Response to the reviews of “Identifying the diabatic processes driving the evolution of a sting jet: the case of Storm Ciarán"

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Dear Editor,

We thank the two reviewers for their thorough reviews and for the positive and constructive comments on our manuscript. We believe that addressing these comments has given us the chance to improve our study. Below we give point-to-point responses to each of these reviews and include some of the new/revised figures generated. Our responses are written in blue, with edits to the manuscript shown in red.

1 Reviewer 1

The manuscript focuses on the processes leading to symmetric instability that is often associated with the descent of a high-impact sting jet. While literature so far focussed on the descent, accompanying mesoscale instabilities and the strong winds, this article is the first (of my knowledge) that looks at the diabatic processes in earlier stages in detail. The approach seems promising, however, while they discuss the limitations of the method, I believe that the authors could reduce some of them to make stronger arguments.

Many thanks for the time spent reviewing our work. We are glad to hear that its approach seems promising. Please see below our replies to the specific comments.

Major comments

1. While I appreciate the mention of limitations of the methodology throughout the paper, I do not quite understand, why some of them cannot be eliminated by choosing higher resolutions, both temporal and horizontal. In literature, simulations/trajectories with higher temporal resolutions of around 15 minutes showed an improvement in quality/resolving the SJ (e.g. work by the first author himself: Volonté et al., 2018), so it might benefit the study here as well. Same goes for horizontal resolution. What is the benefit of a near-operational simulation here? Having simulations with higher resolution would most likely strengthen your arguments without having to mention the limitations so often.

Many thanks for giving us the opportunity to better explain our position on the limitations outlined in the paper. We started addressing these matters individually in our reply to the editor posted on 28 January. Here we include and expand the relevant parts of that reply.

(a) **Could running a model simulation with finer spatial resolution help alleviating the issues with the PV budget?**

We indicated in the manuscript that “spatial resolution being not fine enough” could be one of the causes of the large errors in the PV tendencies, due to their role in allowing “small inaccuracies in trajectory locations” that would then lead to large errors in “small-scale, fully three-dimensional environment with steep gradients, as is the case on the cold side of the bent-back warm front”. We apologise for not mentioning in that discussion that a move to finer resolution could actually worsen the errors as it would generate more noise, small-scale structures and even sharper gradients, something that Reviewer 2 also points out. In addition to this, model settings such as the convective parametrisation scheme, developed and tested in operational settings, would not necessarily be suitable for higher resolution. We decided to add the following sentences to the paragraph in the revised manuscript: "Similar issues have recently been described in other works, such as Wimmer et al. (2022) and Oertel et al. (2023). In particular, it is important to stress that in these fast-changing and small-scale environments, finer resolution simulations can show even larger and inaccurate tendencies, caused by localised and short-lived very large values of diabatic heating gradients, as noted by Oertel et al. (2023) when analysing their simulations, with grid spacing ranging from 3 km to 13 km. In such environments the use of higher frequency trajectory input data is markedly beneficial, such as 15-minute vs hourly as in Volonté et al. (2018). However, Oertel et al. (2023) show that in these situations even the use of all-time-steps online trajectories might not guarantee a complete disappearance of the discrepancies between full PV modification and the sum of accumulated PV tendencies."

(b) **Why did we decide to use near-operational model settings?**

Expanding on the point above, we would like to stress that the benefit of having operational setting is that we are using the model in the way it is designed, so that the likelihood of the simulation being consistent with the actual evolution of Storm Ciarán is maximised. As explained in the original manuscript, the consistency of our IFS simulation with the operational Met Office forecasts, analysed in Gray and Volonté (2024) and found to be in agreement with observations in terms of timing and strength of the wind gusts associated with SJ descent, is a positive results for two main reasons:

- it reassures us that SJ dynamics observed in Storm Ciarán is well captured by our simulation and that the processes analysed are plausible;
- it shows that the configuration of the IFS that was operational in 2023 is capable of properly representing SJ activity in an impactful windstorm.

For the reasons just stated, together with those given in part (a) of our reply, we would not be inclined to re-run the IFS simulation at a finer spatial resolution even if we had the chance to do so, as the necessary amount of time and resources needed would not be justified, given the likelihood of associated issues outweighing potential gains.

(c) **Why not using a higher time frequency for the output data of the model simulation?**

On this point we fully agree with both reviewers. As stated in the manuscript, we acknowledge that higher temporal resolution would likely benefit the study by reducing spatial inaccuracies in trajectory calculations (although likely not removing them completely, as specified in the manuscript addition mentioned in part (a) of this reply) and allowing to gain a better picture of the rapidly changing processes occurring along and around the trajectories. As Reviewer 1 notes, the lead author had already shown the benefits of increasing time resolution when analysing SJ windstorms, and this is now mentioned in the manuscript. Unfortunately, increasing time resolution was not possible while preparing the original manuscript and is still not possible now. This is due to the nature of the funding for this work and the technical settings available to us. Main issues revolve around the increase in the volume of the data that would be produced and the time and effort that would be required to make the new settings work, both in terms of generating sub-hourly IFS output data and of computing trajectories with Lagranto in sub-hourly mode from this particular output.

Having replied point-by-point to the questions raised in this comment regarding the limitations of our work, we would also like to point out that in the concluding section of the manuscript we stress the value of the methodology of our study. In particular, we mention its usefulness in identifying processes at play and, where possible, isolating their role and in providing a proof-of-concept work that could serve as basis for future work that could verify and generalise its findings, particularly if taking advantage of increasing technical and computational resources.

2. Why do you only focus on one starting time step? You mention the occurrence of SJ activity before and possibly after the chosen time step with separated negative PV regions. Why not look at one or two more time steps to see if they are comparable? Even without showing them in detail in the manuscript, it would be interesting to know if they show similar results.

Thanks for making this suggestion. We agree that the analysis of negative PV regions identified at different times throughout the cyclone evolution is extremely interesting. We therefore added a new section to the revised manuscript, Section 3.6, in which we illustrate the evolution of four additional trajectory sets started at different times from filaments of negative PV located in the cloud head, just outside the bent-back warm front. The starting points of these additional trajectory sets are shown in the new version of Figure 4, which in this document we show, also as Figure 4, when replying to the specific comment on the number of panels included in it. While we do not include here all the results presented in Section 3.6, in the rest of this reply we summarise the key points of this additional analysis and show two of the three figures generated. Figure 1 in this document shows that three of the four airstreams identified have a similar character to the main SJ analysed in this study, with a low-level ascent-descent pattern associated with acceleration and relative humidity decrease during the descent. The remaining airstream instead behaves more similarly to a warm conveyor belt, ascending to the upper-troposphere while (not shown) moving poleward with respect to the cyclone centre. All these three "SJ-like" airstreams are weaker than the main SJ, both in terms of descent and of acceleration. Only the latest airstream is comparable to the main SJ, with a maximum wind speed around 50 m s^{-1}, consistent with observations and with the UKMO operational simulation analysed in Gray and Volonté (2024). Focusing on the analysis of PV tendencies, Figure 2 highlights the importance of melting in decreasing PV along "SJ-like" airstreams, while the role of sublimation of snow is confined to the main SJ and negligible in these additional airstreams. In general, these results highlight the continuous attempts to generate "SJ-like" airstreams throughout the evolution of the storm, with the onset of SI favoured by melting of snow and ice. However, the most favourable conditions for descent and acceleration, and therefore generation of extreme low-level winds, are only present during a limited time.

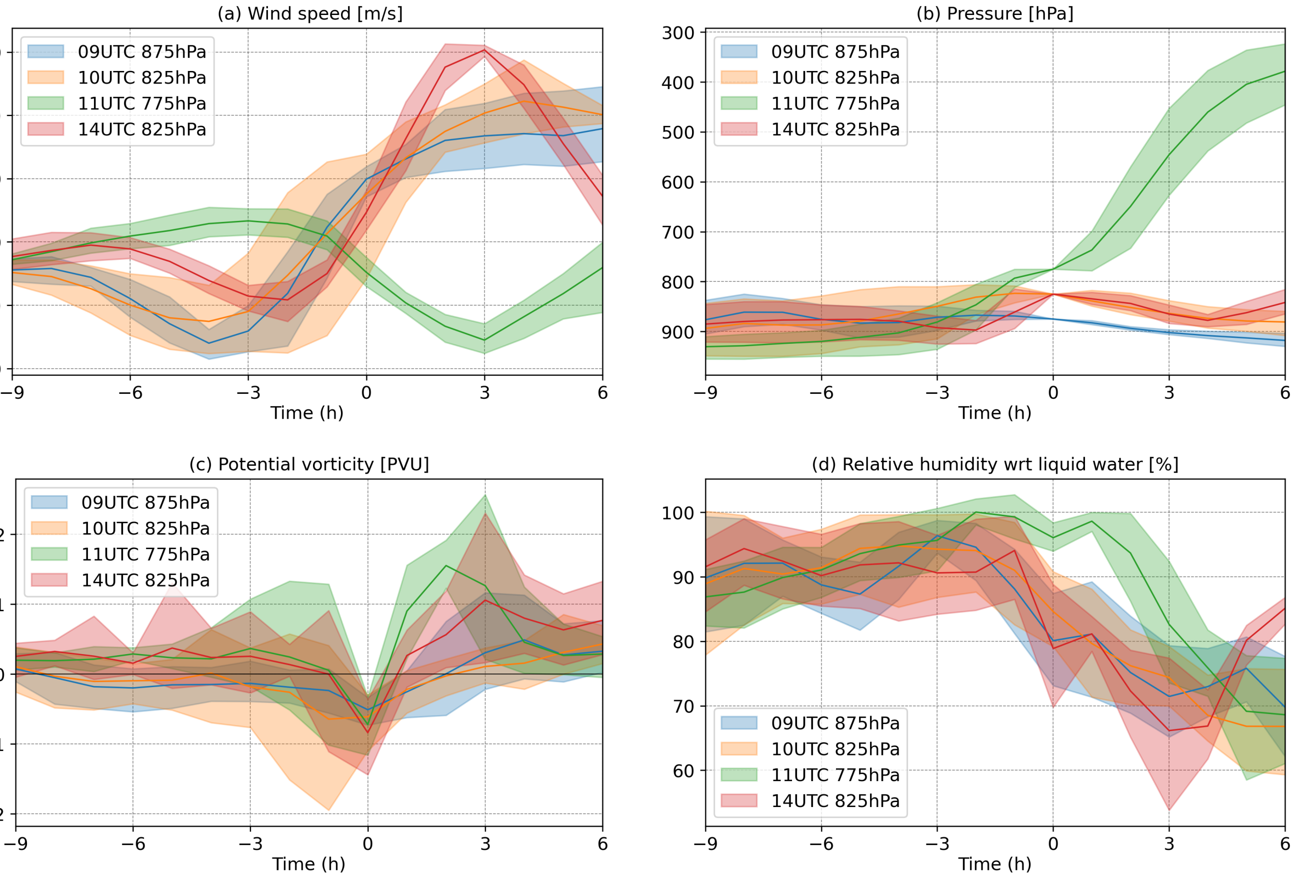


Figure 1. Time evolution of (a) wind speed (m s^{-1}), (b) pressure (hPa), (c) potential vorticity (PVU), and (d) relative humidity with respect to water (%) along the additional trajectory sets (see details in the text and starting points in Figure 4). Coloured solid lines indicate the mean of each field and for each trajectory set. Shading covers the values between the 10th and 90th percentiles. Percentiles are calculated independently at each time, hence a single percentile line can refer to different trajectories at different times.

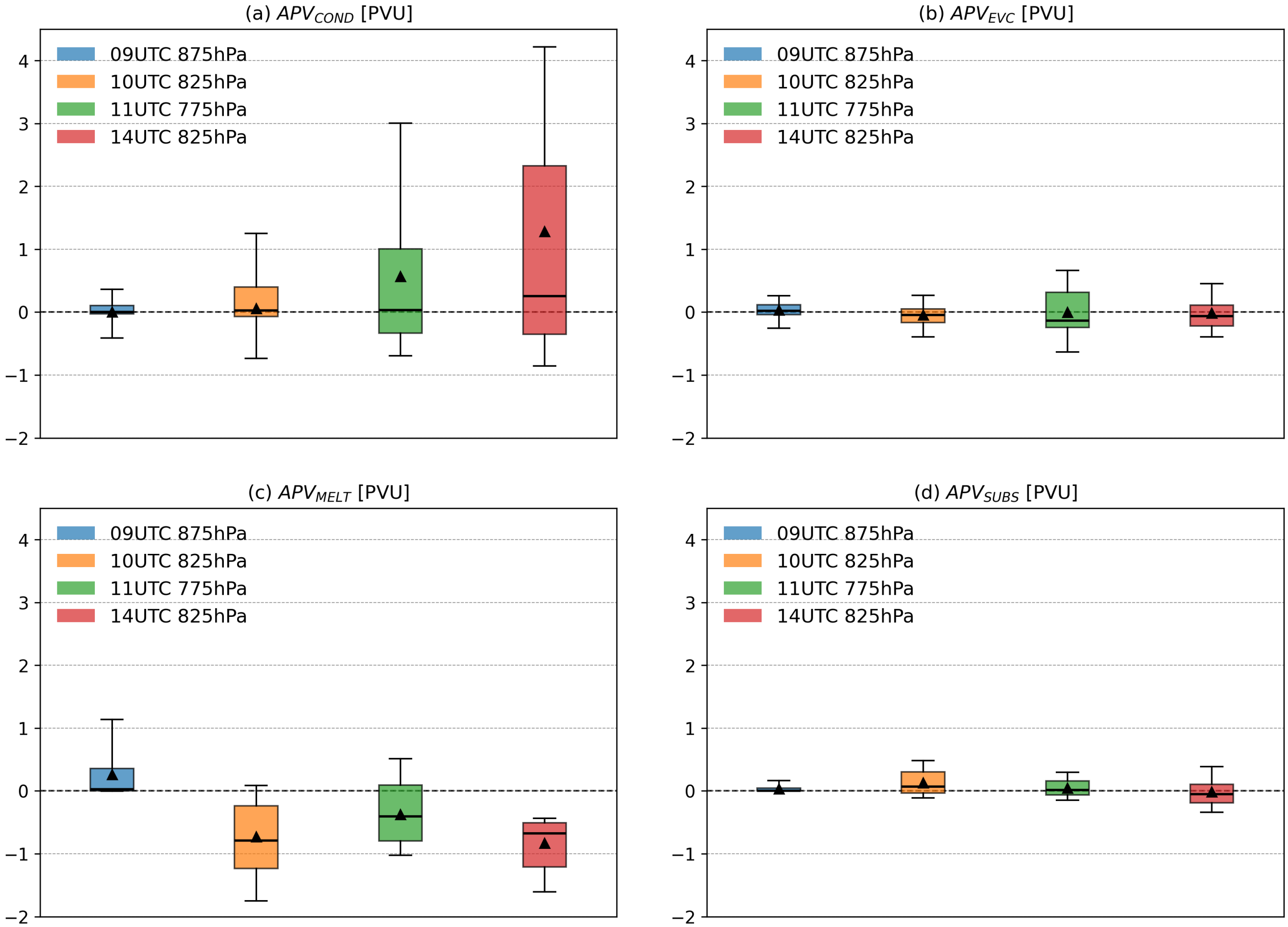


Figure 2. Box-and-whisker plot of the accumulation of individual PV tendencies from (a) condensation of water vapour, (b) evaporation of cloud water, (c) melting of snow and ice and (d) sublimation of snow along the additional trajectory sets. The accumulations are calculated over the 3 hours before the time of minimum PV, i.e., the time in the legend (apart from set "10UTC 825hPa", for which is 09UTC, see related profile in Figure 1b). Each box covers the interquartile range of the trajectory distribution, while whiskers extend to the 5^{th} and 95^{th} percentiles. Lines and triangles indicate median and mean values, respectively.

Minor comments

Abstract

- l.5 I would appreciate adding the month or at least the year of the storm

  Done, thanks.

Introduction

- l. 32ff.: Thinking of their (adapted) schematic, Eisenstein et al. (2022) would be a good reference here as well as they focus on all these different causes of wind gusts and also include convective gusts.

  Thanks. We now specify in the manuscript that "[S]evere gusts can also be associated with cold-frontal convection, as pointed out by Eisenstein et al. (2022)".

- l. 40-53: While it is nice to have recent examples, this can be shortened.

  The description of these examples has now been shortened.

- Section 1.2.: For a long time, SJ literature focused mostly on CSI, while the importance of other mesoscale instabilities, especially SI, seems to have increased in recent years. Please add some context on the timing of occurrence of different kinds of mesoscale instability and why you focus on SI here.

  At the end of the first paragraph of Section 1.2 we had already explained that "[A]s surfaces of θ_e^* are more sloped than those of θ, CSI is more common than SI" and that CSI "requires saturation to be released, while SI does not", and can be released as soon as it forms. We have now added the following: "In recent years consensus grew on the presence of SI in intense SJ cases, as explained in the following paragraph. We therefore focus our analysis on SI, and on regions of negative PV, rather than on its moist and conditional counterpart."

- Mentioning frontolysis and direct frontal circulation later on, you might consider citing Schultz and Sienkiewicz (2013) and their schematic.

  A mention of Schultz et al. (2013) has been added here.

Data and method

- Section 2.1.: What is the motivation to choose a model setup close to the forecast? While I see some benefits, I feel like some mentioned limitations could be better handled using a different setup, i.e. higher temporal resolution for the computation of trajectories as you state this as a limitation but also horizontal resolution (see major comment 1).

  Please see point (b) of our reply to major comment 1.

Results

- l. 224: Why did you choose such a high wind speed threshold? I am wondering if more trajectories might show less noise as 51 seems quite low.

  The wind speed threshold of 52 m s^{-1} (53 m s^{-1} in the revised manuscript) is chosen to identify trajectories that are part of the SJ, therefore associated with the generation of the strongest winds in the frontal-fracture region. We have now tested different wind speed thresholds and shown the corresponding box-and-whiskers plots (analogous to Figure 6 in the manuscript), presented in Figure 3 of this document (not included in the revised manuscript). The panels show that decreasing the wind speed threshold does indeed increase the number of trajectories, but this is not accompanied by a decrease in APV variability. Instead, the discrepancy between ΔPV and APV$_{\text{all}}$ becomes larger, with the mean and median values of the latter not even being negative when the wind speed threshold is lowered to 50 m s^{-1}. This is because in this case, more trajectories around the SJ core and at its edges, undergoing a variety of different evolutions, are included in the trajectory set. To make a compromise on improving the agreement between ΔPV and APV$_{\text{all}}$ and retaining a representative number of trajectories, we decided to choose 53 m s^{-1} as the threshold, therefore retaining 18 trajectories. We explain this choice in the text, with the relevant paragraph in Section 3.1.2 now reading as the following:

"[...] Trajectories are retained if meeting the following criteria, designed so that they are all part of the SJ core:

- wind speed exceeding 53 m/s at release time;
- pressure increase (i.e., descent) exceeding 50hPa in the three hours before the release time;
- negative PV four hours before the release time.

Hence, while 10816 trajectories are initially calculated, the number of those that are retained decreases to only 18 as those three criteria are applied in succession. The starting points of these trajectories are displayed in both panels of Figure 1. The limited number of trajectories retained is a consequence of restricting the trajectory set to the core of the SJ, characterised by the onset of SI, followed by descent and by the generation of the highest low-level wind speed present in our IFS simulation of Storm Ciarán. This strict selection process is made necessary by the nature of the fast-evolving regions the SJ travels in, characterised by tight curvature, steep gradients and large process variability. As explained in detail in Sections 3.3- 3.5, allowing more trajectories to be part of the retained set, including those at the edges of the SJ, would result in larger variability in their evolution and ultimately larger noise in the results analysed."

- Figure 4: I do not see a benefit in showing all time steps as some subfigures seem to be redundant. With the manuscript being quite long and the figure taking up a lot of space, you might consider only showing the important time steps here.

  The number of panels has been reduced from nine to six, removing redundant panels while still showing the time steps necessary to describe the different stages of evolution of the SJ. The new figure is shown here as Figure 4. Four of the retained six panels now show also the starting points of the additional trajectories analysed in Section 3.6, added to the revised manuscript (see reply to Major Comment 2).

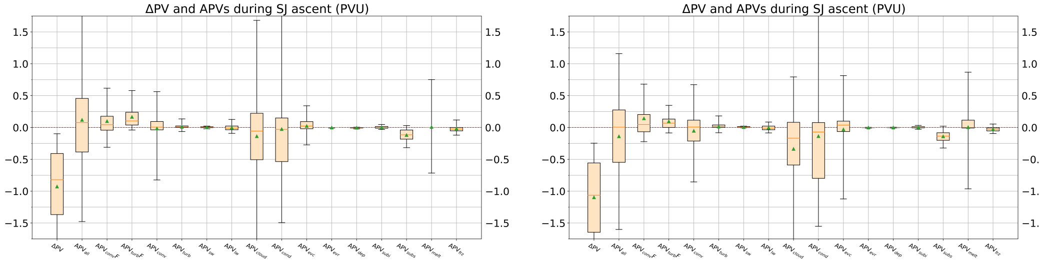
- Figure 4 and discussion: It is really interesting to see separated areas of negative PV possibly associated with SJ activity. It would be nice to see how the other SJ branches behave (see major comment 2)

  Please see our reply to Major Comment 2.

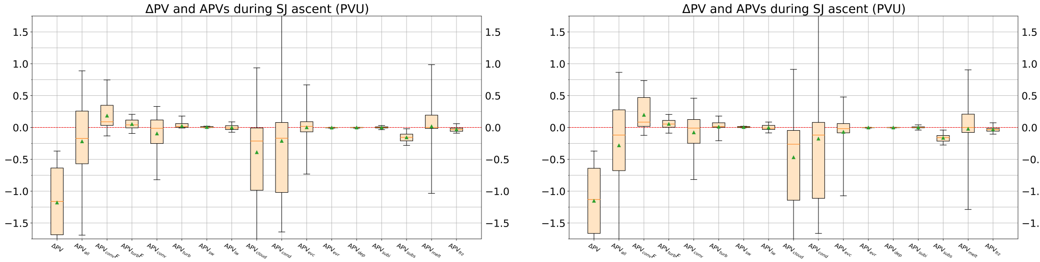
- l. 338 ff: Especially mentioning the two factors here, I come back to major comment 1: Why not a higher temporal and horizontal resolution?

  Please see our reply to Major Comment 1.

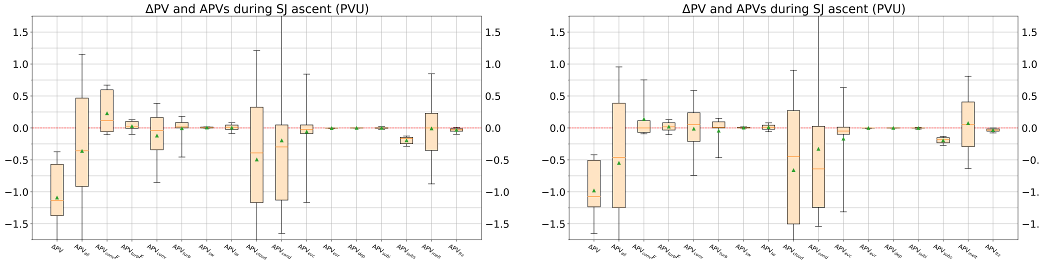
- Figure 6: Please indicate which tendencies belong together and that some are the sum of others


(a) v $>$ 50 m s^{-1} 169 trajectories

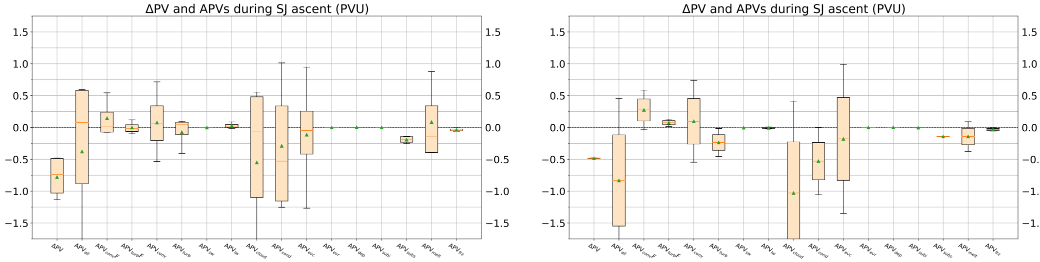
(b) v $>$ 51 m s^{-1} 77 trajectories


(c) v $>$ 52 m s^{-1} 51 trajectories

(d) v $>$ 52.5 m s^{-1} 32 trajectories


(e) v $>$ 53 m s^{-1} 18 trajectories

(f) v $>$ 53.5 m s^{-1} 10 trajectories


(g) v $>$ 54 m s^{-1} 4 trajectories

(h) v $>$ 54.25 m s^{-1} 2 trajectories

Figure 3. Box-and-whisker plots of the total change in PV (ΔPV) and the accumulation of individual PV tendencies (APV_i) along SJ trajectories from t=-6h to t=-4h (09UTC to 11UTC), as in Figure 6 in the manuscript. Each panel shows trajectories selected using a different wind speed threshold, as indicated in its panel title along with the number of trajectories.

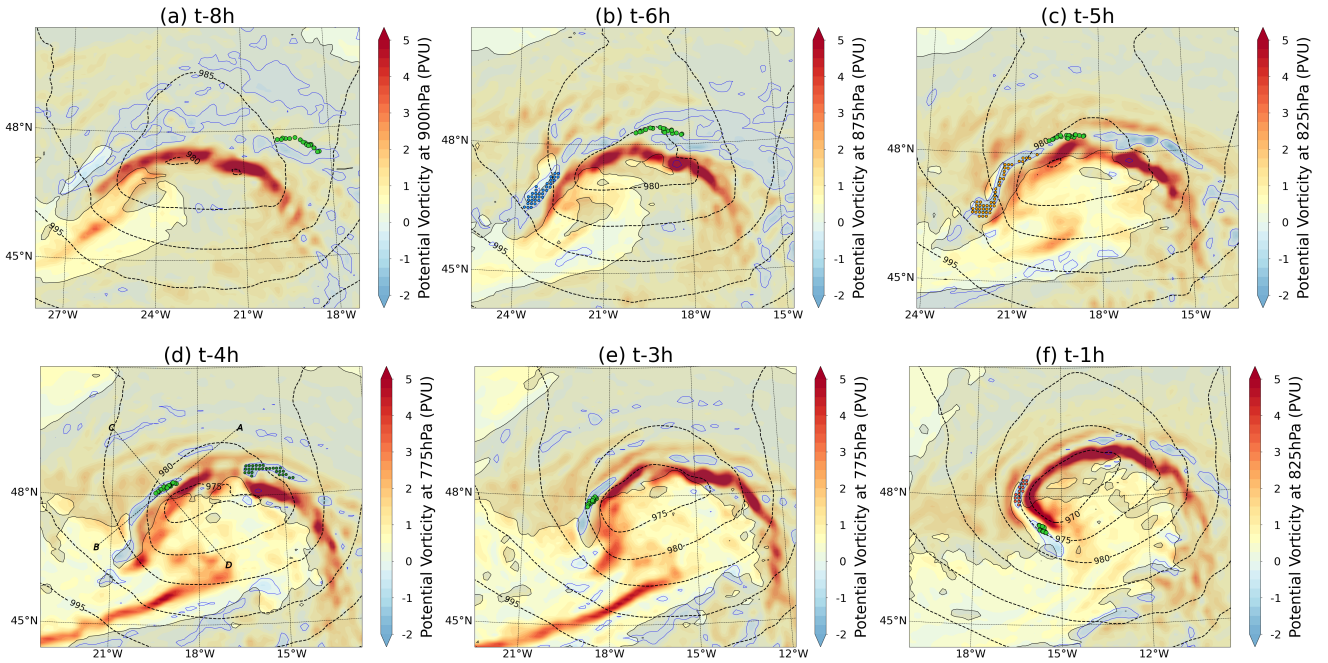


Figure 4. PV (shading) and RH with respect to water (grey shading, > 80%) at the pressure levels indicated and mean sea level pressure (dashed black contours) at the times indicated (hours relative to the trajectory release time, i.e., 15UTC on 01 November 2023). Green dots show the location of SJ trajectories at the related times, while other coloured dots indicate the starting points of the additional trajectories presented in Section 3.6. In each panel the pressure level chosen is the closest to the mean pressure of the trajectories.

We are now explicitly stressing in the caption of Table 1 that APV$_{cloud}$ includes all cloud processes. Table 1 is referenced in the caption of Figure 6 and in the text describing it in Section 3.3. We made a couple of small change to that text, stressing again that all cloud processes are included in APV$_{cloud}$ and that their relevant box-and-whiskers are displayed to its right-hand side in Figure 6.

- l. 361 “median mean”?

  Thanks for spotting this. Corrected to "median"

- Section 3.3.: How can these results be interpreted so clearly with such large variability?

  We agree that the large variability across trajectories does not allow a fully clear interpretation of the processes at play from the timeseries alone. It is the combined examination of PV changes along the SJ trajectories and in the environment around then, focusing on all three dimensions and using both Lagrangian and Eulerian perspectives that leads to a more complete understanding. Therefore, Sections 3.4 and 3.5 are also essential for the interpretation of the results and none of the three sections should be considered alone. It is the combined analysis of all these results that leads to being able to design the schematic presented in the final discussion and conclusions. We revised the text of these three sections in the manuscript to make sure that this approach is as clear as possible and to invite the reader to treat the figures and text from these sections as all connected with each other.

- Figure 9: It is very difficult to recognise all little details without zooming in a lot. Especially what lays underneath the trajectory points. You could consider showing a smaller area focussing more on the trajectory region or instead of showing single points, encircle the area of trajectory locations.

  We tried to improve visibility by slightly increasing the size of the green dots and at the same time zooming in so that the dots would cover a smaller area. The revised figure is shown in this document as Figure 5.

- Section 3.4.: I am wondering if the location relative to negative/positive areas would look different choosing higher temporal resolution (major comment 1). Please discuss this.

  Unfortunately we cannot give a direct answer to this question, as we do not have the means to test potential changes related a different model resolution (see our reply to Major Comment 1). However, using condensation (the noisiest process) as an example, we can say that while the specific locations of local PV increase/decrease maxima are expected to be highly dependent on the specific instant of examination, and therefore on the time frequency of input data, the general patterns are going to be preserved as they are caused by the presence of a bent-back warm front and of additional frontal bands on the cloud head. Considering instead melting and snow sublimation, the analysis of additional airstreams presented in Section 3.6 of the revised manuscript highlights the contrast between the long-lasting presence of melting along the warm front and the short-lived occurrence of sublimation near the tip of the cloud head. This comparison stresses the variability in terms of temporal and spatial scales across the moist processes considered.

Discussion

- l. 592 and single starting time step for trajectories

  Thanks to your previous comment, additional trajectory start times have been included in the manuscript (see reply on Major Comment 2). We have amended the conclusions to reflect this addition.

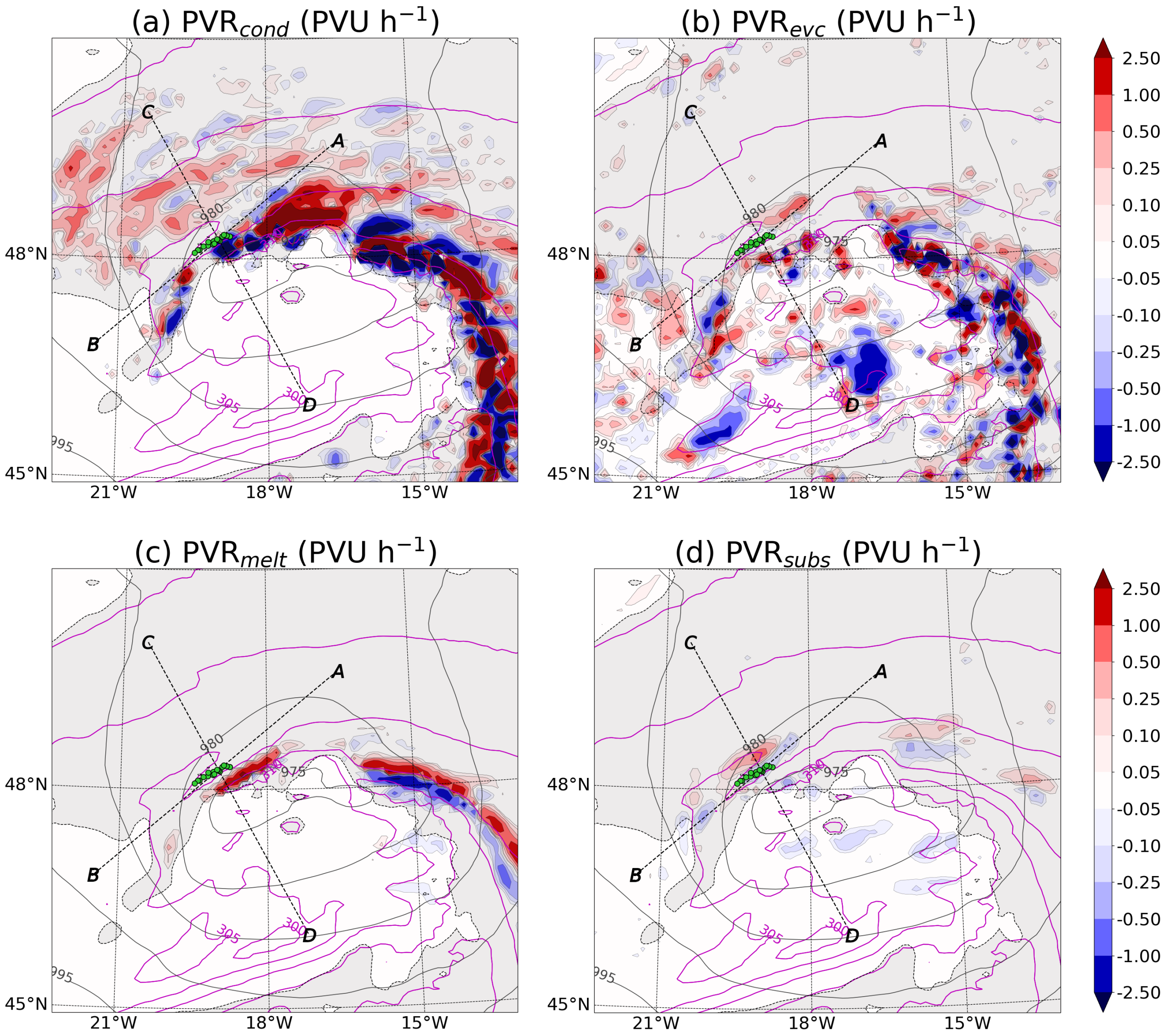


Figure 5. Maps of instantaneous PV tendencies due to (a) condensation of water vapour, (b) evaporation of cloud water, (c) melting of ice and snow, (d) sublimation of snow, indicated by the colour shading, θ_e (magenta contours) and RH with respect to water (grey shading, > 80%) at 775 hPa and mean sea level pressure (dashed black contours) at 11UTC on 01 November. Green dots show the location of trajectories. The pressure level chosen is the closest to the mean pressure of the trajectories.

2 Reviewer 2

The paper discussed the origin of the sting jet that produced extreme winds in intense storm Ciaran using Lagrangian back-trajectories and diabatic tendencies from model parameterizations. It focuses on the ascent period to investigate the processes responsible for the formation of negative potential vorticity, indicative of symmetric instability and which release triggers the descent and acceleration. The results show that condensation of water vapour, followed by evaporation of clouds and melting, is dominant among moist processes but with large variability, while sublimation of snow shows a more consistent pattern but with little contribution. The variability is due the the complex structure of the bent-back front associated with sharp gradients. The paper presents a proof of concept to investigate the contribution of cloud microphysics to sting jet formation, despite limitations in the method due to temporal and spatial resolution that leave large residual in the net change along trajectories.

The paper is interesting, reads well and contributes to the ongoing debate on the origin of sting jets that appears to converge toward the key role of symmetric instability. The diversity of involved physical processes is carefully discussed with help of a combination of Lagrangian and Eulerian diagnostics to disentangle their actual contribution in the complex environment of a bent-back front. The discussion of three-dimensional structures and the contribution of individual moist processes based on numerous subfigures is a bit lengthy but the results are nicely summarized in a schematic. My only concern is about the numerical limitation that is honestly discussed by the authors but questions the validity of the results. General and specific comments are listed below to help improve the paper.

Many thanks for spending time reviewing our work and for this detailed summary. We are glad to hear that it is interesting, enjoyable to read and a potential contribution to the debate on the origin of sting jets. See below our replies to the specific comments.

General comments

1. The openly discussed limitation lies in the spatial and temporal resolution (hourly output on a 0.1° grid) that is not sufficient to adequately follow ascent (and even less descent, as mentioned in the text): why not use higher resolution then? This seems particularly relevant for the temporal resolution whereas higher spatial resolution may result in even sharper gradients and larger variability.

   Please see points (a) and (c) of our reply to Major Comment 1 from Reviewer 1.

2. Somehow related, consistency with Met Office operational forecasts is highlighted a few times as a strength but it is not clear why, as numerical simulations are not restricted by and can explore beyond operational settings.

   Please see point (b) of our reply to Major Comment 1 from Reviewer 1.

3. Horizontal and vertical cross-sections in numerous figures and panels (more than 50 in total!) tend to be repetitive and their number could likely be reduced

   In our reply to the comment on Section 3.3 by Reviewer 1 we discuss the combined role of the various figures in Sections 3.3-3.5 in providing a complete and fully three-dimensional picture of the processes acting during SJ evolution, using both Eulerian and Lagrangian perspectives. Nevertheless, we appreciate that the original manuscript does contain a large number of horizontal maps and vertical sections, and also a considerable number of time series and profiles, and we agree that removing the least important of them could be beneficial to the readers. In addition to this, the new Section 3.6 includes 2 figures, plus one in the appendix. We therefore decided to remove the first two panels of Figure 7, combine Figures 10-12 into a single figure (Figure 10 in the revised manuscript and Figure 6 in this document) and to remove Figures A2 (vertical cross-sections of cloud evaporation tendencies) and A3 ("Lidar plots" of condensation and cloud evaporation tendencies) from the appendix of the original manuscript. At the same time we made several changes to the text in Sections 3.3-3.5, trying to streamline the description of the various results presented in the related figures. We hope that by doing this we will have retained the value of a gradual, multi-perspective and exhaustive analysis of the processes acting along and around the SJ, while making it easier to follow and removing unnecessary annotations.

Specific comments

- l. 5 IFS is not defined yet (and likely not needed)

  Replaced with "numerical weather prediction model", thanks.

- l. 9 why does it matter the simulation is consistent with forecasts?

  Please see part (b) of our reply to Major Comment 1 from Reviewer 1.

- l. 12–16 it is surprising to describe limitations before results

  The description of the limitations has been moved after the results in the revised abstract.

- l. 29, 38 the UK is geographically part of Europe

  To avoid repetitions, we decided to remove the first of the two mentions of "UK and Europe". To improve clarity, we replaced the second mention with "British Isles and continental Europe".

- l. 81 missing word

  Thanks, the sentence now reads: "Volonté et al. (2018) used model simulations of Storm Tini to develop a conceptual model illustrating the primary role of vorticity tilting via slantwise circulations and its close link to the generation of negative PV."

- l. 97 it would be worth citing the subsequent studies rather than the review paper that cites them

  We are now citing a couple of those studies, while retaining the mentioning to the review paper, as it nicely summarises the range of differing results. The revised sentence reads: "Subsequent studies, such as Baker et al. (2014) and Coronel et al. (2016) among others, provided differing results on the importance of latent cooling for the descent and acceleration of SJs, as summarised in Clark and Gray (2018)."

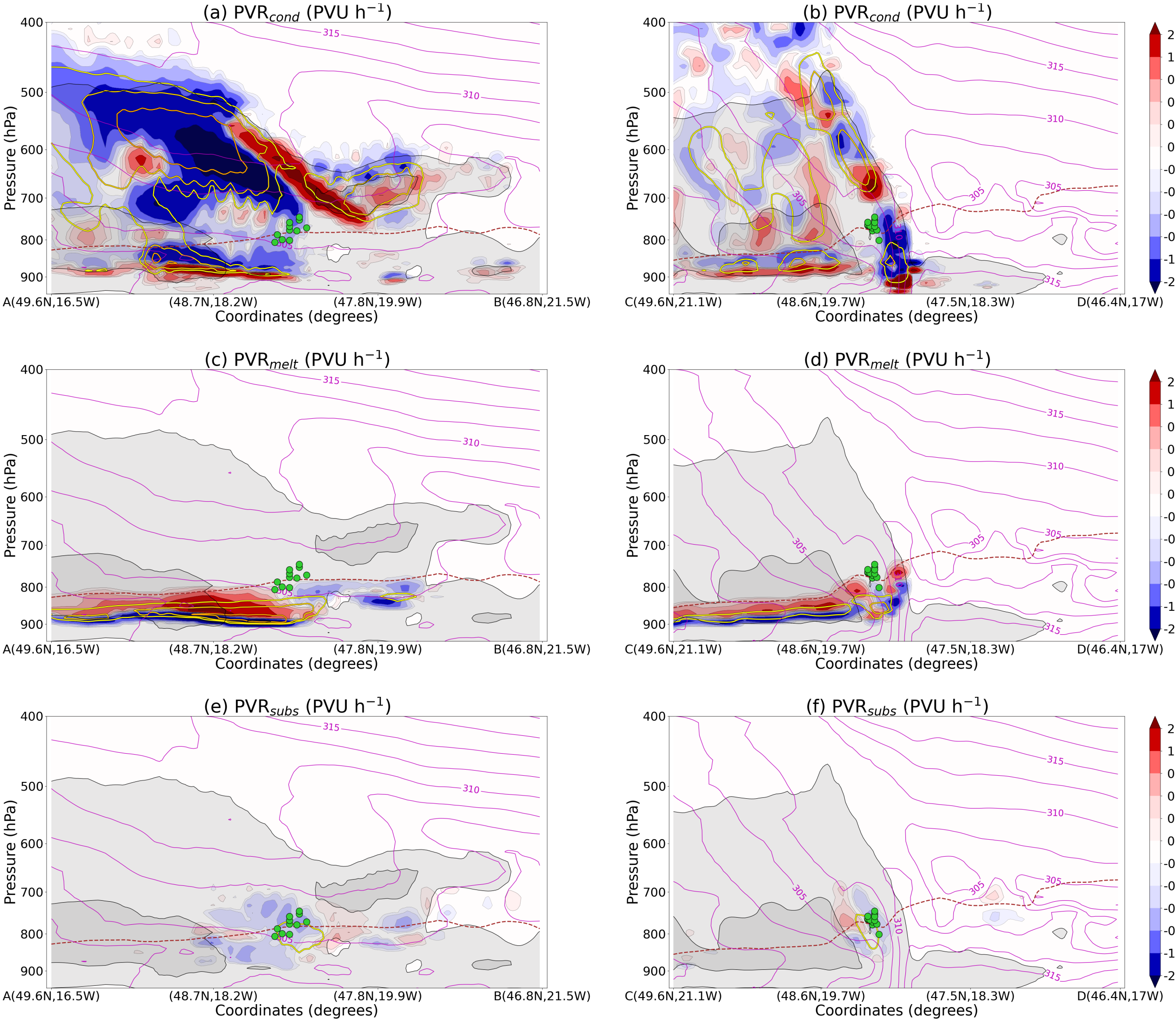


Figure 6. Vertical cross-sections of instantaneous PV tendencies (shading) and θ tendencies (yellow, gold and orange contours, respectively at ±1K, ±2K and ±4K) due to (a,b) condensation of water vapour, (c,d) melting of ice and snow, (e,f) sublimation of snow. Also shown are RH with respect to water (light grey shading when > 80% and darker grey shading when > 98%), θ_e (magenta contours) and freezing level (dashed brown contour) along the (a,c,e) AB and (b,d,f) CD transects shown in Figure 5, referring to 11 UTC on 1 November. Individual trajectories are projected on the section and shown only if less than (a,c,e) 5 km or (b,d,f) 20 km away from it.

- l. 130–138 slightly repetitive; how can latent heating and cooling both lead to increased PV?

  We made a few small changes to the sentences considered, to improve the flow of the text. Regarding the second part of the question, latent heating and cooling can indeed both lead to increased PV, as we try to explain here with a simple example. Considering a simple 1D situation in which heating is applied at a certain height to a stable profile, PV would increase below the heating core and decrease above it (this can be visualised by considering the distortion of θ surfaces associated with the heating). Conversely PV would increase above and decrease below the centre of a cooling region. In a complex three-dimensional environment such as an extratropical cyclones, characterised by numerous sources of heating and cooling in different regions and across a range of spatial and temporal scales, it is thus not surprising to see both heating and cooling processes being associated with PV increase. We are confident that the joint analysis of Q and PV changes provided in Sections 3.3-3.5 will gradually guide the reader through the complexity of the analysis.

- l. 170–173 it should be clarified that APV$_{cloud}$ is decomposed into several terms (Table 1)

  This has been clarified in the revised manuscript, see our reply to the comment on Figure 6 by Reviewer 1.

- l. 182–184 repetition of 1.3

  Thanks, we removed the repetition.

- l. 185, 219 citation formatting

  Thanks, corrected.

- l. 207 twice “at low-levels”

  Thanks, we removed the first of the two "at low-levels".

- l. 192–210 labelling or marking the discussed features on Fig. 1 would be helpful

  Thanks for this suggestion, in the revised manuscript we labelled the main frontal and wind features in Figures 1a and 1b, respectively, and adapted figure caption and text accordingly.

- l. 211–214 it is unclear why consistency with operational forecasts provides confidence, and satellite imagery is not presented here

  Please see part (b) of our reply to Major Comment 1 from Reviewer 1 for a short discussion on why consistency with both operational forecast and satellite imagery provides confidence. The same point was also made in the original manuscript. In the revised version we stress the importance of being consistent with both observations and operational forecasts, and not just one of the two. We would argue that we do not need to include in our manuscript the imagery presented in the cited article.

- l. 220 is the area illustrated somewhere?

  The domain within which trajectory release points are selected was not illustrated in Figure 1 and we considered adding it to the figure. However, we then decided to prepare the revised figure by adding green dots indicating the location of each selected trajectory release point instead of the initial search domain.

- l. 224–226 where do these number come from?

  The criteria have been designed to ensure that the selected trajectories are all part of the SJ core, experiencing the onset of symmetric instability, followed by descent and acceleration leading to the generation of the strongest low-level winds in Ciarán's lifecycle. This is a common technique in the identification of SJs via Lagrangian trajectories, as specified in the revised manuscript, where its rationale has now been clarified. More details are present in our reply to the specific comment on the wind speed threshold by Reviewer 1.

- l. 265 I do not fully get the point: negative PV is a criterion to select trajectories, so why does it prove the SJ is associated with SI?

  Negative PV is a criterion to select trajectories because, as specified in the Section 1.2 of the manuscript, it indicates the presence of SI. The identification process allows us to select trajectories meeting the definition of SJ, as they descend off the tip of the cloud head and accelerate into the frontal-fracture region, generating strong low-level winds. These SJ trajectories are associated with negative PV prior the start of their descent, showing the presence of SI in the evolution of the selected SJ airstream. The relevant sentence has been expanded in the manuscript to better convey the concept explained here.

- Fig. 4 the green dots are barely visible; it would be helpful to add the relative time (t-Xh); also consider reducing the number of panels to e.g. each 2h

  We have now increased the size of the green dots in Figure 4 (shown as Figure 4 also in this document), compatibly with still being able to see the narrow regions of negative PV of which they are part. We also replaced the absolute time indication with the relative time, as suggested in your comment, clarifying this in the caption. We reduced the number of panels from 9 to 6, which in our view are necessary to display the key stages of evolution of the sting jets and, at the same time, indicate the locations of the trajectory release points composing the additional airstreams now included in our analysis (see Major Comment 2 from Reviewer 1).

- l. 284 what is “the conceptual model of SJ evolution confirmed in that study”?

  The conceptual model of SJ evolution "illustrating the primary role of vorticity tilting via slantwise circulations and its close link to the generation of negative PV", presented in Volonté et al. (2018) and confirmed by the idealised simulations in Volonté et al. (2020), is described in Section 1.2. This is now explicitly specified in the revised version of the manuscript.

- l. 298–299 Fig. 2a and 1b

  Done, thanks (with "1" instead of "1b").

- Fig. 5 a horizontal scale would be helpful to estimate the size of the PV bands

  Coordinates of points at 1/3 and 2/3 of the transect have been added to the sections in Figure 5, to help estimating the size of features displayed.

- l. 332 total change in PV

  Thanks, corrected.

- l. 369 it rather indicates that the large PV decrease is captured for a minority of trajectories only; anyway the vertical bounds must be adapted in Fig. 7a to identify this minority

  This sentence is no longer in the manuscript as we decided to remove the first two panels of Figure 7.

- l. 380 which panel?

  Panels b and c in the revised Figure 4, apologies for the oversight.

- l. 397 the them

  Thanks, corrected.

- l. 405–416 Fig. 8 could be used in this discussion; otherwise the Figure should be removed as it is not really discussed

  We are not sure we understand this comment, as the various panels of Figure 8 are discussed throughout this section (lines 377-404 in the original submission). As mentioned when replying to previous comments, we have rewritten several parts of Sections 3.3-3.5 and, among other things, we tried to better clarify the importance of Figure 8.

- l. 422 at at

  Thanks, corrected.

- Fig. 9 as in Fig. 4, the green dots are barely visible

  The size of the green dots in Figs 4 and 9 has been increased, compatibly with the need to avoid covering key features characterising the environment around the SJ.

- l. 453–454 the pattern may be clearer than for other processes but is it relevant if the values are much smaller?

  With an average PV decrease close to -0.25 PVU during SJ ascent (revised trajectory set), to be compared with a total PV change slightly exceeding -1 PVU, the effect of sublimation of snow on the evolution of the SJ is far from irrelevant. Being the only process with a signal clearly larger than noise, the contribution to PV change from sublimation of snow is the most robust among the various cloud processes examined in the manuscript. Having revised the text in Sections 3.3-3.5, we hope to have highlighted its importance better than in the original submission.

- l. 500 dashed brown contour (barely visible)

  Added to the text. Having modified and rearranged the panels into a single figure (Figure 10 in the revised manuscript) that now contains both PVR and Q, we decided that increasing the size of the freezing level dashed contour would have made them too busy, rather than easier to interpret.

- l. 513 missing word

  Thanks for spotting this. That part of the sentence now reads "... in this narrow zone that is located just to the outside of the bent-back front ...".

- Fig. 13 it should be clarified that the plot shows a composite for all trajectories (if I got it correctly)

  Yes, the horizontal locations and pressure levels considered in that figure (Figure 11 in the revised manuscript) are averaged over all trajectories. This has been clarified better in the revised manuscript.

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