The two anonymous reviewers are scientists who are experts in the relationship between synoptic dynamics and climate. Their reviews are exceptionally thorough and insightful; addressing all of their major concerns will improve the manuscript.

Please pay particular attention to the following concerns, which I also flagged in my reading of the manuscript:

- Reviewer 2 is concern about how the target region is defined, and I agree. Presently the two boxes used in the analysis do well to describe why the maximum EKE shifts equatorward in going from November to January, but it is not clear how or why that relates to the mid-winter minimum in maximum EKE. In all three months analyzed, the maximum in EKE is found at about 43N and it straddles the two boxes and so the boxes don't capture the traditional view of the midwinter minimum in EKE: a reduction in the *maximum* of EKE. How would the results and conclusions differ if one box was used that was centered on the maximum EKE?
- Both reviewers note that it isn't clear that changes in the baroclinic conversion on cyclone days is that much greater than on non-cyclone days, and I agree. As per reviewer 1's request, some quantification of this statement is required. Reading the mean values off Fig. 5, the relative daily contributions to drop in baroclinic conversion between Nov and Jan on non-surface cyclone days is (~13 J/kg/s *.55 – 3.5 J/kg/s *.51)*M = 5.4 M J/kg/s (where M is the number of seconds in a day), which is as large as that due to the drop due to surface cyclones days (~17 *.45 – 7*.49)*M = 4.2 M J/kg/s. And that raises an important concern: if the conversion on 'non-surface cyclone days' is due to upper level cyclones, this suggest the upper level cyclone changes contribute as much as surface cyclones to the change in total EKE. It seems that the reduction in baroclinic conversion that is not associated with a surface level cyclone is as important to the change in EKE (and perhaps in the midwinter minimum in the maximum EKE) as is the change in vertical structure of cyclones with a surface signature.
- Reviewer 1 is concerned that the analysis only addresses the changes in EKE in the far western Pacific, and that no evidence is presented for the changes in the central and eastern Pacific. Review 1 suggests either the authors perform a relatively straightforward calculation to demonstrate the relevance of the far western Pacific results to the bulk of the Pacific, or be clear that the conclusions are specific to mechanisms for the changes in EKE

in the far western Pacific and future analysis should be done to address changes in the central and eastern Pacific. I will leave it to you to choose which way to go here.

A separate question I have (related to a point raised by Reviewer 2) concerns the use of a monthly mean of stability S in the calculation of baroclinic conversion. There is a lot of low frequency variability associated with the stationary wave coming off east Asia: how would the results differ if a low passed version of S was used – the same filter used to estimate theta_bar in this calculation?