

Interactive comment on “Response of global evaporation to major climate modes in historical and future CMIP5 simulations” by Thanh Le and Deg-Hyo Bae

Jasper Denissen (Referee)

jdenis@bgc-jena.mpg.de

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General comments

The authors discuss the causal effects of climate modes on past and future (global) evaporation using an ensemble of simulations. With a (relatively) simple metric the authors show whether changes in (land or ocean) evaporation are likely to be caused by climate modes. The authors have done an impressive literature study, which backs up their own data-rich analysis. The main conclusion is basically that individual climate modes have effects all over the globe. The potential of this paper lies in using such a wealth of data (causal effects of different climate modes on global evaporation and

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its consistency across an ensemble of climate model simulations). In the end, most of the results are presented separately (per climate mode). I would advise to synthesize all causal effects of different climate modes in one global figure, where the authors could show in which regions (past or future) which climate mode is dominant. The text abundantly mentions which areas are influenced most by which climate mode, but visualizing this in a figure would give a much better overview in my opinion.

Specific comments

- Line 43: *“Besides, evaluating the models’ consistency in reproducing the impacts of internal climate variability on evaporation is important for understanding the difference between models”*. I do think this is important, but hardly mentioned in the paper. This could be emphasized more (in the abstract/conclusions?).
- Line 54-55: Why are the starting/ending years ‘roughly’ and not exactly...?
- Line 59: From which climate model does ‘rlilpl’ come? Why only one member?
- Line 61-62: *“Most of climate models do not provide separately the data of evaporation from canopy (i.e. transpiration) and water evaporation from soil (i.e. evaporation).”* What does this mean for this research? I miss a connecting sentence here. Maybe append something like: “which complicates attributing changes in evaporation to canopy or soil related processes.”
- Line 65: “ . . . , CMIP5 data is useful. . . ” Why is the data useful? Because of the abundance of data based on climate models with slightly different assumptions?
- Line 73-74: Why focus on the tropical Pacific, tropical Indian and North Atlantic Oceans with the mentioned climate modes? Please motivate why these (and related climate modes) are in focus.
- Line 79: Please elaborate on what the order of the causal model means.

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- Line 90: What is it that makes the applied techniques necessarily robust?
- Line 100-102: “*Specifically, high agreement of climate models in teleconnection between ENSO and tropical ocean evaporation implies that models can simulate the impacts of ENSO on evaporation.*” Why does a high agreement between models necessarily signal a capability to simulate impacts of ENSO on evaporation?
- Line 105: Why focus on two significance levels and not just on one? If two significance levels are necessary, please indicate why.
- Line 145: Figure S3 and S4 do not show seasonal responses of evaporation to the IOD. Do you mean Figure S5 and S6?
- Line 170: The differences in Fig. 4 do not imply unique combinations: a difference of -0.1 can result from Granger causalities 0 and 0.1 or 0.9 and 1.0 . I would advise to test whether the difference is significant or not.
- Line 185-194: Are these results or do these belong in the discussion section?
- Line 209: In Figure 5, panels a and b can be combined, just as panels c and d to be able to be able to visually compare a bit better. The authors could even think of combining all the panels together, to see be able to compare easily between land and ocean, past and future (there are only 12 bars).
- Line 217-220: What is the added value of Figure S10 (showing Figure 5 with $p = 0.25$)? The conclusion remains the same and inferences of this figure aren't mentioned.
- Line 231: Same comment as for Figure 4: I would advise to test whether the means are significantly different or not.

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- Line 231-232: “*Specifically, the fraction area of Earth surface showing lower probability of ENSO effects for 2006-2100 period is approximately 52.9% (Figure 6a).*”
I highly doubt this conclusion. The authors should first assess whether the differences are significant or not and after that determine reassess the overall ENSO effects. 52.9% is just slightly higher than just assigning an de(in)crease of ENSO effects randomly (50%). This also goes for following similar conclusions.

Technical corrections

- Line 40: “. . . between individual climate modes”
- Line 56: “Using other data periods with similar lengths (i.e., 95 years) **do** not alter. . .”
- Line 61: “Most of **the** climate models. . .”
- Line 158: “. . .of NAO impacts. . .”
- Line 187: “. . .in agreement with previous studies. . .”
- Line 198: “in **the** Australian continent. . .”
- Line 229-230: “. . .for **the** future period 2006-2100 and **the** historical period 1906-2000. . .”
- Line 264: “. . . for a short term. . .”

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