Reply to the editor's comments

We would like to thank the editor for concisely summarizing the concerns of both reviewers and sharing further comments and ideas that have helped us to further improve the manuscript.

Editor comment #1: How would the results and conclusions differ if one box was used that was centered on the maximum EKE?
Reply: This is a good idea. A detailed analysis of a box shifting with the maximum in baroclinic conversion and further comments can be found in our reply to review #2.

Editor comment #2: 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. 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.
Reply: Yes, this is correct, the relative changes are similar. A quantification is given in the revised manuscript and also in the reply to review #1. Absolute values of baroclinic conversion are larger during cyclone days, but the relative change in the conversion on non-cyclone days from November to January is quite similar to that of cyclone days. Note, the definition of non-cyclone days allows that the target region is covered by up to 25% by a surface cyclone. We do agree that baroclinic conversion associated with upper level eddies that are shallow (not extending through the depth of the troposphere) is also suppressed.

Editor comment #3: 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.
Reply: We fully agree. We now provide clear statements in the abstract and conclusions. The eastern Pacific requires further investigation. We also adapted the title of our study. For more details see the reply to reviewer #1.

Editor comment #4: 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?
Reply: During the preparation of the data for Schemm and Rivière (2019), which uses the same data set, we tested different ways to compute S and the results only marginally differed. We thus decided to stick to the traditional way of defining S based on a vertical reference temperature profile of the monthly mean as in Cai and Mak (1990) or Orlanski and Katzefy (1991).