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Climate of the Past
Discussions

Interactive comment on "Dynamical and hydrological changes in climate simulations of the last millennium" *by* Pedro José Roldán-Gómez et al.

Anonymous Referee #1

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In this manuscript the authors analyse in a multi-model framework the typical responses of different key climate variables to the changes in the radiative forcing that occurred during the last millennium. This is nicely done by concatenating a large set of CMIP5/PMIP3 simulations, and computing the leading EOFs for several variables that describe different thermal, dynamical and hydrological aspects of the climate system. Since all concatenated simulations are driven with past estimates of the external radiative forcings, which synchronise some of the climate excursions across the different experiments, the EOFs extracted from the ensemble tend to successfully represent the common forced signal to all simulations. The analysis explores separately the long-term responses due to both anthropogenic and natural radiative forcing factors, as well

as the short-term impact of the largest volcanic eruptions.

I find the multi-model approach to be original and insightful, and the results of great interest. I thus recommend a minor revision of the manuscript, and enclose a list of comments that the authors would need to address to render the article suitable for publication in Climate of the Past.

General Comments:

1. The article is rather lengthy, and some parts feel repetitive. It would certainly benefit from some synthesis effort, so that the key messages are not obscured by the details. Some Figures could be removed, and their specific discussion in the main text shortened. For example, the most important changes in the hydrological cycle could be well described with just two variables: precipitation and the drought severity index. The P-E patterns are really close to those of precipitation (suggesting that precipitation is the dominant contributor to the surface freshwater fluxes over the continents). And soil moisture, as the authors already acknowledge in the paper, is not the most appropriate variable for inter-model comparison because different models compute it differently. And besides, it does not show a clear significant response to the forcings.

2. The global patterns of response (both the EOF and MCA-LIA composites) are beparticularly useful, as they help to easily identify the regions with the largest responses. But not so much the analyses based on zonal averages of dynamical and hydrological variables (Figures 8a and 11c), for which many of the regional features of the response are smoothed out. Indeed, it would be more interesting to address directly the response of the key indices that control these regional changes (ENSO/PDO, NAO, SAM,...). Plotting their associated time series, like in Fig 13, would allow to see how robust their forced signals across simulations are.

3. The volcanic impact analysis has also room for improvement. On one hand, it is currently focused on the global mean response, which makes sense for temperature (a variable that responds directly to changes in the radiative forcing), but not so much for

the dynamical and hydrological variables, whose response is, as I already mentioned, more regional. Focusing the plots on the regions with the largest response, as identified by the EOF or MCA-LIA composites, would help to identify stronger and more persistent influences of the volcanic eruptions to those in the global means. On the other hand, the current volcanic analysis is missing some estimate of statistical significance, which is essential to identify whether those responses are indeed meaningful. This could be done with a bootstrap approach that scans the periods with no volcanic eruptions to establish the significance threshold.

Specific comments:

- Page 2 Line 2: responses → changes

- Page 2 Line 5: consistently → consistent

- Page 3 Line 1: also have been → have also been

- Page 3 Line 10: the CMIP5/PMIP3

- Page 3 Lines 14-15: ", the Meteorological. . ., and with 13" → "and the Meteorological. . ., and 13l"

- Page 4 Figure 1 caption/Page 5 Line 10: composing → aggregating

- Page 5 Lines 28-30: The phrasing is confusing. I didn't really understand how it's done until I saw Figure 2d. The sentence suggests that compositing (or averaging) is not done with the five years before and 10 years after the volcanos, but it is instead done over the 12 main volcanic eruptions. And this is done for every year from the 5 preceding to the 10 following those volcanic eruptions.

- Page 5 Lines 31-33: Could you explain why is Gao's forcing used in some comes, and Crowley and Unterman's in others?

- Page 6 Table 2 Caption: temperature → surface temperature; of each → for each

- Page 6 Lines 1-3: Could you clarify if to make the multi-model concatenated array in which the EOF's are computed you first regrid all the experiments to a common grid? And to which one in that case? Otherwise the EOF array would be irregular in time. Or have the simulations been concatenated in space?

- Page 6 Line 4: Do you really apply an average? Or is it simply the EOF of the concatenated simulations? If there is no averaging it is better to refer to it as the "multi-model EOF".

- Page 7 Line 4: It would be more clear if you change "resulting EOFs" for "single experiment EOFs". Also, to prove that the differences are minor, you could compute the spatial pattern correlations between the individual EOFs and the multi-model one, and provide the range of correlation values in the text.

- Page 8 Figure 3 Caption: Also for clarity I would change "a PC analysis" → "the multi-model EOF analysis"

- Page 8 Lines 9-17: It feels weird to start your result section with a paragraph revising previous results. That's what the introduction is for. Previous results can also be discussed in the results section as well, but to contrast with your findings once they have been introduced. I strongly recommend to start directly with the second paragraph.

- Page 9 Lines 11-12: Not sure I agree. There are still important differences across members with the same model, which are hard to discern given the high line density in Figure 1c. To compare appropriately the forced vs internally driven temperature changes you would need, for a specific model ensemble, to compute the ensemble mean (which would describe the forced signal) and remove it from each of the individual members (to extract its internal variability component). I expect that many centennial changes will be of similar magnitude than the MCA-LIA transition. The exception should be the industrial warming trend, which will most probably remain unparalleled.

- Page 9 Lines 27-29: I suggest rephrasing the second sentence to make clear that polar amplification is characteristic of the sea ice covered regions (via ocean/sea ice albedo feedbacks, among other processes) but not of the continental areas.

- Page 9 Lines 32-34: There is an important qualitative difference between Figure 2a,c that the authors do not comment. In the EOF, there is a stronger response in the Tropics than in the subtropics, that does not occur during the MCA-LIA transition. Could the authors discuss it, and the potential reasons?

- Page 10 Lines 7-8: Same as before. You start a subsection of results describing previous literature. Also, please note that the two modes of internal variability that you mention explicitly (ENSO and PDO) are coupled modes that involve the ocean, and therefore only partly related to atmospheric dynamics. It would make more sense to put forward the NAO, which is purely atmospheric and has been studied during the last millennium with different proxy reconstructions.

- Page 11 Lines 5-6: Could you specify what you mean by long term behaviour? The first PCs of SLP are basically characterised by a flat line and a positive trend starting in 1700. By contrast, the respective ones for surface temperature include strong multi-centennial oscillations, which for some models are of similar magnitude than the industrial warming trend.

- Page 11 Line 6-7: I wonder if the MCA-LIA difference that can be seen in Figure 4b is really significant. It does not seem to occur consistently for all the models. Indeed, another indication that the MCA-LIA difference is not a remarkable feature comes from the spread of correlations across model PCs in Figure 4b, which are only clearly above zero if the industrial era is considered.

- Page 11 Line 17: What do you mean by SLP stratification? Do you refer to the typical zonally-symmetric dipolar SLP response of SAM to global warming, with relative low surface pressure conditions at subpolar latitudes and high conditions at polar latitudes?

- Page 12 Line 3: There is not such a good similarity in the Southern Hemisphere.

Note for example that over Antarctica the response is of the opposite sign in the MCA-LIA pattern than in the EOF. age 12 Lines 4-5: The SAM intensifications/weakenings during the MCA/LIA are far from evident from Figure 4c. In particular, the significant response is not zonally-symmetric, and as mentioned before, Antarctica experiences a relative high during the MCA. If you really want to prove that MCA-LIA transition was accompanied by a weakening of the NAO/SAM, you should do show it with their respective indices.

- Page 13 Lines 32-34: There is no evident change from MCA to LIA in the PCs of the zonal wind. This implies that the EOF pattern mostly reflects the changes during the industrial period but not during the MCA.

- Page 13 Lines 34-35: To know if the EOF corresponds with a poleward displacement you need to show as well the mean zonal climatological winds. Otherwise, how can you tell that positive/negative loadings do not correspond to intensifications/weakenings of the climatological winds?

- Page 15 Lines 3-5: The spatial patterns in figure 7 show also important differences that should be acknowledged. For instance, in the North and Tropical Atlantic, or in the whole Pacific region.

- Page 18 Lines 9-10: Similar to the previous comment. In this case the response is really different in the Tropics.

- Page 18 Lines 16-17: I don't understand this statement. Figure 10 shows a positive response in North America, while climate projections suggest that the response is zero.

- Page 18 Line 17: Marine → Maritime

- Page 19 Line 2: You are not really showing consistency, just a multi-model response (which could be dominated by certain simulations/models)

- Page 20 Lines 1-2: As previously mentioned for the SLP patterns, shifts can only be diagnosed in relation to a climatological state, which has not been shown nor discussed.

- Page 20 Lines 4-5: The distribution is clearly centered at zero for all regions but EAS and SAS. For SAF there is a slight tendency to more positive values, but it could be happening by chance. A significance assessment would be helpful to draw more robust conclusions. You could, for instance, test if the median of the distribution is significantly different than zero.

- Page 20 Line 30: Strong statement. CCSM, HadCM , MRI and MPI don't really support this.

- Page 20 Line 35: There is no real agreement in the big picture in figure 12. Every model tends to have a different area of influence, which is particularly evident in the negative correlations.

- Page 24 Line 30: are → have

- Page 25 Lines 11-12: I find the phrasing of this sentence confusing. It's not clear if you refer to the covariability of all variables (including surface temperature) with the changes in the forcings or if you refer to the covariability between the PC related to the forcing of surface temperature, and the equivalent PCs for the other variables. I would simplify it just saying that "PC analysis was used to identify the multi-model typical pattern of response of different variables to the external forcing changes from decadal to multidecadal timescales"

- Page 26 Line 11: How can you tell that the hydrological is enhanced? Figures 14-17 simply show how the EOF of the forced modes of variability are, with regions of increased and regions of decreased precipitation.

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