We thank the co-editor and Referee #2 for their useful comments in this second round of revision. To address these comments, some substantial changes are made to the analysis. One of them is the extension of the analysis to 2019, which contributes to increasing the robustness of the results. Another is to use field significance whenever statistical significance is tested. We also standardize the threshold for separating $SVD1_{SST}$+ years from $SVD1_{SST}$- years to a standard deviation of ±0.75. Note that we changed the order of Figs. 8 and 9 to improve the flow of the manuscript.

While working on this revision, we have noticed some unfortunate mistakes. A minor one is that wave activity fluxes shown in Fig. 7 are for 300 hPa instead of 500 hPa. A more major one is that in order to separate the years when SVD1 is positive from those when it is negative in the analyses shown in Figs. 8 and 9 we used $SVD1_{SSV}$ instead of $SVD1_{SST}$ as stated in the text. After correcting this mistake, and adjusting the reference location to where changes in correlation between v' and T' are most significant according to field significance (Fig. 6), differences in eddy structures became less important. Nonetheless, we choose to keep this analysis to illustrate that subtle changes in eddy structures can lead to the observed changes in energy conversion.

Co-Editor Comments:

1. In section 3.2, the authors need to make a clearer statement on the relation between ENSO and SVD1_SST, which also refers to referee #2's major comment 1. Based on the results in Tab1, correlations between the time series of SVD1_SST and Nino 3 (Nino 3.4) index already reach -0.96 (-0.94), suggesting that SVD1_SST mostly reflects the variability of ENSO. I think this is the main rationale that SVD1_SSV (and the authors' following analysis) can greatly represents ENSO's influence. However, compared to this evident result, the authors' argument on the relation between ENSO and SVD1_SST in the text seems a bit weak and less straightforward.

We now make a stronger statement by adding the following (in bold): Indeed, the time series representing the temporal variability of this pattern (SVD1SST, Fig. 2c) is strongly anticorrelated to all the four Niño indices (Table 1), which indicates that $SVD1_{SST}$ essentially reflects SST variability associated with ENSO.

2. In section 3.5, I suggest the authors revise their argument on the relation between winter-mean state and extreme temperature events (i.e. line 321-322), which also refers to referee #2's major comment 4. The winter-mean state is the average of the day-by-day temperature in winter season, in which the extreme events always make considerable contribution. Thus, the mean state and extreme events are intrinsically coupled. Merely from Figure 10, it is not convincing enough to conclude that the mean state sets the frequency of the extremes.

We agree with the co-editor and referee #2. We cannot causally link the winter-mean and the frequency of extremes, because the winter mean is itself an average of all days, including the extremes. We thus avoid implying causality and rather aim to discuss changes in the properties of the probability distribution of surface temperature associated with the observed changes in the winter-mean, variability, and frequency of extremes.

3. Line 161-162: the domain integrated energy flux term might be small over some region but not zero. For the west coast of North America, as it is in the downstream region of the jet stream, the contribution of this term may be not negligible. It is fine that the authors focus on energy transfer/conversion terms, but the description of the energy flux term need to be more accurate.

To clarify this aspect, we add: Fluxes of energy by the mean flow ($-\nabla \cdot \overline{\boldsymbol{u}}(EAPE + EKE)$) and pressure work ($-\nabla \cdot (\boldsymbol{u}'\Phi')$) are not assessed in this study since they can basically contribute to redistributing energy horizontally, and thus cannot explain the modulations of EAPE and EKE observed in our analysis. Although there are non-negligible local contributions from these terms, mostly associated with the downstream transport of EAPE and EKE by the basic-state westerlies over the North Pacific, their overall contribution is small in comparison to CP and CK when integrated over a large domain.

4. Acknowledgements: I think the reviewers deserve a mentioning here.

We add proper acknowledgments.

Referee # 2 comments:

This study is interested in the influence of tropical Pacific SSTs on subseasonal variability in North American SAT during the winter season. The authors use SVD, regression/correlation and composite analyses to investigate how ENSO affects subseasonal variability through modulation of subseasonal eddies - specifically, via changes to the vertical structure of the eddies which have bearing on the amount of baroclinic energy conversion that occurs. The revised manuscript has gone a long way towards addressing a number of concerns about the clarity, interpretation and statistical significance of the study, and I very much appreciate the work the authors have put in. The subject is interesting and relevant to improving our understanding of climate dynamics, as improving near-term climate predictions, and better understanding the large-scale conditions for extreme events. It would be good to see a few final issues resolved before publication.

MAJOR COMMENTS

1. Thanks for the explanation of why this SVD technique was used over traditional ENSO indices. I agree it is worthwhile exploring the "flavour" of tropical Pacific variability associated with SSV of temperature over North America. As I understand, it turns out that the SST pattern from the SVD is ENSO-like, so it doesn't really give us any new information on "flavours" - other than perhaps the discussion of the residual in section 3.6. If so, then it seems okay to leave the title as is, but in other places, this result and implications should be clarified (e.g., ENSO should be replaced by "tropical Pacific variability" in places like the first line of the abstract and title of section 3.2; the abstract and summary should convey that the important thing for North American SSV turns out to be pretty much ENSO). Otherwise, the nice addition/explanation on L105 is completely disconnected from the rest of the manuscript.

Thank you very much for the feedback. Following your recommendations, we have modified the abstract and the title of section 3.2 to make it clear that we assessed the connection between tropical Pacific variability and North-American subseasonal variability and found that ENSO-like variability is dominating this connection.

2. The original review included a comment about the portion of extratropical SSV related to ENSO. I noted that it seemed important to establish this up-front, since later on in Fig. 10, you show an SSV signal unrelated to your ENSO index (SVD1) that is both substantial in amplitude and very similar to the ENSO-related signal. The addition of Fig. 1 showing the climatology is very useful, and I see now about 10% of the local interannual variance is related to ENSO, compared to 50% unrelated. Can you explain a bit more then why you conclude that ENSO plays a prominent role in modulating SSV over North America (L356)? Is the idea that the 10% we get from ENSO is at least predictable, compared to the rest of the SSV that is just internal atmospheric variability? It seems important to mention this in the abstract also.

Thank you for the comment. We now mention in the abstract that $SVD1_{SST}$/ENSO explains a major fraction of the SSV variability over North America that is associated with tropical SST variability. To avoid such misinterpretation that it explains a large fraction of the total SSV variability, we have added: “We find that El Niño-Southern Oscillation (ENSO) explains a dominant fraction of the year-to-year changes in subseasonal SAT variability that are covarying with SST, and thus likely more predictable”. We have also added the following discussion in the summary section: “This relationship explains about 77% of the squared interannual covariance between SST and SST-driven SAT variability and a more modest (up to ~10-20% in some sectors) of total subseasonal SAT variability including SST-forced and internal components. Although small, this fraction is nonetheless important because it represents what is predictable from SST variability, unlike atmospheric internal variability which is less predictable.”

3. The results would be more compelling if the statistical tests were a bit more systematic, to allow the reader (and the interpretation/discussion) to focus on the robust signals. - some figures use a 95% significance level, others use a 90% level - also, I believe in some cases where "confidence level" is used, it should be "significance level" - some

composites are defined with +/- 1 values of SVD1, others with +/- 0.5 - the correlations in Fig. 6 are quite noisy spatially) and use a rather generous significance level - it seems like field significance should be tested – no significance indicated on Fig. 9, which explores the mechanism.

Thank you very much for the comment. We have standardized our statistical tests to use field significance (Wilks 2016) that we describe in section 2.5 with a significance level of α_{FDR}=0.1. We have also standardized composites and eddy structure analyses with a threshold of 0.75, which is midway between the two thresholds that were used previously. In addition, we have added field significance to Fig. 9 to show where regressed geopotential height anomalies are significant. However, we cannot show the significance for heat fluxes since they are a product of two regressed quantities.

4. Section 3.5 is clearer than it was previously. However, I'm not sure the reasoning hangs together with the results as they're presented - it seems some of the arguments would need to be backed up by more rigorous analysis of the temperature distributions and how much they overlap. For example, I don't think we can conclude whether the winter-mean sets the frequency of extremes by shifting the distributions (L322), or whether the presence of a few extreme days determines the winter-mean. Perhaps it would be better to make the discussion more general overall - in some regions it seems the change in SSV broadens the distribution towards one side or the other (cold or warm), and in other regions, while in other regions, we see what may be more a shift. And then show some histograms to bolster the discussion?

We agree with the reviewer's comments. We now avoid implying causality between shifts in the winter mean and the frequency of extremes since winter-means are influenced by extremes. We rather attempt to discuss changes in the probability distribution of temperatures (including the mean and variance) and how they are linked (without implying causality) with changes in the frequency of extremes. Following the reviewer's comment, we have added a new figure (Fig. 11) to show histograms at locations that are affected by shifts and broadenings in the distributions.

OTHER POINTS

- L173: "North America" might be more straightforward than "conterminous..."

Modified

- L173-174: lower-case "northwest-southeast-tilted"

Corrected

- L143 and L179: slightly different filter details for high-pass

This is on purpose. To illustrate the spatial distribution of high-frequency variability associated with migratory cyclones and anticyclones we used a 2-8 day filter to be consistent with previous literature where this band is used frequently. This filter excludes mesoscale systems, tides, and larger synoptic-scale systems at the boundary of subseasonal variability. For the analysis of energetics, however, we used a 10-day high-pass filter to include all variability whose frequency is higher than the subseasonal time scale as defined. This choice does not have any adverse impact on our diagnostics since mesoscale systems and tides do not provide large feedbacks on subseasonal variability. The feedbacks captured are largely due to synoptic systems.

- L231: seems Fig. 6 is mentioned in the text before Fig. 5

We do indeed refer to Fig. 6 before Fig. 5 because it is practical to refer the reader to some information shown in Fig. 6 at that time, but it is more logical to keep the discussion associated with Fig. 6 after the discussion associated with Fig. 5. To avoid confusion, we indicate “(**shown later in** Fig. 6)”.

- L239: How was 1500 chosen as the optimal number of resamples?

We found by carrying out repeated trials that significance is stable by this number of sample, thus we chose it to reduce the computational costs. Nonetheless, we increased it to 3000 for the revised manuscript.

- L292: "Differences in the Z500..." - where do we see this?

We now specify that we are referring to Fig. 8 and list Alaska as an example.

- Captions should include more details so that the reader need not go back to the text to look up information, abbreviations, etc.

We have added information about the time scales of variability in Figs. 1, 2, and 12 and indicate the meaning of SSV.

- Fig. 3: colour bar for energy conversion should probably be adjusted

We have adjusted the color bar.

- Fig. 8: nice with the crosses, but they don't show up well in this colour

We change the color to bright green.

References:

Wilks, D. S., 2016: “The Stippling Shows Statistically Significant Grid Points.” *Bull. Am. Meteorol. Soc.*, **97**, 2263–2274.