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

Anonymous Referee #2

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This paper examines from a Lagrangian storm-following perspective, the theory that the Pacific midwinter suppression results from the baroclinic conversion becoming less efficient as the jet shifts equatorward to a more subtropical position during mid winter. In particular, it examines how this picture is modified by the fact that there are three different regions from which storms originate and seed the Pacific storm track. For that the authors track surface lows that reach two target regions, defined based on the changes in EKE between Nov and Jan (and between Jan and Mar), in the Eastern Pacific (the storm track entrance region). They then examine statistics of the evolution of the cyclones which pass through the two regions. I think the results are interesting, convincingly robust, and relevant, thus the work merits publication however there are a

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few points which need addressing prior to publication.

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.

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.

There are a few confusing points in the discussion, which I will try to point out here:

- 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.

- The argument that the baroclinicity shifts equatorwards into the Kuroshio cyclogenesis region during mid winter, 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..

- 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 Kamachatka cyclone life cycles fit with the argument of Nakamura and Sampe?

- The main results for the Kamachatka cyclones (lines 334-337): "The fraction of explosively deepening cyclones first reduces from November to January but then 335 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

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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?

Specific comments:

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?

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.

Do you have any idea why the number of Kamachatka cyclones decreases and the number of East china sea cyclones increase as the season progresses?

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?

The discussion on page 7 needs some tightening - there is repetition of the results of the previous sections and within the section itself.

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.

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.

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