Reply to the reviewer's comments

Anonymous Referee #2

We would like to thank both reviewers for carefully evaluating our manuscript and for providing comments that helped us to further improve our study.

Reviewer: After reading the paper and thinking of the results I am wondering why the authors did not define a single EKE target region, which moves from month to month with the EKE maximum, and performed the analysis this way, i.e. examining the storms which reach each month's region, separated to the different cyclogenesis regions. This would reduce confusion between a reduction of EKE due to a shifting relative to the averaging domain and a real overall reduction of the total storm energy. The main hesitation I have with the approach taken here is the fact that the two regions span around 15- and 10-degrees latitude-order of 1000-1500 km, which is on the order of typical cyclone radii. Thus, I am guessing a cyclone will feel parts of both regions as it evolves and propagates along its track. The interpretation of a latitudinal shift in terms of a dipole is less intuitive on a single storm scale. It sounds intuitive reading the paper since the authors discuss tracks that pass through each region but that in some sense gives a wrong picture. I am not saying the approach is wrong but the authors should somehow justify it, at the very least by a discussion of spatial scales, why they choose to divide the domain this way, and how the results relate to the physical picture of single cyclones. Best will be of course to compare the analysis for single regions which shift with the EKE maximum.

Authors: We very much thank the reviewer for these thoughtful comments and we appreciate the suggestion of choosing a target box that shifts with the EKE maximum.

During January and March, the EKE maximum at the 500 hPa level is located over the mid-Pacific, but in November over the eastern Pacific (black contours in Fig. R1). The corresponding cyclone tracks are generated in very different environments (cyclogenesis frequencies are shown in Fig. R1). Hoskins and Hodges (2002; p. 1060) noted *"that very few synoptic systems can be tracked along the length of the Pacific storm track. Indeed, most of the systems generated over eastern Asia do not even reach the mid-Pacific. It is the systems that are generated in the central-east Pacific that occlude on the northwest coast of North America."* We argue that comparing cyclone tracks generated in such different environments is potentially confusing. Instead, in agreement with Reviewer #1, we propose to focus on the western North Pacific where the maximum in baroclinic conversion occurs throughout the winter. Climatologically, mean baroclinicity is highest over the western Pacific and it increases from November to January (green contours in Fig. R2). Similar to the suppression of EKE, there is, however, a reduction of the baroclinic conversion during this time period (black contours in Fig. R2), which is "unexpected" given the increase in baroclinicity. We therefore propose a modification of the suggestion by the referee and choose a target region that shifts with the maximum in baroclinic conversion over the western Pacific.

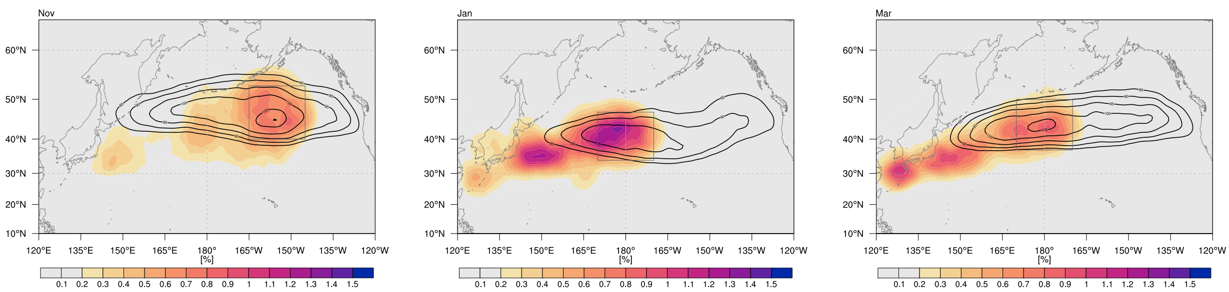

Figure R1: Cyclogenesis frequency (color shading; %) for surface cyclone tracks that propagate through a target region (shown as a gray box) that shifts with the EKE maximum for (left) November, (mid) January and (right) March. EKE at 500 hPa is shown by black contours.

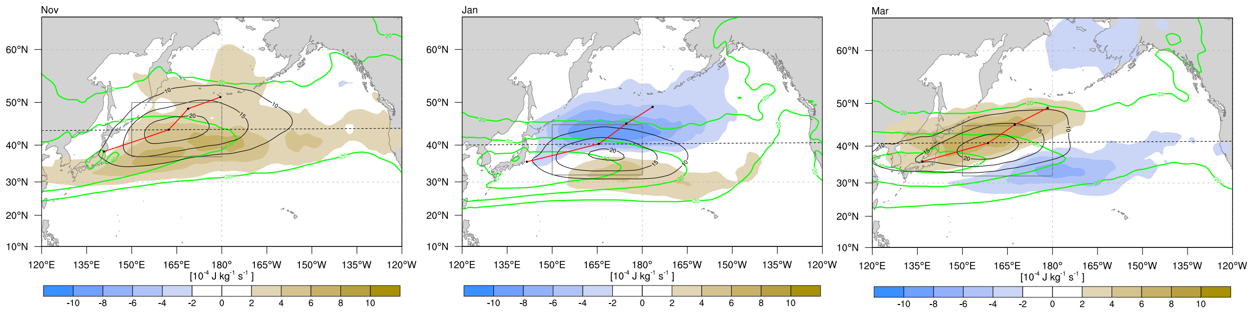

Figure R2: Baroclinic conversion at 500 hPa (black contours; 10^4 J kg-1 s-1) and change relative to the previous month (shading). Gray boxes denote a target region that shifts with the maximum in baroclinic conversion. Also shown is the mean position of cyclone tracks that enter the target region from upstream (red line), with black dots marking the mean position of cyclogenesis, maximum deepening (6-hourly SLP change), maximum intensity (minimum SLP) and cyclolysis.

A target region that shifts with the maximum in baroclinic conversion is shown in Fig. R2. Also shown as a red line is the mean position of the cyclone tracks that propagate into this region from upstream, excluding tracks with genesis in the box (Fig. R1). The first black dot indicates the mean cyclogenesis location, the second the mean location of maximum deepening (6-hourly SLP change), the third the location of maximum intensity (minimum SLP), and the fourth indicates the mean cyclolysis location. During November and March, the location of maximum deepening (second dot) coincides with the maximum in monthly mean baroclinic conversion (black contours). During January, it is slightly north of it. We now repeat the statistical analysis of these tracks. To this end, we split all tracks in time steps before and after the tracks cross the latitude of the monthly mean maximum deepening (shown as the dashed line in Fig. R2) and we next analyze baroclinic conversion averaged in a 1000 km radius at every time step before and afterwards (similar as in the main manuscript).

The corresponding box-and-whisker plot of baroclinic conversion (Fig. R3) shows that baroclinic conversion is highest in January but only before the maximum deepening is reached (this is in agreement with highest baroclinicity in January at latitudes equatorward of the latitude of maximum conversion). Once the tracks have passed the location of maximum deepening, the baroclinic conversion is reduced relative to the time steps before maximum deepening (compare black to gray box-and-whiskers in every month in

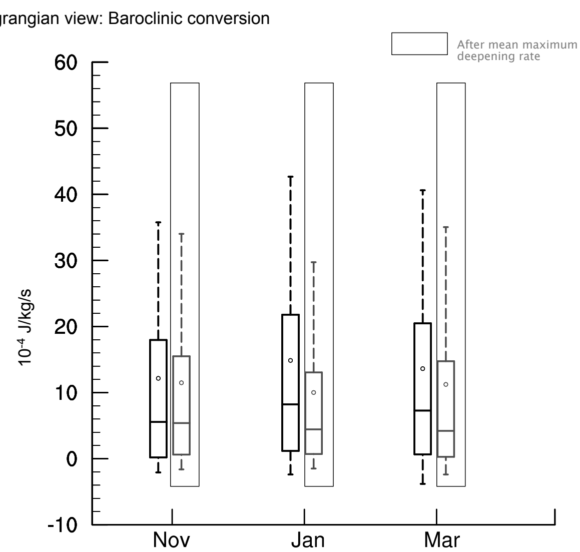

Figure R3: Box-and-whisker diagram for baroclinic conversion at 500 hPa (104 J kg-1 s-1) averaged within a radius of 1000 km around the surface cyclone centers of surface cyclone tracks that enter the target region shown in Fig. 2 from upstream for time steps south of (black) and north of (gray) the maximum deepening (6-h SLP change).

Fig. R3) but baroclinic conversion is now also suppressed in January relative to November in March (compare gray box-and-whiskers between the different months). This is in agreement with the red mean cyclone track in Fig. R2, which on its way poleward propagates out of the contours of largest monthly mean baroclinic conversion in January (black contours in Fig. R2) but in November and Marc. Overall, the results nicely complement our results presented in our manuscript in Fig. 6, where we split the tracks in time steps inside and outside of a “northern target region”. We will add the above new results as a new section to the revised manuscript and we thank the reviewer for suggesting a shifting target box.

Reviewer: Also, it is not clear at the moment if the main contribution of the paper is in elucidating the changes in the eddies which contribute to the midwinter suppression and the dependence on the cyclogenesis region, or if it provides a more fundamental understanding by further by also explaining the changes in the eddies. For the latter, the authors need to tighten the discussion of how the results fit in with existing theory.

Authors: The main goal of our study is to describe how the life cycle of cyclones generated in the different genesis region, which all affect the region of suppression in the western North Pacific, changes during midwinter.

Reviewer: The main underlying theory - that equatorward shifting of the jet results in a weakening of the storms due to their meridional tilt, inherently looks at the entire storm and how its meridional shift varies with height - the division into poleward and equatorward parts in this argument does not necessarily make sense.

Authors: We are undecided to which argument in our manuscript the reviewer is referring to. We separate the cyclone tracks into sections equatorward and poleward of a critical latitude (above in Fig. R2 the latitude of maximum baroclinic conversion) but unrelated to the vertical tilt.

Reviewer: The argument that the baroclinicity shifts equatorwards into the Kuroshio cyclogenesis region during midwinter, suggests at first that the storms should grow more efficiently during mid-winter, but the overall argument made is that they grow less efficiently. I think the answer to this is given in the summarizing argument, on lines 338-345, but I am not sure I fully understand it- do the authors mean to say that the larger meridional tilt seen in Schemm and Riviere is in a sense an artifact of the time averaging over the cyclone life cycle, and since the cyclone moves poleward quicker, while undergoing faster growth and decay as it shifts poleward, the time averaged structure has a stronger tilt? Thus, the overall growth over the full cyclone life cycle is what becomes less efficient? This in essence sounds similar to the original arguments by Nakamura (1992), that storms grow faster but also move quicker, but instead of the stronger zonal wind advecting the storms out of the baroclinicity region, the storms move poleward and they undergo the full nonlinear life cycle of growth and decay.

Authors: We added the baroclinic conversion efficiency to our analysis. It is shown in the revised Fig. 6 of the manuscript and shown below as Fig. R4 of this document. Similar as before, we split the Kuroshio tracks into time steps outside and inside of the northern target region. As suggested by the reviewer, the cyclones are more efficient in January compared with November and March as long as they are equatorward of the target region, but once they propagated poleward and outside the baroclinic zone they quickly become less efficient. This is in agreement with what is shown in brown and blue shading Fig. R4 in Schemm and Rivière (2019). We hope that this clarifies our reasoning: First the cyclones are more efficient and baroclinic conversion rates are large, but further poleward their efficiency is reduced. We do

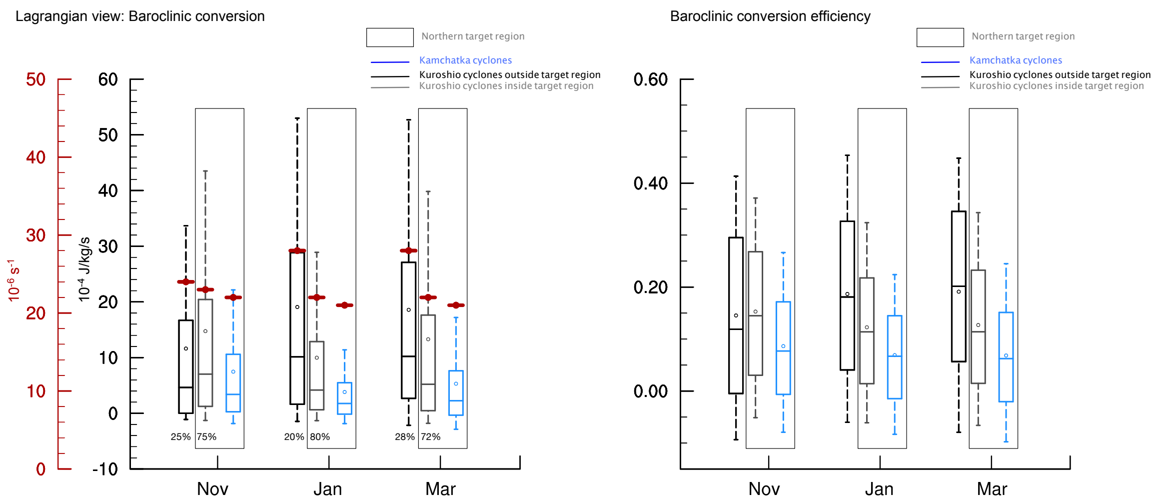

Figure R4: (revised version of Fig. 6 in the manuscript): Box-and-whisker diagram for (left) baroclinic conversion at 500 hPa (10-4 J kg-1s-1) averaged within a radius of 1000 km around the surface cyclone centers of Kuroshio and East China Sea cyclones, before (black) and after (gray) entering the northern target region, and for Kamchatka cyclones (blue). Additionally, shown are the mean background baroclinicity along the tracks (red horizontal lines) and the percentage of time steps before and after entering the target region. (right) Similar as in the left panel but for the baroclinic conversion efficiency (unitless).

not estimate the propagation speed nor do we perform any time averaging along the tracks, but we do agree that in essence the argumentation is similar to Nakamura (1992) that storms move quicker out of the baroclinicity region, but they also become less efficient on their way poleward. We add this link to the original 1992 paper to the corresponding section.

Reviewer: Schemm and Riviere discuss Nakamura and Sampe's argument that the growth is less efficient on a strong and subtropical jet due to a stronger meridional tilt which the storms assume if their surface cyclogenesis remains at the same latitude. They point out that the meridional-vertical tilt implied by Nakamura's argument (equatorwards with height) is opposite to the tilt they find (poleward with height). They mention that the meridional tilt would be different for different seeding latitudes (I assume this is part of the motivation for this paper). I think the authors should more explicitly tie the current results to this argument, and specifically does the change found in Kamchatka cyclone life cycles fit with the argument of Nakamura and Sampe?
Authors: We try to tie our work better to the previous literature using Fig. R4 to which we added the baroclinic conversion efficiency. The figure shows that the cyclones become less efficient when propagating poleward. The main motivation of this paper is the life cycle perspective: How does the conversion and the efficiency change during the life cycle and which cyclones contribute to the reduction in the baroclinic conversion? In Schemm and Rivière (2019) we speculate that the argument of Nakamuara and Sampe applies to the southern seeding branch. According to Chang (2005), the southern seeding branch is associated with East China Sea cyclones (though the upper level waves do not necessarily trigger East China Sea cyclones) and can also sometimes trigger Kuroshio cyclones (though they are mostly triggered by the northern seeding branch). To connect the individual cyclone tracks to one of the two seeding branches would be beyond the scope of this study.

Reviewer: The main results for the Kamchatka cyclones (lines 334-337): "The fraction of explosively deepening cyclones first reduces from November to January but then remains at similar levels until March. Highest values in baroclinic conversion are found during midwinter, but these occur at lower latitudes, south of the northern target region, and they are sustained for a reduced number of time steps. In terms of minimum sea level pressure, Kuroshio cyclones are most intense in January." The finding of a reduction in explosive cyclogenesis but more intense cyclones during January is confusing. Also- is it obvious why the growth in mid-winter is sustained for less time?

Authors: We tried to clarify this point. The fraction of “bomb” cyclones reduces (deepening larger than 24 hPa in 24 hours normalized to 60° N as in Sanders and Gyakum 1980) from November to January, but the largest 6-hourly baroclinic conversion rates are still observed during January. They are maintained for a shorter time period, which matters most for the overall deepening. We do not know if this is obvious, but the life time of cyclones reduces in midwinter and the number of time steps with high growth rates is lowered.

Reviewer: Figure 2: What is counted as propagation through a region- that the cyclone track which follows the cyclone center (a single pixel of minimum pressure?) pass through it, or a part of the cyclone (the region of 1’s corresponding to the detection scheme) passes through it? Similarly- the cyclogenesis is counted as the whole cyclone or its center?
Authors: For the selection of the tracks we use the minimum SLP as the center of the cyclone and the cyclone center must be at least once within the target region. We added this information to the manuscript. For the cyclogenesis, we compute a radius of 500 km around the genesis location to define a “genesis region”. In this way, all genesis events are treated equally.

Reviewer: Figure 3: I am not sure I understand what is shown here - the caption says “relative contributions...to the total surface cyclone frequency in the northern target region”, which implies a very wide cyclogenesis region to the west and north of the target region, which is not what I expect, and I am not sure how this fits with figure 2..? The plots look more like the contribution to total cyclone frequency from those cyclones originating in the target area. But then the percentage is out of the total cyclones contributing to the target region, but not including cyclones which miss the target region? so the sum of the right and left columns add to 100% in the target region but not outside of it? An explicit explanation of how the fields in figure 3 relate to those in figure 2 might help clear things.
Authors: The plot needs a better explanation. The plots are the contribution to the total cyclone frequencies propagating through the target area not originating from the target area. Additionally, we separate those that enter the target area from the south from all others, which essentially separated Kuroshio and East China Sea cyclones from Kamchatka cyclones. In the target region it adds up to 100% but not outside.

Reviewer: Do you have any idea why the number of Kamchatka cyclones decreases and the number of East china sea cyclones increase as the season progresses?
Authors: We do not have an explanation for the increase in East China Sea cyclones. For Kamchatka cyclones is seems to be the reduction in the mean baroclinicity and eventually also a reduction in the upper-level seeding, but this hypothesis would require further testing.

Reviewer: Section 2: Methodology - using a monthly mean static stability alongside low and high pass filtered quantities - how do you deal with the jumps in static stability in between months? how much does the static stability change from month to month? Do you use the climatology or each year’s monthly mean?
Authors: During the preparation for the study of Schemm and Rivière (2019), we tested different filters and averaging windows and did not find a significant difference. For the static stability, the reference temperature profile is in agreement with the traditional literature computed from a climatological monthly mean.

Reviewer: The discussion on page 7 needs some tightening - there is repetition of the results of the previous sections and within the section itself.
Authors: Page 7 is the method section; we would appreciate if the reviewer could point us to the corresponding section that needs some tightening because we are unsure whether the reviewer refers to page 17 or section 7.

Reviewer: line 265- Please state explicitly why you say the non cyclone days contribute *much* more than non cyclone days- they clearly contribute more but its not clear on quick look that its all that much more. Being more quantitative might help.

Authors: In the revised version, we give the exact numbers of the change from cyclone days and non-cyclone days between November and January. We highlight the fact that cyclone days have much higher values in baroclinic conversion, but, as pointed out correctly by both reviewers, the changes are about the same (revised l. 275-280).

Reviewer: line 278- the authors average at a radius of 1000km around the cyclone center. 1000km is roughly the latitudinal length of the southern box, so if the cyclone is at the southern edge of the EKE decrease box, the averaging could include a very large portion of the EKE increase region as well. . . is this problematic and how does this affect the results? see major comment above.

Authors: Yes, this is correct, if the cyclone is near the southern edge of the northern target region it will tap into the southern target region. In Schemm and Rivière (2019) it was shown that when using a radius of 2000 km or an average inside the outermost closed SLP contour used for the tracking, the results are qualitatively still very similar to the 1000 km radius, though smaller (larger) in absolute values, respectively. We believe that the increase/decrease pattern in EKE is partly a result of the changes in baroclinic conversion along the tracks of the analyzed cyclones and when a cyclone affects first the southern box, for some time steps both target regions and further poleward only the northern target region then this is what results in the climatological mean and a transition zone does not seem to be problematic for the interpretation of our results.