Review comments: On the time evolution of ENSO and its teleconnections in an ensemble view - a new perspective (ESD-2019-57)

In this paper, the authors aimed to examine changes in ENSO SSTA patterns and teleconnections under climate change, using a recently developed ensemble-based method (SEOF). SEOF was applied on all the ensemble members from CESM1 at each time step, avoiding using statistical mean or standard deviation under a non-stationary climate.

This ensemble-based method provides a very interesting perspective to study change in the warming climate. This manuscript showed how to apply this method on the change in ENSO patterns. However, I do feel this manuscript lacks details in terms of the physical interpretation of the method (SEOF) and results, which makes the article quite difficult to follow. Also, the content of change in teleconnections which only used correlation to analyze (Section3.2) seemed to be insubstantial and did not really provide new ideas. Thus, I suggest a major review to provide more information to help readers to interpret the SEOF methods and their results. Also, for example, how do the results (change in SST amplitudes or variability and teleconnections) make sense physically? Here are some specific comments I've made:

Major comments

1. I've found it is a bit difficult to interpret SEOF method intuitively. My understanding is that: conduct EOF analysis over all the ensemble members at each time step, as an analogy to conduct EOF analysis over a time series under a stationary climate. Therefore, each ensemble member here represents each year (under a stationary climate).

However, currently, the majority of climate studies treat ensemble members as different possibilities caused by atmospheric internal variability. The standard deviation of ensemble members is used to evaluate the strength of internal variability (noise), while the ensemble mean is used to present the response to forcings (signal). Thus, in this study, it is confusing when the authors use the std of PC1 to represent the strength of ENSO.

I would suggest to provide more details, leading the readers to easier understand the merit of snapshot framework & SEOF since it is a relatively new method. The current descriptions (in terms of the method) lack of details and difficult to follow (e.g. L41-45; L112-116).

2. The authors kept emphasizing that snapshot framework is better than the traditional temporal statistics method (e.g. L74-77, L231-235). However, the authors did not provide detailed explanations of the pros/cons of both methods, nor did they compare the similarities and discrepancies of the results from the two different methods. Were their results more reasonable (in terms of physics) compared to the ones using temporal statistics method?

I would think that using the time period says from 1900 to the present, it is feasible to compare the results from these two methods with the reanalysis data. By doing so, it would provide a more convincing evidence that the snapshot framework is a more suitable tool.

3. As noted in several recent studies (e.g. Seager et al. 2019 *Nat. Clim. Change*), in most of the state-of-the-art GCMs (including CMIP5), they have an El Nino-like trend in SST over the tropical

Pacific in the warming climate, inconsistent with the observation (that is, increase in the west-east SST gradient -> La Nina-like trend in SST). Does this bias exist in the CESM1? If so, would it affect the fidelity of the research (change in ENSO pattern in the warming climate) here? http://ocp.ldeo.columbia.edu/res/div/ocp/pub/seager/SeagerEtAl2019NC.pdf

4. The correlation analysis in the section 3.2 did not really provide constructively new ideas. The correlation between ENSO SST anomalies and precipitation across the globe has been well-examined and established for decades. And the relationships from this manuscript (e.g. L165-169) are consistent with the previous literature. Therefore, the zero-lag correlation analysis in the current climate here seems to me only demonstrates that CESM1 and the snapshot framework can decently produce ENSO-related SST-precipitation relationships.

Also, the impacts of ENSO on precipitation (or say teleconnections) cannot be simplified by just examining correlation, especially for boreal summer season. ENSO can be at developing or decaying phases during boreal summer season. The teleconnection patterns and therefore impacts on regional precipitation can be quite different between these two phases. Moreover, El Nino and La Nina have asymmetric characteristics during the decaying phase: an El Nino tends to decay rapidly; while a La Nina tends to decay slowly and even persist into the following winter (e.g. Okumura and Deser 2010). In this context, the teleconnection patterns of El Nino and La Nina are not mirror images, which means, applying correlation analysis on JJAS variables might not be able to reflect the real impacts from El Nino and La Nina on teleconnections. And in this sense, the half-year-lag correlation conducted in this article did not reasonably consider the life-cycle of an ENSO event. https://journals.ametsoc.org/doi/full/10.1175/2010JCLI3592.1

As the sensitivity of seasonal precipitation over land depends strongly on the configuration and location of teleconnection patterns, I would suggest the authors to include the atmospheric circulation patterns when discussing the change in teleconnection patterns. The circulation patterns could also provide more intuitively physical sense that how the change in tropical SST modulates the large-scale atmospheric circulation and thereby precipitation over the remote area.

5. The title says “On the time evolution of ENSO and its teleconnections in an ensemble view”. This title does not explicitly express that the focus of this paper is the change in ENSO under climate change scenario. Instead, “time evolution of ENSO” strongly misleads to the evolution of an ENSO life-cycle (from developing to peak to decaying phases…).

6. In general, as the authors deployed EOF analysis on all the ensemble members at each time step and compared the results from EOF analyses at different time steps. I would suggest that when mentioning variability, change or any analysis used in the article (e.g. linear fit), it would be beneficial to (explicitly) explain that it is over ensemble members or time steps.

7. The authors used “time instant” in this article, but I found this is really confusing. “Instant” gives people a mistaken impression that it means “an infinitesimal space of time”. I would suggest that time step is one of the possible alternatives. Or the authors could emphasize that “time instant” used here means seasonal average when “time instant” was mentioned the very first time in the article. Similarly, “instantaneous forcing” (e.g. L48) is also confusing. Is the forcing just turned on for a very short while?

Minor comments

Introduction

1. L26: However, the model simulations of future ENSO changes diverge widely among climate models. & L36: To avoid the above-mentioned contradiction, in this study we present an ensemble-based analysis

My question is, how could the authors be sure that their method provided the right direction? (Similar to the 2nd major comment).

2. L39-41: Instead of just listing these papers, I would suggest the authors specify some topics that have been examined using large-ensemble when referring these papers.

3. L55: "This approach": what approach? Large ensemble? Or snapshot framework
Seems like it means "large-ensemble" based on the following context.

Data & Methods

1. Why use JJAS 4-month average compared to DJF 3-month average?

2. L109: other way around? Consider leading SEOF mode as instantaneous ENSO loading pattern?

3. The authors mentioned Maher et al. 2018 several times (e.g. L105; L142), I am not sure all of them are necessary and provide useful information. As many readers might not read the paper before, so if the authors would like to include it, it would be better to provide more details why the authors need to compare them.

Results

1. L131: SST variability? SST anomalies?

2. L150: the explained variance in JJAS is increased (Fig.3b) -> Does this mean ENSO pattern is more favorable in the future? If so, is this consistent with previous studies?

3. Section 3.2: As mentioned in the major comments, JJAS could be during the developing or decaying phases among an ENSO life-cycle, it is important to specify the lag-relation. For example, it is well-known that the Indian Ocean has delayed response (that is during the decaying phase) to an El Nino.

Also, L188-203, it would be much more helpful if the authors could include the change in atmospheric circulations. This would provide more physical sense of how the atmosphere would change given the change in the tropical SST. Just listing the changes in precipitation over some random areas does not really provide essential information for readers to take away.

4. L202: we conclude that a half-year-forward estimate of the precipitation from PC1 data in these regions becomes “more accurate” -> this statement is not accurate. What did the authors mean “more accurate” (in terms of what? forecast?)? How did they assess the accuracy?

Conclusions

1. L213-215: Why are the changes in the ENSO pattern in JJAS season larger than in DJF season? Do the authors have any possible physical explanation of this seasonality difference?

Figures

In general, the figures are not easy to read. For example, the authors could add titles to each panel. Also, the font size of all the labels are small.

Figure 4 is particularly difficult to read. The coastline contours are not clear. Also, they authors could consider to exclude the high latitude region. The differences between each color interval are not clear enough. It is really difficult to tell the differences. For me, it is just a bunch of red/blue patches. Also, since it is for precipitation, blue (red) might mislead to wetter (drier) condition, so I would suggest to adjust the color bar.