Weather Clim. Dynam. Discuss.,
https://doi.org/10.5194/wcd-2020-33-RC1, 2020

Interactive comment on "The North Pacific Storm-Track Suppression Explained From a Cyclone Life-Cycle Perspective" *by* Sebastian Schemm et al.

Anonymous Referee #1

Received and published: 21 September 2020

This is an interesting and well-written paper combining Eulerian and feature-tracking diagnostics to investigate midwinter suppression of the Pacific stormtrack. The topic and approach fit very well into the scope of WCD. I think that the conclusions and interpretation are well supported by the evidence presented and the manuscript does not suffer from flaws requiring major revision. However I have one comment that may require some minor additional analysis, and several requests for clarification and minor rewording.

Main comment:

The paper exclusively focuses on the western Pacific, and does a good job of account-

ing 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.

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).

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.

l. 110: please state the cutoff frequency used for the high-pass filtering.

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?

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)

l. 163, Table 1: please specify what exact genesis regions are used to define Kamchatka, Kuroshio and East China Sea cyclones.

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.

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.

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?

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.

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