Review of "Impact of Global Warming on Natural SST Variability from CCSM4 and Observations"

by Dong-Ping Wang, Dake Chen, and Minghua Zhang

Recommendation: Resubmission; the needed revisions are too extensive to recommend Major Revision, I am afraid.

The manuscript addresses an important climate science question pertaining to potential modulation of SST natural variability under anthropogenic warming, with ENSO being the prime analysis target. The analysis technique is reasonable but the analysis itself is weak, including the writing and discussion which sometimes lack rigor and specificity.

Major concerns

- The starting premise is that 'natural' and 'historical' climate model simulations are needed to extract the global warming induced change in ENSO's spatiotemporal structure (e.g., L127-129), because the model's environment can be controlled, and because of the short observational record (L54-55). Interestingly, the authors then proceed to draw important conclusions (e.g., L25-26) from comparison of the transition mode structure in models and observations.

- The observational analysis is based on the satellite-era (post-1982) SST record. Why so? I am not aware of a compelling case made for restricting SST analysis to this short period, especially when addressing questions related to detection of secular change or its influence. The longer post-IGY SST record would have been quite up to the task, for instance. A longer period analysis is warranted in my opinion for better definition of ENSO variability (canonical, non-canonical, and biennial components). Note 'canonical' ENSO variability refers to the component that has remained relatively invariant across the 20th century, while 'non-canonical' ENSO refers to the striking change in ENSO evolution after the 1976/77 climate shift (e.g., Guan and Nigam 2008); the non-canonical ENSO is closely related to the Pacific Meridional Mode of Chiang and Vimont. Interestingly, the present analysis is 'silent' on this issue: Isn't there some obligation to connect with existing literature on the well-documented/discussed changes in ENSO variability during the 20th century?

- The analysis is flawed when it seeks to attribute the ENSO-change to anthropogenic warming without *first* factoring for the potential modulation from decadal-multidecadal natural variability. After all, a PDO-related 1976/77 climate shift is widely noted in the literature.

- The manuscript needs to forge stronger connections with existing literature. Such connections enhance the credibility of the analysis and move science forward by identifying robust findings. For instance, there is no intercomparison with Guan and Nigam's results, who used the same analysis technique on the same data set!

Constructive remarks

A more reasonable strategy would be to inter-compare the spatiotemporal structure of ENSO and other 'natural' variability modes, including decadal-multidecadal ones, in the historical and nature runs of the leading IPCC models. This has not been done, but would be very helpful in benchmarking these models.

Specific concerns

- L42: Why not state linear trends in the recent and full century period? Specificity and precision is helpful
- L42-43: How do you know? From the variance explained by PCs, or comparison of standard deviation?
- L52: "El Nino has large natural variability." What are you trying to say? Please be more specific, and provide references to back up your statements.
- L54-58: Need a rewrite for more coherent presentation.
- L57: "modified atmospheric teleconnection" What is this? Needs elaboration and supporting references.
- L65: Don't we need some evaluative remarks on the spatiotemporal structure of SST variability in CCSM4? Readers would be interested in knowing why CCSM4 was chosen? How does it compare with the other AR5 simulations in generation of ENSO variability? Please comment and provide some rationale for your choice, with references. It would, of course, be very helpful if another model simulation was also examined, as the undertaken analysis is not very burdensome.
- L69-74: What about potential aliasing of decadal variability (as noted above)?
- L99-103: The primary analysis in Guan and Nigam (2008) was conducted with a 5-season (not 7-) window, and for the 1900-2007 period (not 1870-onward). There is nothing in the present analysis to show that inclusion of Pacific SST data southward of 20S and at the full 1-degree resolution made a difference.
- L102: Replace 'degraded' by 'coarser'.
- L108-109: The original 1-degree resolution is as such redundant; that is why Guan and Nigam used coarser resolution SST data.
- L159: Why not show the 3rd mode in the nature run; you have the space.
- L164: Plot a thin trend line, and state its slope in the legend or figure caption, including its uncertainty.
- L170: 'transition' mode is used for mode-2 above, so there is confusion here: What is mode-3 called? Please use the same names in figure labels too, to be reader friendly.
- L191: Is mode-2 biennial variability; modal evolution suggests as much.
- L206: ENSO's delayed influence in the Indian Ocean basin was, perhaps, first pointed out by Nigam and Shen (1993; Structure of Oceanic and Atmospheric Low-Frequency Variability over the Tropical Pacific and Indian Oceans. Part I: COADS Observations. *J. Climate*, **6**, 657–676.)
- L256: But what about small differences between the transition mode of the historical and nature runs? What does that say? Surprised there is no discussion of this in the manuscript.
- L261: SST observations also show a warming trend much like that in the model historical run! See Fig. 13.