are defined for the first leg, from 22.5°W 63.3°N to 19°W 58.6°N. For the second leg, 20 seeding point are defined between 19°W 58.6°N (last position of leg1)

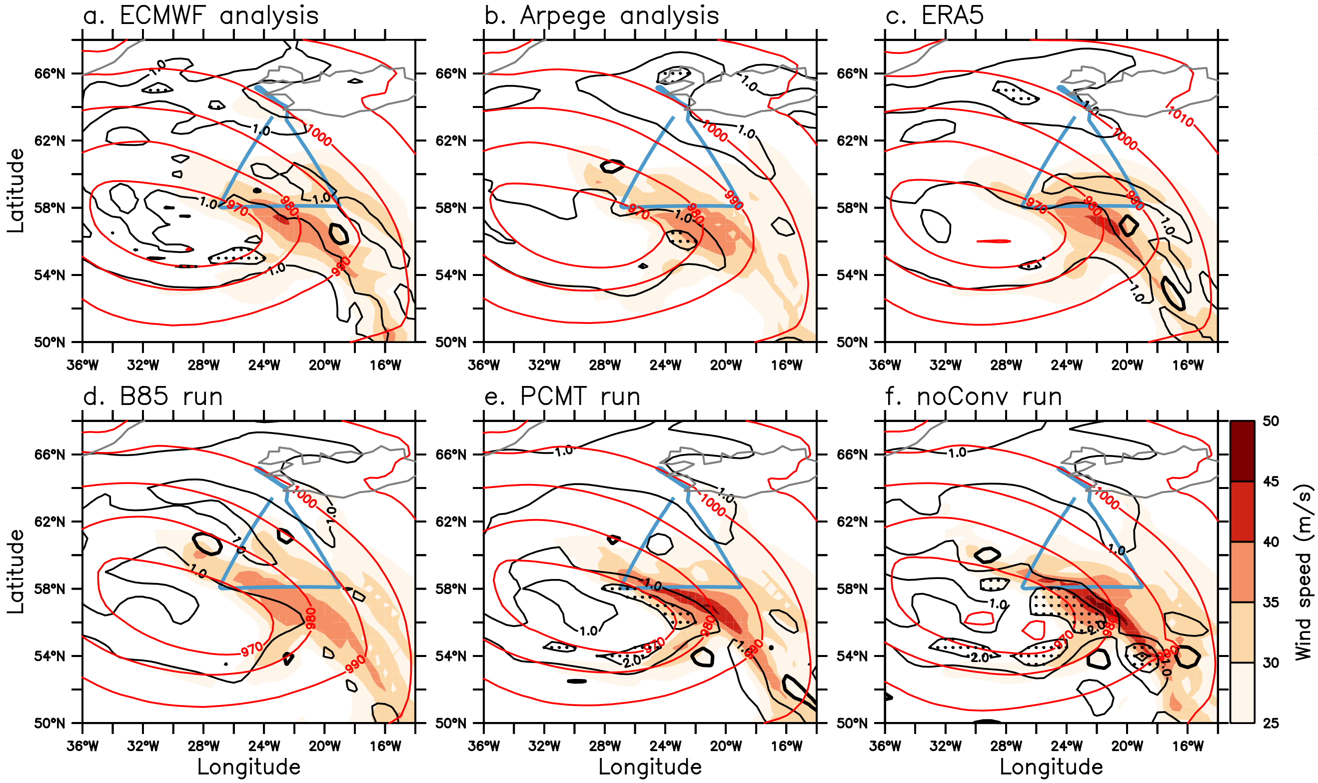

Figure 9. Wind (shadings) and Potential Vorticity (black contours with hatched areas for values superior to 2 PVU and bold contour for 0 PVU) at 600 hPa with potential temperature averaged between 750 and 850hPa (red contours) at 15 UTC, 2 October 2016 for a) IFS analysis, b) ARPEGE analysis, c) ERA reanalysis, d) simulation with B85, e) simulation with PCMT and f) simulation with explicit deep convection. The Flight F7 of the SAFIRE Falcon appears in blue line.

to 26.9°W 58°N. For the third leg, 32 seeding point are defined between 26.9°W 58°N (last position of leg2) to 22.5°W 63.3°N (first position of leg1). Thus, this lead to 84 seeding points approximately separated by 0.3° in longitude and latitude, and ordered in the same way than the flight. They define the trajectory index.

As these seeding points does not fit perfectly the flight position, we do not consider flight position as abscissa. Time cannot be used either in the abscissa as all trajectories from a same leg are initialized at the same time. The geographical position has been avoided as trajectory seeds have different latitude and longitude positions and the use of such position may be too heavy in the figures. In opposition, this trajectory index, as trajectories are seeded in the order of the flight, can be associated to the time along the flight.

Thus, in the subsection 2.4.2, we add some references to the trajectory index : 'For each flight leg, the trajectories are seeded on a vertical regular grid spacing of 12.5 hPa from 975 hPa to 200 hPa (63 seeding points on the vertical axis) and on a horizontal grid spacing of about 0.3° in longitude and latitude (84 seeding points on the horizontal axis, whose index, ordered according to flight travel, defines the trajectory index)'.

– L301: Is 'anomaly' the right word here (and in other parts of the text)? I suppose these are anomalies with respect to the B85 simulation, but to me a more precise word would be 'difference'.

The word 'anomaly' is not well chosen, but it effectively corresponds to anomalies with respect to B85. As you suggest, we change for 'differences'.

- L323: It is slightly confusing to say that the green dots are on both sides of the jet stream. They are in the plot but geographically they are on the same side. It's only that the flight legs are unfolded in the figure. Am I interpreting this correctly?

  Your interpretation is correct. The flight legs are unfolded in the figure. To avoid such confusion, we change the 'both sides of the jet stream' to 'on the warm side of the jet stream'.

- L382: The latest time shown in Figs. 5a,b is 15 UTC 2 October, but there are several references in the text to later times (e.g. 1545 UTC in this line).

  References to later times are mistakes. References to 16 UTC are, in reality, references to 15 UTC. However the '1545 UTC' at L382 corresponds to the true trajectory seed time, which does not appear in the graph. To avoid such confusion, the formulation has been replaced by only '1500 UTC on 2 October'.

- L334: The fluctuation at 11 UTC for leg 2 was actually quite large and indicates a PV-decreasing process of the same magnitude as the process that created it in the first part. Therefore, I'd be slightly hesitant to call it a fluctuation.

  We replace 'may undergo some fluctuations' by 'may be temporarily reduced.

- L340: What does 'quasi-systematically' mean in this context?

  We replace 'quasi-systematically' by 'most of the time'.

- Figure 6: I understand the total should be the time derivative (dPV/dt) of the curves in panel a (PV), in which case between 6 and 10 it should be negative for PCMT! On the other hand, panels c-d do correspond with what I would have expected.

  Between 0600 and 1000 UTC, the PV in PCMT indeed decreases but more importantly between 0900 UTC and 1000 UTC. It is true that the sum of all the tendencies is not negative when there is a slight decrease between 0600 and 0900 UTC but is negative between 0900 UTC and 1000 UTC when there is a strong decrease. As already said, there is no systematic correspondence between the two but the sum of all the tendencies captures the important phases of PV decrease and increase. As already said in our response to referee 1, closing quantitatively such a PV budget is not possible because of the various reasons described in Spreitzer et al. (2019) (see also our response to comment 30 of referee 1). However, we do show that most of the time the sum of all the tendencies gives the right PV evolution.

- L370: Change 'ascent' to 'ascend'.

  The correction has been made.

- L483: Change 'level pressure' to 'pressure level'.

  We switch the order of these two words.

- L496: I suggest changing 'a sooner' to 'an earlier' and the same for L516

  The two modifications have been taken into account.

References

Attinger, R., Spreitzer, E., Boettcher, M., Wernli, H., and Joos, H.: Systematic assessment of the diabatic processes that modify low-level potential vorticity in extratropical cyclones, Weather Clim. Dynam., 2, 1073–1091, https://doi.org/10.5194/wcd-2-1073-2021, 2021.

Binder, H., Boettcher, M., Joos, H., Sprenger, M., and Wernli, H.: Vertical cloud structure of warm conveyor belts – a comparison and evaluation of ERA5 reanalysis, CloudSat and CALIPSO data, Weather and Climate Dynamics, 1, 577–595, https://doi.org/10.5194/wcd-1-577-2020, 2020.

Cazenave, Q., Ceccaldi, M., Delanoë, J., Pelon, J., Groß, S., and Heymsfield, A.: Evolution of DARDAR-CLOUD ice cloud retrievals: new parameters and impacts on the retrieved microphysical properties, Atmospheric Measurement Techniques, 12, 2819–2835, https://doi.org/10.5194/amt-12-2819-2019, 2019.

Chagnon, J., Gray, S. L., and Methven, J.: Diabatic processes modifying potential vorticity in a North Atlantic Cyclone, Quart. J. Roy. Meteor. Soc., 139, 1270–1282, 2013.

Crezee, B., Joos, H., and Wernli, H.: The Microphysical Building Blocks of Low-Level Potential Vorticity Anomalies in an Idealized Extratropical Cyclone, J. Atmos. Sci., 74, 1403–1416, 2017.

Flack, D. L. A., Rivière, G., Musat, I., Roehrig, R., Bony, S., Delanoë, J., Cazenave, Q., and Pelon, J.: Representation by two climate models of the dynamical and diabatic processes involved in the development of an explosively deepening cyclone during NAWDEX, Weather Clim. Dynam., 2, 233–253, https://doi.org/10.5194/wcd-2-233-2021, 2021.

Harvey, B., Methven, J., Sanchez, C., and Schafler, A.: Diabatic generation of negative potential vorticity and its impact on the North Atlantic jet stream, Quart. J. Roy. Meteor. Soc., 146, 1477–1497, 2020.

Hersbach, H., Bell, B., Berrisford, P., Hirahara, S., Horanyi, A., Munoz-Sabater, J., Nicolas, J., Peubey, C., Radu, R., Schepers, D., Simmons, A., Soci, C., Abdalla, S., Abellan, X., Balsamo, G., Bechtold, P., Biavati, G., Bidlot, J., Bonavita, M., Chiara, G. D., Dahlgren, P., Dee, D., Diamantakis, M., Dragani, R., Flemming, J., Forbes, R., Fuentes, M., Geer, A., Haimberger, L., Healy, S., Hogan, R. J., Hólm, E., Janisková, M., Keeley, S., Laloyaux, P., Lopez, P., Lupu, C., Radnoti, G., de Rosnay, P., Rozum, I., Vamborg, F., Villaume, S., and Thépaut, J.-N.: The ERA5 global reanalysis, Quart. J. Roy. Meteor. Soc., 146, 1999–2049, https://doi.org/10.1002/qj.3803, 2020.

Heymsfield, A., Krämer, M., Wood, N. B., Gettelman, A., Field, P. R., and Liu, G.: Dependence of the Ice Water Content and Snowfall Rate on Temperature, Globally: Comparison of in Situ Observations, Satellite Active Remote Sensing Retrievals, and Global Climate Model Simulations, Journal of Applied Meteorology and Climatology, 56, 189 – 215, https://doi.org/10.1175/JAMC-D-16-0230.1, 2017.

Joos, H. and Forbes, R. M.: Impact of different IFS microphysics on a warm conveyor belt and the downstream flow evolution, Quart. J. Roy. Meteor. Soc., 142, 2727–2739, https://doi.org/10.1002/qj.2863, 2016.

Joos, H. and Wernli, H.: Influence of microphysical processes on the potential vorticity development in a warm conveyor belt: a case-study with the limited-area model COSMO, Quart. J. Roy. Meteor. Soc., 138, 407–418, https://doi.org/10.1002/qj.934, 2012.

Martinez-Alvarado, O., Joos, H., Chagnon, J., Boettcher, M., Gray, S. L., Plant, R. S., Methven, J., and Wernli, H.: The dichotomous structure of the warm conveyor belt, Quart. J. Roy. Meteor. Soc., 140, 1809–1824, 2014.

Mazoyer, M., Ricard, D., Rivière, G., Delanoë, J., Arbogast, P., Vié, B., Lac, C., Cazenave, Q., and Pelon, J.: Microphysics impacts on the warm conveyor belt and ridge building of the NAWDEX IOP6 cyclone, Mon. Wea. Rev., p. in press, 2021.

Rivière, G., Wimmer, M., Arbogast, P., Piriou, J.-M., Delanoë, J., Labadie, C., Cazenave, Q., and Pelon, J.: The impact of deep convection representation in a global atmospheric model on the warm conveyor belt and jet stream during NAWDEX IOP6, Weather Clim. Dynam., 2021, 1–32, https://doi.org/10.5194/wcd-2021-38, 2021.

Rysman, J.-F., Berthou, S., Claud, C., Drobinski, P., Chaboureau, J.-P., and Delanoë, J.: Potential of microwave observations for the evaluation of rainfall and convection in a regional climate model in the frame of HyMeX and MED-CORDEX, Climate Dyn., 51, 837–855, 2018.

Saffin, L., Gray, S. L., Methven, J., and Williams, K. D.: Processes Maintaining Tropopause Sharpness in Numerical Models, J. Geophys. Res., 122, 9611–9627, 2017.

Schäfler, A., Harvey, B., Methven, J., Doyle, J. D., Rahm, S., Reitebuch, O., Weiler, F., and Witschas, B.: Observation of Jet Stream Winds during NAWDEX and Characterization of Systematic Meteorological Analysis Errors, Mon. Wea. Rev., 148, 2889–2907, 2020.

Spreitzer, E., Attinger, R., Boettcher, M., Forbes, R., Wernli, H., and Joos, H.: Modification of Potential Vorticity near the Tropopause by Nonconservative Processes in the ECMWF Model, J. Atmos. Sci., 76, 1709–1726, 2019.