Reply to the reviewer's comments

Anonymous Referee #1

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: The paper exclusively focuses on the western Pacific, and does a good job of accounting for midwinter suppression in that region. However, that region covers less than half of the area in the Pacific basin where suppression is observed to occur, which stretches eastward all the way to N America (Fig 1b). The authors note that their focus region is "located at the entrance of the storm track" (l.126), implying that eddies in that region will subsequently move downstream, so that the eastern part of the storm track will behave similarly to the western part. The implicit message is that a theory for suppression in the western region will also explain suppression in the Pacific storm track as a whole. But is this really true? After all, cyclones have a marked bias to poleward propagation, and it's not obvious they will follow the purely zonal propagation required by this implicit statement.

I think that leaving the reader guessing about this point risks being misleading, and requires clarification. For example, the authors could use the cyclone track data to show that cyclones passing through the northwestern "suppressed" box do indeed go on to feed the eastern part of the stormtrack where suppression is observed. Alternatively, they could omit further analysis, but provide a clear statement (in the abstract and conclusions) that mechanisms responsible for suppression in the east require further analysis.

Authors: This is an excellent point. We focus on the part of the storm track over the western North Pacific where baroclinicity is largest in midwinter. The cyclone tracks analyzed in our study have their lysis on average poleward of 50° N in the central Pacific. The Pacific storm track is known to "restart" over the central Pacific. 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.*" A finding, which was later confirmed by Wernli and Schwierz (2006; Figs. 9c and 9d). Indeed, a large fraction of the cyclogenesis over the eastern Pacific is secondary cyclogenesis (Schemm et al. 2018; Fig. 5b). We therefore fully agree with the reviewer that our study, focusing on the western North Pacific, does not address midwinter suppression of the entire Pacific storm track. To potentially explain the suppression over the eastern Pacific, it could be rewarding to explore suppression of the downstream development (Simmons and Hoskins 1979, Orlanski and Chang 1993), which would be of high scientific merit and should be reserved for a follow-up study.

In the revised manuscript, we adapted the title and we provide a clear statement in the abstract and the conclusions that we focus exclusively on the western North Pacific where climatological mean baroclinicity is highest and that the suppression over the eastern Pacific requires additional analysis.

Minor comments

- l. 46: "Subtropical jet regime": For the reader not deeply versed in the current literature, it would be useful to give a brief explanation of what you exactly mean by this expression (and what other regimes are possible).
  Authors: We added an explanation.

- l. 66: "propagate in tandem poleward": Fig 21 in Hoskins et al 1985 and surrounding text do not actually say anything about preferential poleward propagation, so far as I can see; the poleward propagation mechanisms instead are discussed in later work for example by Gwendal Riviere and Talia Tamarin, and possibly others I'm not familiar with. Some citations to literature on poleward propagation should be inserted here. This is clearly also relevant to my main comment above.
  Authors: Yes, we added more appropriate references. It is long-standing knowledge from case studies (Palmén and Newton 1969), also discussed in idealized experiments (Hoskins and West 1979, Davies et al. 1991) and later from feature-based climatologies (e.g., Hoskins and Hodges 2002) that cyclone tracks are deflected poleward. Important recent studies about the underlying mechanism are by Gilet et al. (2009), Rivière et al. (2012) and Tamarin and Kaspi (2017). In our study, the poleward motion can be inferred from Fig. 3, which shows that less than 20% of all cyclone tracks in the Gulf of Alaska are generated over the Kuroshio.

- l. 110: please state the cutoff frequency used for the high-pass filtering.
  Authors: We added this information. It is 10 days.

- l. 117: The analysis of EKE and baroclinic conversion in this and later sections is all carried out at 500 hPa. This choice needs some justification. Would analysis at other levels, or in the vertical average, give the same qualitative results and conclusions?
  Authors: The level is a pragmatic choice. The midwinter increases with altitude (Schemm and Schneider 2018), but baroclinic conversion is typically largest in the lower troposphere. At the 500 hPa level, baroclinic conversion is still large and the midwinter suppression is a well-marked feature in the conversion rates. Qualitatively, we expect the same results for vertical averages or integrals. For example, the study by Schemm and Schneider (2018) was based on vertically integrated conversion rates and yielded comparable results. The location of our target regions is also not affected by the choice of the level and all results related to the surface cycle tracks are thus largely independent of the vertical level.

- Fig 1: It would be useful to show a plot of cyclone track densities overlayed on EKE to appreciate their relationship (this could be done directly in Fig 1, or separately in supplementary material to avoid clutter).
  Authors: We show cyclogenesis frequencies and considered adding the surface cyclone frequencies to Fig. 1 and Fig. 3., but the cyclone frequency peaks poleward of the EKE maximum and provide no new insight into the nature of the suppression. A figure showing EKE and surface cyclone frequencies overlayed is Fig. 1 in Schemm and Schneider (2018). Following the reviewer's suggestion, we show the mean position of the tracks (Fig. R2 in reply document to Reviewer #2) and we will add it to the final version our study.

- l. 163, Table 1: please specify what exact genesis regions are used to define Kamchatka, Kuroshio and East China Sea cyclones.
  Authors: We added this information to the table.

- l. 216 and elsewhere: I recommend sticking to the expression "feature tracking" or "cyclone tracking", rather than the vague and potentially misleading "quasi-Lagrangian". Many studies (including some by these authors) combine true Lagrangian analysis with feature tracking, in which case the inappropriateness of "quasi-Lagrangian" becomes obvious. Better for the community to have a single word for a single concept.
  Authors: We fully agree and removed "quasi-Lagrangian" from the manuscript.

- l. 245: Surface cyclones do not necessarily correspond only to deep (troposphere filling) eddies; they could also be shallow, diabatically maintained eddies. Some rewording may be needed here.
  Authors: Correct, we adapted the sentence accordingly.

- l. 265: Some quantification would be useful here: what fraction do cyclone days/non-cyclone days cumulatively contribute to mean baroclinic conversion, and to the suppression in January?
  Authors: On cyclone days, the conversion reduces from 17 x 10^4 J/kg/s in November to 6.4 x 10^4 J/kg/s in January and on non-cyclone days from 13 to 3.4 x 10^4 J/kg/s in the same period. In both categories, we find that the suppression is approximately 10 x 10^4 J/kg/s and the number of days in both categories is about 50%. In the revised paper, we highlight this more prominently. We also include the exact numbers in the corresponding section. We also emphasize that a non-cyclone day might still be affected by a cyclone, which propagates in close proximity or along the edge of the target region. But it is now correctly mentioned that baroclinic conversion on non-cyclone days is equally affected by the suppression and the relative reduction is similar.

- lines 292 and 301: Seems to me, by eye from Fig 6, that mean baroclinicity is reduced from Nov to Jan by about the same amount for both Kuroshio and Kamchatka cyclones. It's possible I'm misunderstanding here, in which case please clarify this point.
  Authors: This is correct, mean baroclinicity reduces by the same amount for both cyclone categories from November to January. We added a new panel to Fig. 6, which shows the conversion efficiency. It is not only the reduction in mean baroclinicity that matters, but Kuroshio and Kamchatka cyclones also become less efficient in converting the mean baroclinicity into eddy total energy. The reduction in baroclinicity and conversion efficiency contribute both to the reduction in baroclinic conversion. For example, the mean baroclinicity over the northern target region is similar for Kuroshio and Kamchatka cyclones, however Kamchatka cyclones have lower conversion rates due to an overall lower conversion efficiency. But it is correct, the relative change from November to January in the mean baroclinicity and conversion efficiency is for both the same. We clarified our reasoning in this section.

Literature:

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