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Le *et al.* (2019): Response of global evaporation to major climate modes in historical and future CMIP5 simulations

GENERAL COMMENTS

This paper explores the historical and future impact of three major modes of internal climate variability on evaporation from oceans and land into the atmosphere based on data from CMIP5 model simulations and a Granger causality framework. Such an analysis might provide useful insights about the distribution of water resources in the near future and to help better forecast extreme hydrological events. As such, I truly see scientific value in this study; however, in my opinion, the present paper first needs to be improved in two ways: (1) the description of the method needs to be improved and more details are necessary to allow the reader to fully understand the work flow and (2) the results should be better interpreted and discussed in a physical manner to make them worth publishing. Below, I list some more specific comments and suggestions.

SPECIFIC COMMENTS

1. Section 2.1 needs some better motivation for some choices:
    a. Why has RCP 8.5 been chosen? This needs some motivation.
    b. Why is only data from 1906–2000 used for the historical period?
    c. Why is only one ensemble member per model used (r1i1p1)? I think the analysis might be more robust when an ensemble of model outputs is used.

2. Section 2.2 needs to be improved to fully understand the workflow:
    a. It needs to be clear from this section how the authors will deal with the model output from the models listed in Table S1. Will the authors average everything out or separately perform the analysis at every single model and compare the results to each other? Now, this is only clear from the figure captions.
    b. How do the authors deal with different spatial resolutions of the model outputs?
    c. At Line 78, the authors mention the temporal resolution of the analysis; but it is not clear when and why both annual and monthly aggregations are used. In addition, differences in the results from these two experiments are not properly addressed in the paper.
    d. Line 80: how is the optimal order of the regression model determined? Is this order different for every grid cell or the same across the globe?
    e. Line 86: how are the data normalized and de-trended? Why are the data de-trended?
    f. Given the importance of the Granger causality framework for this work, I think it is necessary to at least summarise it in this section. At this point, the reader is simply directed to literature.
    g. It has been shown that modes of internal climate variability might be significantly correlated with each other and that this correlation needs to be taken into account to properly analyse their effect on other variables (see e.g. Martens *et al.* (2018) or Gonsamo *et al.* (2016)). Also IOD and ENSO are correlated (see e.g. Figure S17 in Martens *et al.* (2018)). It is not clear to me how this is achieved by using the model described in Equation 1.
    h. How did the authors check the validity of Equation 1? Are the fitted models tested for significance?

3. As the authors correctly point out near the end of Section 2.1, several issues arise when using output from climate models. Both the modelled evaporation and the calculated climate indices are uncertain, and it is unclear to which extent this affects the analysis in the paper. I understand that the authors somehow try to tackle this by relying on the output from different models; but I think too little attention is given to this issue in the paper. I would at least expect a brief discussion about the possible uncertainties in the analysis: how reliable are the derived climate indices used to describe the IOD, ENSO, and NAO? The authors could for instance benchmark them against observed indices. How reliable is the evaporation in the models? Again, this can be done by benchmarking against in situ observations. Alternatively, the authors could discuss the uncertainties based on existing literature to put their results in context:

e.g. in which regions are the results presumably less reliable due to uncertainties in evaporation or internal climate variability?

4. The impact of IOD on evaporation over land is surprisingly very low; although it has been shown in several publications that the IOD is significantly affecting the surface hydrology; e.g. in Australia. How do the authors explain this low impact found in their study?

5. One of the main advantages of using output from climate models is the availability of surface and atmospheric variables driving evaporation, all linked by the model in a physical manner. As such, the observed patterns described in Section 3 can be better explained from a physical point of view in my opinion. Why are certain links between evaporation and the climate modes found (or not found) in specific regions? Most of the discussion is relatively speculative at the moment, while I think it should be feasible to explain the observed patterns by some additional analyses. Speculative sentences like "*… the influence of ENSO on evaporation might be associated with Wind-Evaporation SST*" (P4-L108), "*In the Northern Hemisphere, this result might be due to decrease in solar radiation*." (P6-L165), "*This increase in ENSO impacts might be related to the increase …*" (P6-L177), or "*There are different factors that might contribute to the ambiguity of climate mode impacts on evaporation …*" (P7-L204) could be better answered, by also analysing the effect of the modes on other model variables.

6. The statements at P8-L237-238 and P9-L258-260 are confusing. Modes of climate variability affect surface meteorological variables that drive the evaporation process like precipitation, wind, and air temperature, which, in turn, affect evaporation. The fact that no clear link can be found between evaporation dynamics and the modes of climate variability does not necessarily mean that these drivers are more important to explain variability in evaporation, but rather indicates that the drivers are not affected by the modes of climate variability in the models.

7. I am a bit surprised that there is generally little difference between the results for the future and historical periods. Several studies have shown that the modes of climate variability analysed in this paper are affected by climate change, and that (e.g.) more extreme states of these modes are expected (this is also acknowledged in the paper several times). How do the authors explain this small difference?

TECHNICAL CORRECTIONS

1. P1-L28-29: "*… and are likely to have impacts on global evaporation and transpiration …*": it should be explained why this is expected, or the statement should be backed-up with references.

2. P2-L31-32: It is unclear what is meant by this statement. I think "indicator" is simply the wrong choice of word here; else, the authors need to add which aspect of e.g. the global water cycle is "indicated" by evaporation.

3. P2-L40: References should be given here to make clear about which "previous works" the authors are talking.

4. P2-L36-48: Please note that Martens *et al.* (2018) preformed a comprehensive analysis of the impact of 16 major modes (including the ones tested here) of climate variability on terrestrial evaporation. Although the paper is cited in the results section, I think it is fair to cite it here as well.

5. P3-L61-62: The importance of this statement for the paper is not clear.

6. P4-L98: Indian Oceans → Indian Ocean

7. Why do the authors use the probability of the absence of Granger causality, rather than the presence? To me this is rather confusing, especially when looking at figures. Also the discussion of Figure 5 at page 7 is complicated by this, I think.

8. Regarding Figures 1–4 and Figure 6:
    a. I would like to advice to use a different color map. The use of a "rainbow" color map is misleading and should be avoided (I encourage the authors to google this and find out the reasons).
    b. The labels indicating 60 and 90 degrees latitude (both south and north) overlap with the map.
    c. For the contours, I would use a color not used in the color map.
    d. The symbols used to indicate the lines of equal latitude and longitude should be different from the dot used to indicate the agreement between models. I would simply not plot the parallels and meridians to make the figures less busy.

9. Regarding Figure 5: this figure is currently useless in providing information on differences between IOD and NAO. Would it be possible to plot these on the right y-axis with a different scale?

REFERENCES

Gonsamo, A., Chen, J. M., Lombardozzi, D. Global vegetation productivity response to climatic oscillations during the satellite era. Global Change Biology 22 (10), 3414–3426 (2016).

Martens, B., Waegeman, W., Dorigo, W., Verhoest, N., Miralles, D. Terrestrial evaporation response to modes of climate variability. npj Climate and Atmospheric Science 1 (1), 43 (2018).

Brecht Martens
Ghent University
Laboratory of Hydrology and Water Management