Final Author comments on egusphere-2026-2700 with the title:

"A synoptic perspective on the role of the upstream diabatic cooling in modulating the North Pacific storm track"

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04 September 2026

We thank the three reviewers for their constructive feedback, that help us to improve this manuscript. Their comments made us realise that the manuscript in its current form is rather complex which might make following the storyline and interpreting the results difficult for the reader. Therefore, we intend to implement the following changes:

1. We refocus the manuscript on the more specific research question: "How do CAOs featuring strong or weak upstream diabatic cooling affect the storm tracks". Most of the analysis presented in this manuscript target this question already, with the exception of Fig. 4 dedicated to the analysis of Eady Growth Rate (EGR) events, which aims at the more general impact of diabatic cooling on the evolution of baroclinicity at the entrance of the storm track. Consequently, we remove the analysis shown in Fig. 4 (on the EGR peaks) and constrain the manuscript to the comparison of CAOs with large and small BCAR contribution (C_BCAR). While the conclusions substantially remain the same, we believe that this choice allows us to present the result more clearly and coherently, making the paper more easily digestible. As a consequence, we will also rewrite the introduction and conclusions such that they are focused more clearly on this specific research question.
2. We improve the description of the methods by including a more detailed description of the CAO identification and categorisation (Sect 2.1) as well as a clearer description of the days where the number of cyclones are counted (Sect. 2.5).
3. We propose to change the title to: "On the role of upstream diabatic cooling in modulating the North Pacific storm track: A marine cold air outbreak perspective". This will also reflect the specific context of the cold air outbreaks better, in line with the research question addressed by the manuscript.

We would like to add that this refinement of the research question does not imply a change in the main scientific contribution of the paper. CAOs correspond to periods in which continental air, that experienced diabatic cooling over land, moves over the storm track regions: thus, they represent occurrences where diabatic processes happening over the upstream land surface can directly affect downstream storm track activity. We plan to make this link even more explicit in the revised version.

This document presents the reviewers' comments in blue and our responses in black.

Reviewer 1

This is an interesting and relevant study which focuses on the role of upstream diabatic cooling associated with Cold Air Outbreaks over the Japan Sea on the North-Pacific storm tracks. The authors select 98 CAO events over the Japan sea region to investigate the role of diabatic cooling on the weak and strong cooling events. The paper is in general well written, however some further clarification is needed, especially in the motivation of the selection of the CAO events and the motivation behind the definition of weak and strong BCAR events, see detailed comments below.

Thank you for this constructive evaluation of our manuscript. During this review process, we realise that we have not explained some of the methodologies and the reason for their application well enough. Moreover, we now understand that we aim to answer two different research questions within one manuscript. This hinders the interpretation of the results and connecting the presented results. Hence, will refocus this paper on the specific research question of how CAOs featuring strong or weak upstream diabatic cooling affect the storm track. To do so, we will remove the analysis on the EGR peaks shown in Fig. 4 and rewrite the introduction and conclusions such that they are in line with this research question. We will also describe our applied methods in more detail, such that the reader can easily follow our manuscript without having to read Schnyder and Riboldi (2026).

Selection of CAO events (section 2.1): To me it is not completely clear how the CAO events used in this study are selected. For example, the authors write that "CAO events are selected based on particularly high values in the cold air outbreak index.... In the Japan sea." (Lines 85-86). What threshold is used for the selection of the CAO events and over which region exactly? What fraction of total CAO in the region is selected?

Thank you for this comment, which gives us the opportunity to further clarify the rationale behind our CAO event selection. The CAO events are identified following the approach in Schnyder and Riboldi (2026) using a timeseries of box-averaged cold air outbreak index (CAOI) evaluated between 30-60°N and 130-150°W, excluding land and sea ice grid points. The timeseries considers all winter months (December, January, and February) from 1979 to 2023. We identify a CAO event whenever the 90th percentile is exceeded, whereby consecutive timesteps are grouped together to a single event. A minimum event duration of 24h and a minimum separation of 48h between the events are imposed additionally. Given the choice of the 90th percentile, this yields an event set of strong CAOs. We will include a more detailed definition of the CAO identification in the revised Sect. 2.1.

The motivation of the definition of C_BCAR (Equation 1): I struggled a bit to understand the motivation behind the definition of C_BCAR. The equation has subscripts (a) and (b) which are not defined. The authors write that θ_BCAR is defined as "the mean of the lowest θ values along each BCAR trajectory". Is this the mean over the lowest value per trajectory? Why is then this value subtracted from the coldest value over all trajectories, and divided by the difference between the maximum and minimum θ among all CAO events? I understand that the motivation behind this definition might be explained in Schnyder and Riboldi (2026) already, but it would be helpful for the reader if part of this is repeated, especially since this is vital for the interpretation of the results within this manuscript.

We realize that, for conciseness reasons, we did not fully explain the rationale behind the definition of C_BCAR. C_BCAR is a quantity that can be computed for every CAO event individually and is designed as a measure for how strong the influence of upstream diabatic cooling is on a CAO event. For this, we consider (a) the fraction of BCAR trajectories in each CAO event, and (b) how cold the BCAR trajectories in each CAO are compared to other events. Note that the BCAR trajectories are those trajectories that experience the strongest diabatic cooling over the upstream continent (shown in Schnyder and Riboldi 2026). To assess how cold the BCAR trajectories are compared to other CAO events (expressed in b) we therefore normalise the average minimum θ reached by BCAR trajectories in one event with respect to the average minimum θ reached by BCAR trajectories in other CAO events. As a consequence, C_BCAR is close to 1 for CAO events where a large fraction of trajectories are BCAR trajectories, and these trajectories are comparatively cold with respect to other CAO events. On the contrary, in CAO events where C_BCAR is close to 0, only few trajectories are identified as BCAR trajectories, and they are relatively warm. We will include a more comprehensive explanation of this quantity in the manuscript in Sect. 2.1.

Oscillate behaviour of the baroclinicity (Line 200-201 and further): The authors argue that “the composite analysis centered on CAO events suggests that baroclinicity at the entrance of the storm tracks oscillates as a function of upstream radiative cooling and the cyclones within the storm tracks”. I find that hard to see from the composite results (Figure 3-4). Can the authors argue why they see this in the composites? I mainly see a slight weakening of the EGR/baroclinicity, but I find it hard to see when the subsequent replenishment of baroclinicity occurs. Complementary to this, isn’t it expected that this oscillation occurs at longer time scales (e.g. around 5 days, as suggested by e.g. Marchegianni and Spengler, 2023). Have the authors looked at longer time scales to see if the baroclinicity is replenished?

We agree that this statement might sound too strong in the manuscript, and the oscillatory behaviour is not visible from the figure itself. The statement came from the idea that, whenever conditions allow for efficient cooling over land (e.g. due to the arrival of an upper-level wave train, see Fig. 7), then the EGR will increase at the entrance of the storm track (shown in Fig. 3) and the subsequently developing cyclone will then use up the baroclinicity. Baroclinicity is then slowly restored by the heat fluxes (Marcheggiani and Spengler, 2023). Once a new wave train moves over Siberia that allows for particularly efficient cooling again, the chain of processes is set in motion again. We would expect this chain of processes to act on a similar time scale suggested by Marcheggiani and Spengler (2023). However, since we do not explicitly show this oscillation with our composites, we will change our formulation in the text such that we do not talk anymore about an oscillation, but rather of a chain of processes.

Minor comments:

General: The refer to large and small CBAR events, this could imply to me CBAR events with a large and small area. However, if I interpret the text well they want to distinguish between strong and weak events, so I would suggest to explicitly name them so throughout the manuscript.

Thank you for the suggestion. We assume that with CBAR, this comment refers to C_BCAR which describes the BCAR contribution (as described in response to main comment 3). We understand that it would be simpler to refer to these events as stronger or weaker. Although MCAO events with a large BCAR contribution (C_BCAR) are stronger, we think it would not be entirely correct to refer to large C_BCAR events as strong CAOs, since our categorisation is built upon C_BCAR (and not the strength of the CAO directly). Since this terminology was also already introduced in Schnyder and Riboldi (2026), we would prefer to keep this terminology.

Line 39: Isn't the source term not more generic related to diabatic processes, instead of just diabatic cooling?

Indeed, Ambaum and Novak (2014) speak of diabatic processes in general. We likely did not emphasize enough that the focus on diabatic cooling comes from our interest in CAOs and, more in general, on diabatic processes happening over land at the cold side of the jet stream. This would exclude other diabatic processes, such as surface heat fluxes from oceanic currents, that are also important for baroclinicity as already discussed shown by, e.g., Yao and Yang (2016), Hotta and Nakamura (2023), and Marcheggiani and Spengler (2023). Due to Ambaum and Novak's (2024) description of a "gradual build-up", we had implicitly assumed that diabatic cooling would play an important role. We will correct this in the revised manuscript.

Line 86: At which resolution (vertically and horizontally) are those parcels released, and in which region?

To determine the region of the CAO extent (and therefore the region from where to launch the trajectories), we identify a CAO mask for each CAO event using a CAOI threshold of 8K. The threshold of 8K is chosen, since comparatively high SSTs lead to a climatologically high CAOI in the region of interest. The trajectories are then initiated at every grid point in the CAO mask within the planetary boundary layer (PBL) between the local PBL height (diagnosed by ERA5) and 1000hPa in steps of 25hPa at every grid point (every 0.5°). We will include this information in the methods.

Line 95: I think this should be maximum in the subscript instead of minimum?

Thank you for spotting this typo, it should be "maximum ($\theta_{BCAR,warmest}$)". We will make the according change in the manuscript.

Line 132: Don't you want per definition a single SLP maximum per anticyclone mask?

Thank you for this question. What we mean here is that, in some instances, there is more than one anticyclone located over Siberia, or an anticyclone with two distinct centers. In these cases, we only track the anticyclonic center featuring the highest sea-level pressure (SLP). To avoid possible misunderstandings we could propose a rewording, such as “we identify the location of the SLP maximum”

Equation 2/Line 146: The pressure surface P2 is not defined.

Thank you for pointing out this missing information. P2 refers to the upper boundary considered for the vertical integration. We use the 100hPa pressure surface, consistent with previous studies (e.g., Fink et al. 2012, Chen et al. 2024, Christ et al. 2026). We will include the definition and information on the choice of upper boundary in the revised version of the manuscript.

Line 160: What is the motivation for using the 500 km radius instead of the anticyclone mask?

The rationale behind this choice is the fact that anticyclones are less confined than extratropical cyclones for inertial stability arguments (e.g., Holton 2004, sect. 3.2.5) and can encompass much larger areas. Therefore, we choose to consider grid points that are in the relative vicinity of the center by using a radius of 500km. The composites shown in Fig. 6d,e,f indicate consistent contributions (meaning of the same sign) of the terms to the surface pressure within a 5-7° radius around the anticyclone center, making 500km an appropriate choice for the radial averaging.

Line 167: The first box is never mentioned in the main text, so maybe just refer to as a box instead of ‘second box’?

Thank you for the suggestion, we will make the according change in the manuscript.

Line 172: The (beginning of the) sentence could be phrased better, I think you maybe want to say “We additionally count the number….” instead of “The additionally count the number….”?

Thank you for spotting this typo, we will make the according change in the manuscript.

Figure 3: At which interval are the pressure contours plotted?

Thank you for pointing out this missing information. The contours of SLP anomalies are plotted every 2hPa. We will add the information to the figure caption.

Figure 7: What are the intervals for the geopotential height anomalies? Moreover, the regions which are statistically significant are slightly hard to see

The geopotential height anomalies are plotted every 20hPa, only the significant anomalies are shown. As for the significance of the net radiation anomalies, we will increase the dot size to increase visibility in the revised version.

Line 335: I would remove/rephrase 'impressive', since it is rather subjective.

We will remove the word "impressive" and will take this as an opportunity to rewrite these lines more concisely, e.g., by rewriting "Cyclones developing after CAOs do not intensify as rapidly as the primary cyclones, and do not become as deep (Figs.8a,b)."

Line 345: I would remove/rephrase 'unspectacular', since it is rather subjective.

We will also remove the word "unspectacular" and suggest to replace it with "less impactful", as the size of these cyclones are smaller in size and produce less precipitation.

Figure 9: Units are missing at the axis labels, I assume it is lon/lat degrees from the cyclone centre?

Thank you for spotting the missing labels: yes, the axis represent the latitude and longitude degrees from the cyclone center. We will add labels in the revised version.

References:

Chen, T., C. Braun, A. Voigt, and J. G. Pinto: Changes of Intense Extratropical Cyclone Deepening Mechanisms in a Warmer Climate in Idealized Simulations. *J. Climate*, **37**, 4703–4722, https://doi.org/10.1175/JCLI-D-23-0605.1., 2024.

Christ, S., Quinting, J. & Pinto, J.G.: Characteristics of diabatically influenced cyclones with high wind damage potential in Europe. *Quarterly Journal of the Royal Meteorological Society*, 152(776), e70083. Available from: https://doi.org/10.1002/qj.70083, 2026.

Fink, A. H., S. Pohle, J. G. Pinto, and P. Knippertz.: Diagnosing the influence of diabatic processes on the explosive deepening of extratropical cyclones, *Geophys. Res. Lett.*, 39, L07803, doi:10.1029/2012GL051025, 2012.

Holton, J.R.: Introduction to Dynamic Meteorology. 4th Edition, Elsevier, Amsterdam, 535 p., 2024.

Hotta, D., and H. Nakamura: On the Significance of the Sensible Heat Supply from the Ocean in the Maintenance of the Mean Baroclinicity along Storm Tracks. *J. Climate*, **24**, 3377–3401, https://doi.org/10.1175/2010JCLI3910.1, 2011.

Marcheggiani, A. and Spengler, T.: Diabatic effects on the evolution of storm tracks, Weather Clim. Dynam., 4, 927–942, https://doi.org/10.5194/wcd-4-927-2023, 2023.

Schnyder, F. and Riboldi, J.: Linking upstream cold, continental air to the intensity of marine cold-air outbreaks along the western boundary currents of the North Pacific and North Atlantic: A Lagrangian analysis, J. Climate, 39, 2243—-2262, https://doi.org/10.1175/JCLI-D-25-0336.1, 2026.

Yao, Y., Z. Zhong, and X.-Q. Yang: Numerical experiments of the storm track sensitivity to oceanic frontal strength within the Kuroshio/Oyashio Extensions, *J. Geophys. Res. Atmos.*, 121, 2888–2900, doi:10.1002/2015JD024381, 2016.

Reviewer 2: Akira Kuwano-Yoshida

General comments

This paper investigates the impact of radiative cooling associated with East Asian cold air outbreaks (CAOs) on the modulation of the Northwest Pacific storm track using reanalysis data. Although the research topic is interesting, improvements are necessary regarding the definitions of terms, analytical methods, and physical interpretations. In addition, there are numerous errors in the figures, tables, captions, and figure numbering, which hinder a proper evaluation of the manuscript. Therefore, appropriate revisions are required.

Thank you for this constructive evaluation of our manuscript. We realise, that we have not explained some of the methodologies and the reason for their application well enough. Moreover, we now understand that we aim to answer two different research questions within one manuscript. This hinders the interpretation of the results and connecting the presented results. We take this feedback to heart and will refocus this paper on the specific research question of how CAOs featuring strong or weak upstream diabatic cooling affect the storm track. As a consequence, we will remove the analysis on the EGR peaks shown in Fig. 4. Furthermore, we will improve the description of the applied methodologies and correct any mistakes that were made in the figure captions and labels.

Specific comments

Page 3, Line 82: The definition of "The Japan Sea" is problematic. The CAO mask adapted from Schnyder and Riboldi (2026) refers to a domain encompassing the Sea of Japan, the East China Sea, and the Northwest Pacific as "the Japan Sea." Since this is geographically misleading, it should be revised to a term such as "the seas around Japan" or "the Japanese adjacent seas."

We appreciate the referee's comment about the correct naming of our area of study. To avoid misunderstandings, we would follow the suggestion and refer to the domain encompassing the Sea of Japan, the East China Sea, and a portion of the Northwest Pacific as "the seas around Japan", abbreviated to "Japan-adjacent seas" (JS). We will use then "Japan Sea" only to specifically designate the Sea of Japan, located between the Japanese archipelago and mainland China. These changes will be brought in the revised manuscript.

Figure 3: The notation in the figure should be unified to $tCAO$ to match the main text. The same applies to Figure 7 and Figures S1, S2, and S3.

Thank you for the suggestion. We will unify the referral to the time in the plots such that they are consistent with the text.

Figure 3: In the caption, "(right)" should be corrected to "(center)."

Thank you for spotting this error. We apologize for any confusion and will correct the text in the revised figure caption.

Figure 4: How was the box used to calculate the net radiation anomaly selected? If the goal is to investigate the effect of radiation on the formation of CAOs, the analysis should focus on the contribution of radiation along the BCAR traversed by each backward trajectory, rather than using a geographically fixed box.

Thank you for this question, as this allows us to clarify any misunderstandings. Figure 4 aims to investigate the role of net radiation on the formation of peaks in the EGR (independent of the CAO event). Figure 3 showed that there is a large anomaly in the EGR at the entrance of the storm track 1 day prior to CAO events that feature strong upstream cooling. In Figure 4, we therefore wanted to check whether strong upstream cooling is relevant for large baroclinicity events in general, irrespective of the occurrence of a CAO event. As for the box of net radiation, the box was chosen to encompass the region where we also find the largest net radiation anomalies in the days leading up to the EGR peaks in Fig. 4. However, we realise that this additional analysis is disrupting the general flow of the manuscript and is confusing, as it introduces another event set that is otherwise never compared with the rest of the content. Therefore, we decided to remove this analysis (and Figure 4) from the manuscript and instead focus on the comparison of the two types of CAO events.

Figure 5: Explanations for panels (a) and (b) need to be included in the caption.

Thank you for pointing out this missing information. We will include an explanation for panel a and b in the caption.

Page 11, Line 220: It is unclear how many of the 237 cases corresponding to the 90th percentile of the EGR were accompanied by CAOs; because this correspondence is not provided, the results cannot be properly interpreted.

Please note that this analysis was originally conducted with the intention of understanding the relationship between the upstream diabatic cooling (measured using the net radiation) and peaks in baroclinicity (measured using the EGR) irrespective of the occurrence of a CAO. Figure 3 shows that CAOs featuring strong upstream diabatic cooling feature enhanced baroclinicity at the entrance of the storm track with respect to climatology and CAOs featuring small C_BCAR. Because of that we wanted to understand, whether this relationship of efficient upstream diabatic cooling and increased baroclinicity also holds without preconditioning on the presence of a CAO. While going through the referees comments, we realised that we had not explained this motivation well enough in our manuscript, and that we are actually trying to answer 2 different research questions here, that go beyond the scope of a single research paper. To focus the paper on one research question ("How do CAOs featuring stronger or weaker upstream diabatic cooling affect the storm tracks") we will remove Fig. 4 from the manuscript.

Page 11, Line 234: The definition of "ADIAB" is unclear.

ADIAB is the sum of the advective and vertical motion term in equation 3 (the sum of the non-diabatic terms). This additional term was introduced in the figure, since ADV and VMT largely cancel each other out. ADIAB therefore allows us to better understand the role of diabatic and adiabatic processes for anticyclone intensification. We will make the meaning of "ADIAB" more explicit in Sect. 2.4 of the revised manuscript.

Figure 6: Only panel (c) uses a different unit, making direct comparison difficult. The units should be standardized to hPa $6h^{-1}$.

We apologise as there is a typo in the axis label. The vertical axes show the temperature tendencies due to temperature advection, vertical motion and diabatic processes, which will contribute to the surface pressure changes. The units should thus be K/s, which we will correct in the revised manuscript.

Page 15, Lines 292–294: While this statement holds qualitatively, the quantitative extent to which it alters the circulation remains unclear. Therefore, this point should be examined in the Discussion section rather than presented in the Results section.

Thank you for this suggestion. We agree that this section is rather a discussion of the results and in parts redundant with the discussion in Sect. 4.2. We will rewrite Sect. 3.3. such that the emphasis is more on the westward vertical tilt of the upper-level wave with respect to the surface anticyclone (visible in Fig. 7) and incorporate lines 292-294 in the existing discussion in Sect. 4.2 lines 399-408.

Page 15, Line 312, Table 1: How was "after CAO" calculated for the random days?Furthermore, how were the random days selected?

The random days were drawn from all dates in December, January and February between 1979-2023 (the same period used for the CAO identification) using a random number generator. The random days have to be at least 48h apart (the same as the CAO events). This approach is repeated with bootstrapping for 1000 iterations. The label "after CAO" in the context of the random days is indeed not chosen wisely. For CAO events, we evaluate cyclone numbers and properties in the 1-2 days, 1-3days and 1-4 days after the CAO event. For the random days, we also consider 1-2 days, 1-3days and 1-4 days after the chosen day (same as for the CAO events). By constraining the random days to the same set of conditions (separation of 48h, only considering days in DJF) as used for the CAO identification and applying the exact same procedure, we arrive at two comparable sets of events. We will include a more detailed description of the procedure in Sect. 2.5 and change the wording of "1-xdays after CAO" to "1-xdays after".

Figure S5: The figure and its caption are inconsistent with each other.

Thank you for spotting this, indeed the title in the figure 1a) should be “primary cyclones”. We will correct this in the revisions.

Page 20, Lines 410–412: Isn't it expected (or obvious) that the land–sea thermal contrast itself is modulated by synoptic disturbances?

Thank you for this interesting question. We would argue that the land-sea contrast is rather often viewed as a given feature of the background state, than something that can be enhanced by synoptic disturbances. The studies by Portal et al. (2022) and Brayshaw (2009). are good examples of this perspective as they assume the land-sea contrast to be a time-invariant feature forcing storm track activity. Baroclinicity at the entrance of the storm track, on the other hand, could be seen as the instantaneous manifestation of the low-frequency land-sea contrast upon which the effect of synoptic disturbances and cold air outbreaks is superimposed (as shown by Marcheggiani and Spengler 2023, 2025). The subtle difference between land-sea contrast and baroclinicity is analogous to the one present between the tropopause-level waveguide, corresponding to the background, planetary-scale potential vorticity gradient in which Rossby waves develop, and the upper-level jet stream, which is the instantaneous manifestation of both the background flow and the flow induced by transient, synoptic disturbances.

Since we remove Fig. 4 from the manuscript and focus on the research question of how small and large C_BCAR CAOs affect the storm track, we will rewrite this paragraph such that it includes rather a discussion of the more general role of diabatic cooling for the storm tracks (since the results of the comparison of the two CAO event types eludes a more general role of diabatic cooling).

References:

Brayshaw, D. J., B. Hoskins, and M. Blackburn: The Basic Ingredients of the North Atlantic Storm Track. Part I: Land–Sea Contrast and Orography. J. Atmos. Sci., 66, 2539–2558, https://doi.org/10.1175/2009JAS3078.1. 2009

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Marcheggiani, A. and Spengler, T.: Cold air outbreaks drive near-surface baroclinicity variability over storm track entrance regions in the Northern Hemisphere, Weather Clim. Dynam., 6, 1479–1489, https://doi.org/10.5194/wcd-6-1479-2025, 2025.

Portal, A., C. Pasquero, F. D’Andrea, P. Davini, M. E. Hamouda, and G. Rivière: Influence of Reduced Winter Land–Sea Contrast on the Midlatitude Atmospheric Circulation. *J. Climate*, **35**, 6237–6251, https://doi.org/10.1175/JCLI-D-21-0941.1., 2022.

Reviewer 3:

General comments:

This paper investigates the role of diabatic (radiative) cooling over the Eurasian continent in association with cold air outbreaks around the Sea of Japan in modulating baroclinicity and the storm track activity over the North Pacific in winter. The influences of upstream radiative cooling on the evolution of cold air outbreaks and the extratropical cyclone tracks over the North Pacific may be new findings of this paper. However, the focus of this paper seems to be unclear, because the relationship between radiative cooling and cold air outbreaks and the relationship between cold air outbreaks and storm track activity are not separate in the analyses. It is advisable to reconsider the structure of the paper. In addition, there are errors and inconsistencies in figures and captions, which make it difficult to evaluate this paper. I therefore recommend major revision.

Thank you for this constructive evaluation of our manuscript. Indeed, we realise that we are trying to answer two research questions in this manuscript with different analyses targeting different aspects. This makes the interpretation of the results and connecting the presented results difficult for the reader. Taking this feedback to heart, we will refocus this paper on the specific research question of how CAOs featuring strong or weak upstream diabatic cooling affect the storm track. Thus, we will remove the analysis on the EGR peaks shown in Fig. 4 and rewrite the introduction and conclusions such that they are in line with this research question. We hope by focusing more on the comparison of the CAO events featuring small and large C_BCAR the storyline of the manuscript will become clearer.

Specific Comments:

I could not understand why the influences of diabatic cooling were compared only when cold air outbreaks (CAOs) in the Sea of Japan occur. The influence of whole CAOs on the storm track and its relative importance in the North Pacific storm track variability have not been explored in this paper, so the importance of the difference between the effects of large and small *CBCAR* is not clear to the reader. It might be more straightforward to examine the impact of upstream diabatic cooling variations on the storm track variability in the North Pacific, regardless of occurrence of CAOs.

We understand the criticism about investigating periods of CAOs when trying to answer the more general question of the effect of diabatic cooling on the storm track. The main reason behind this choice is that, in order for us to investigate the effect of upstream cooling onto the storm track, the diabatically cooled air has to actually reach the storm track first, and CAOs correspond exactly to situations of offshore flow from the continent towards the ocean. Radiative cooling tends to be a slow process, and the stable continental air masses can sit over land for many days before being advected offshore. Furthermore, there is in principle no guarantee that the diabatically cooled air will always reach the storm track after some time: for instance, the cold air mass can be tempered over land if it is pushed equatorward during a cold spell affecting land regions. These aspects related to the interplay between “slow” diabatic cooling and “fast” dynamics would make an analysis starting directly from the cooling events much more complex. In comparing the impact of CAOs featuring strong and weak upstream diabatic cooling on the downstream storm track, our findings suggest a more general role of upstream diabatic cooling in modulating storm track variability. This has led to us opening up the question to a more general context, even though our analyses are mainly constrained by the context of the CAOs. While we think both avenues (impact of CAOs featuring upstream diabatic cooling or the effect of the upstream diabatic cooling) are worth exploring, we would argue that it is also not straight forward to directly examine the direct impact of diabatic cooling on the storm track. To do so, one first has to develop a method for identifying periods of enhanced upstream diabatic cooling (e.g., could be done using net radiation). For the purpose of improving this manuscript, we will focus on the impact of the different CAOs on the storm tracks, by constraining ourselves to the comparison of large and small BCAR contributions. We believe that this comparison can already allude to the role of upstream diabatic cooling for the storm tracks (without answering specifically) and give interesting insights from a synoptic perspective on the role of CAOs for storm track variability.

(L100-105) Though the trajectory of large *CBCAR* shows stronger cooling than that of small *CBCAR*, the difference between them is 0.26 K/day (Fig. 1b), that is, only 1.04 K per 4 days. On the other hand, the mean θ exhibits a difference of about 4 K at -7 days between the large and small *CBCAR* trajectories. It seems that near surface diabatic cooling has relatively small effects on CAOs compared to other factors, such as the origin of air mass and advection of cold air. What is the motivation for comparing CAOs with large and small *CBCAR* in this paper?

Thank you for this comment as this allows us to explain our approach better. Please note that in Fig. 1 we are comparing two specific subsets of trajectories that already experience strong diabatic cooling, as we are only considering BCAR trajectories. Comparing BCAR trajectories to other types of trajectories, e.g., other low-level trajectories (that stay at low level also, but that do not stay over land for at least 3 consecutive days), we previously found that BCAR trajectories experience the strongest diabatic cooling and ending up with the lowest minimum potential temperature, despite starting with a higher initial potential temperature (Fig. 4b in Schnyder and Riboldi 2026). The substantial difference in the number of trajectories that classify as BCAR trajectories (and as such as strongly cooling trajectories) between small and large C_BCAR CAO events (as shown in Fig 1b of this manuscript) suggests that the upstream diabatic cooling indeed is more efficient, as it manages to cool more trajectories effectively. Furthermore, previous analysis has shown that the BCAR trajectories originate from upstream Siberia (Fig. S7b in Schnyder and Riboldi 2026). While it is true that the initial potential temperature for BCAR trajectories in large C_BCAR CAOs is lower compared to small C_BCAR CAOs, it is possible that these trajectories previous to t-7days have spent more time over the Siberian land mass, and therefore accumulated more diabatic cooling, already bringing them to a lower initial potential temperature at t-7days. It is for these reasons (1) BCAR trajectories experiencing the strongest diabatic cooling, 2) difference in the number of BCAR trajectories and 3) the initial starting locations of BCAR trajectories being located over the Siberian land masses), we argue that the diabatic cooling is a relevant factor.

(L200-219) The effect of radiative cooling is emphasized here, but heating such as sensible and latent heat fluxes from the sea surface, and advection of warm and moist air from the south may be important as well, since baroclinicity is related to meridional temperature *gradient*, not to low temperature.

We agree that both diabatic cooling over the upstream land surfaces and warm processes on the southern side of the baroclinic zone can enhance baroclinicity at the entrance of the storm track. Many papers have previously shown the relevance of warm air advection, latent heating, surface heat fluxes and the SST distributions in increasing the temperature gradient at the entrance of the storm track (e.g., Hoskins and Valdes, 1990; Hotta and Nakamura, 2011; Yao et al., 2016; Bui and Spengler, 2021; Marcheggiani and Spengler, 2025). However, the role of upstream cooling in enhancing the temperature *gradient* has remained unexplored to date. For this reason, we specifically focus here on the role of diabatic cooling at the *poleward side of the baroclinic zone* in enhancing baroclinicity. We will include a brief discussion regarding the importance of processes both on the warm and cold side of the baroclinic zone at the end of Sect. 3.1, so that the scientific focus of the study can be made clearer.

(Section 3.2) A comparison of Figs. 6a and 6b gives the impression that the magnitude of VMT is large at all levels while ADV is large only at upper levels, and therefore the vertically integrated VMT is much stronger than the vertically integrated ADV. However, Figure 5b shows the contributions of ADV and VMT are of similar magnitude. What makes this (seeming) inconsistency?

Thank you for this comment. While addressing this comment, we realised that we made a typo in the vertical profiles: The vertical profiles show the temperature tendencies due to advection and vertical motion (unit K/s) rather than their direct contribution to changes in the pressure changes at the surface (hPa/6h). This makes the direct comparison of the vertical profiles and the horizontal composites as well as the contributions shown in Fig 5b less straightforward. We did however, double and triple check and confirmed that the vertical integral of these tendencies amount to the horizontal composites shown in Fig.6d,e,f. Comparing the horizontal composites, we can see that VMT is larger in magnitude compared to ADV, consistent with the boxplots shown in Figure 5, and the vertical profiles in Figure 6a,b,c.Please also note that the variability between cases is quite large for the two terms (see large spread in Fig. 5b) which may give rise to this seeming inconsistency.

Minor comments:

Figure 1 shows that even for the small *CBCAR* events, the air underwent potential temperature decline of several degrees. What made this diabatic cooling?

Yes, we generally observe diabatic cooling along all trajectories, this is not specific to BCAR trajectories only (see Schnyder and Riboldi, 2026). Hartig and Tziperman (2023) show that diabatic cooling is also an important contributor to cold spells over the North American continent, with the main processes identified being longwave radiative cooling, and vertical mixing. A detailed case study of a large BCAR contribution event showed that the diabatic cooling along the coldest MCAO trajectories generally occurs due to clear-sky, surface-based radiative cooling and, to a lesser extent, cloud-top radiative cooling (Riboldi et al., in prep.). Though this analysis was conducted for a large BCAR contribution event, we expect that the same processes are relevant for small BCAR contribution event, but with a lower magnitude.

(Section 2.4) The Eq. (2) seems to be an equation for the surface pressure, not for SLP. How is the difference between SLP and surface pressure adjusted in the analysis?

Thank you for pointing this out; this is a wording mistake from our side. We used the exact method as Fink et al. (2012) using the surface pressure tendency but were imprecise in our description. It should correctly be referred to as the surface pressure tendency equation. We will correct this throughout the entire manuscript during the revisions.

(L 146) The value of p2 should be specified.

Thank you for pointing out this missing information. We will specify p2 (defined as the upper boundary pressure surface) in Sect. 2.4.

I think that L172-178 is about the "subsequent" cyclones, though the word "subsequent" does not appear here. It might be better to clarify in the text.

Yes, this text is about the subsequent cyclones. We will explicitly refer to the subsequent cyclones to make this clearer.

What are "rotated latitude" and "rotated longitude" that appear in Figs. 6d-f? Please insert a description.

The rotated latitude and longitude comes from the fact that when we make anticyclone centered composites, they are projected to the equator. This eliminates artifacts emerging from meridians getting closer towards the poles. The cyclones themselves are not rotated, so we see that this label is confusing. We will simplify the labels to "latitude" and "longitude" in the revised version.

(Figure 6) The terms in Eq. (3), ADV, VMT, DIAB are vertically integrated quantities and therefore have no vertical structure. However, vertical profiles are plotted in Figs. 6a-6c. What kind of quantities are plotted here? In addition, why is the unit in Fig. 6c different from the unit in Figs. 6a and 6b?

In Fig. 6a-c we are interested in the vertical distribution of temperature advection, vertical motion and the diabatic cooling in order to understand better, where in the column these processes are important. The vertical profiles therefore show the temperature tendencies due to advection, vertical motion and diabatic cooling. As noted earlier, there was a typo in the label of the vertical axis: The units are K/s and not hPa/6h. These are the fields that go into the calculation of the vertically integrated pressure tendency. We will correct the typo in Fig. 6a,b and apologise for the confusion.

(L261) "This contributes to an overall reduction in the uncertainty"... Why can uncertainty be reduced? The use of temperature tendency data from the reanalysis may make interpretation of the results simpler, but it would increase the dependency on the model physics.

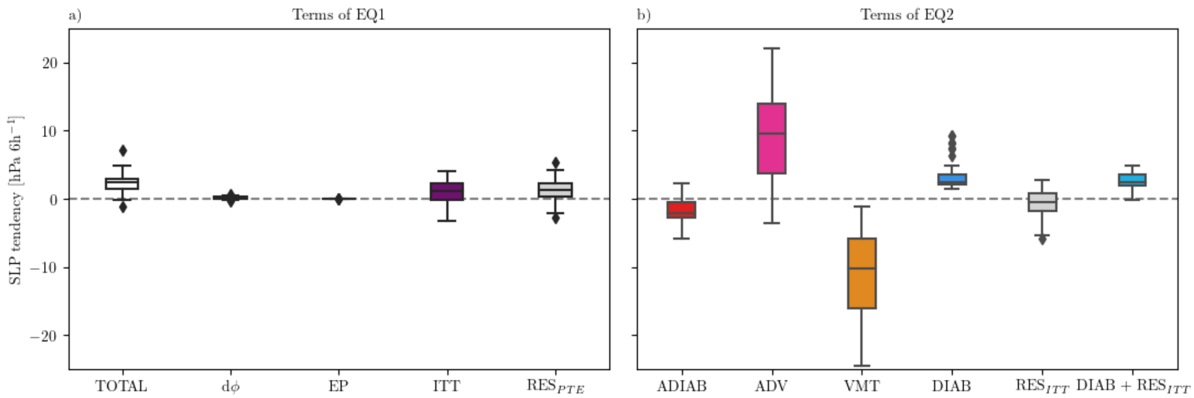

Fig. R1: Processes contributing to changes in the SLP, evaluated around 42 anticyclone centers (r = 500km) at the time of their maximum intensification.

This is true, and we agree that the word "uncertainty" may not have been the best choice. What we meant to say was that in contrast to other studies, where the pressure tendency equation was applied to ERA5 data, our approach of calculating the diabatic tendencies explicitly rather than estimating it as a residual provides novel insights into the exact contribution of the diabatic processes. In our cases, the variability of the diabatic contribution is rather small, indicating quite a consistent contribution to the anticyclone intensification. The residual can correspond to both negative and positive contributions (as shown in Fig. 5b): thus, estimating the diabatic contribution in combination with the residual will potentially result in diabatic SLP contributions of opposite sign despite the clear presence of cooling (Fig. 6c), blurring the conceptual picture. To better showcase this, we add an additional box showing the sum of "DIAB" and "RES_ITT" in Fig. R1b. The interquartile range of "DIAB + RES_ITT" (indicated by the coloured box) is not shifted much compared to "DIAB", showing that in most cases the residual is near-zero (as a residual should behave). However, the lower whisker extends to the zero-line, likely because of a negative residual being added to positive diabatic cooling rate. Consequently, evaluating the diabatic contribution as "DIAB +RES" would lead us to the conclusion that the diabatic contribution in some cases is smaller than when evaluating the diabatic contribution explicitly. We will adjust our formulation in the revised manuscript.

The caption of Fig. 7 is not consistent with the figures. The labels of ordinate read, -6 days, -4 days, -2 days, CAO, -2 days(?), but the caption says, (a,b) -5d, (c,d) -4d, (e,f) -3d, (g,h) -2d, (i,j) tCAO, (k,l) +2d. In addition, there are no (k,l) panels in the figure.

Thank you for pointing out these inconsistencies. We will correct the labels in the revision of the manuscript.

(Figure 8d). I could not find the definition of "area".

The area is computed using cyclone masks following Sprenger et al. (2017). For this, the outermost SLP contour that encompasses an SLP minimum is determined. We will include an explanation of the definition of the cyclone area in the manuscript.

(L318-320) The differences in the maximum deepening rate and the maximum depth between the two types of PRIM cyclones are very small and I do not think they are meaningful.

Indeed, the differences are small (while the variability is large), however they are consistent with the evidence showing enhanced baroclinicity and a deep upper-level trough for the primary cyclones initiating large C_BCAR CAOs compared to those initiating small C_BCAR CAO. Since the availability of baroclinicity and the presence of a strong upper-level forcing are the main ingredients for the growth of (deep) cyclones, we think the small difference in the boxplot is still meaningful. However, we agree that the main differences are between the primary cyclones and subsequent cyclones/climatology, and focus on the differences between those in the revised manuscript.

(L365-366) Considering the marginal difference between cyclone characteristics between the large and small BCAR shown in Fig. 8, I do not think it very convincing that diabatic cooling over land promotes the growth of deep cyclones.

Thank you for this comment. The hypothesis, that diabatic cooling over the upstream continent promotes the growth of deep cyclones, is deduced from both the observations that the baroclinicity is greatly increased (enhanced both with respect to climatology and CAOs featuring a small BCAR contribution) and from the presence of a deep trough at the entrance of the storm track, the two main ingredients for achieving deep extratropical cyclones. From Figure 8 we can, however, also appreciate the large variability between cyclones as indicated by the large spread. Please note that the sample size for this analysis, with 49 primary cyclones per group is quite small. However, we think the fact that the interquartile range for maximum depth and maximum deepening rates for primary cyclones associated with large BCAR contributions is smaller compared to the other groups is not to be neglected, indicating that in particular these cyclones are similar in characteristics. In the revision of the manuscript, we will highlight the large variability and discuss this result more critically.

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