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Earth System
Dynamics
Discussions

Interactive comment on "Regional scaling of annual mean precipitation and water availability with global temperature change" *by* Peter Greve et al.

Anonymous Referee #1

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This paper applies the regional scaling concept (e.g. Tebaldi and Arblaster, 2014) to the distributions of annual mean precipitation (P) and precipitation minus evaporation (P-E) anomalies. The rationale behind this methodology is based on the work by Seneviratne et al., 2016, in which it is shown that local temperature and precipitation extremes scale linearly with global mean temperatures in CMIP5 CO2-increasing scenarios projections. The mean and uncertainty ranges of P and P-E local responses scale with the global mean temperature in the same way, independently of the emission scenario. This paper extends Seneviratne et al., 2016, considering annual mean local precipitation response scaling with global mean temperatures over all the SREX regions (Seneviratne et al., 2012). The impacts of uncertainty due to models internal

variability, to the inter-model spread and to the scenario spread are also separately accounted for.

1 GENERAL COMMENTS

The aim of the paper is focused and clear, and it well suits in the current debate about the regional impact of global temperature change. Exploring the ranges of applicability of the pattern scaling approach allows to improve the capability to communicate the impact of climate change to the stakeholders and public opinion. In this sense the assessment of regional changes in P and P-E is of outmost importance for the adaptation to future changes in local water resources.

The regional pattern scaling is here assessed in terms of a basic least squares fit, regressing annual mean P and P-E anomalies over annual mean global temperature anomalies at every grid-point. The uncertainties related to internal variability, model and scenario-related uncertainties for each model are obtained by resampling the residuals 1000-times over each gridpoint. The empirical probability distribution provides thus a way to characterize the range encompassing the median values of the regression slopes, allowing the distinction between very likely (90-100%), likely (66-100%) increase/decrease in P or P-E. The use of a basic linear scaling is justified by the lack of a-priori information about the shape of the annual mean P and P-E distributions over the various regions is not available. This shifts the focus from the choice of suitable downscaling techniques to the evaluation of uncertainty ranges attributable to the regression coefficients. In this respect, I think that the manuscript partially fails in discussing the impact of models' choice. Seneviratne et al., 2016 outlined limitations to the regional scaling pattern approach in this context. Particularly, point 4) of their discussion emphasized the risk of common biases through models for some regional phenomena. They point out that a careful model evaluation against appropriate observations would be necessary to deal with this problem. The internal variability of considered model and the multi-model ensemble uncertainty is addressed in the manuscript, but some more effort should be devoted to the evaluation of each model. Particularly, the biases induced by the imbalance in the water mass budget and the impact of different choices of the model ensembles should be carefully addressed, in order to assess the applicability of the method and the robustness of the findings.

2 SPECIFIC COMMENTS

l. 22-23 p. 3: if not argued the choice of the models may look a bit arbitrary. On one hand the authors rely on Fischer et al., 2014 to choose only one model for each modelling centre. On the other hand they do not consider the impact of biases in the atmospheric moisture budget. Models are known to show diverse estimates of the global mean water budget (cfr. Liepert and Lo, 2012, Env. Res. Lett.) and this may in principle prevent from consistent estimates of regional changes in P and P-E. Evaluating the long-term mean atmospheric moisture budget in control runs, identifying the regions where climate models diverge from available observations is thus a pre-requisite to this analysis. An inconsistent global mean moisture budget is a potential source of biases and the impact of adding/removing individual models should be carefully evaluated. In the framework of the regional pattern scaling, it would also be relevant to compare the atmospheric moisture budget separately over continents and oceans with the total runoff from the continents (which is a standard output in climate models, if I am not wrong is named as “mrro”), in order to provide a complete description of the hydrological cycle consistency in the model;

l. 23-24 p. 3: as also mentioned in l. 23-24 p. 6 the choice of ensembles with different numerosity is inherently a considerable source of uncertainty, unless one considers the 14-member and 7 (in the case of RCP6.0) and 11 (in the case

of RCP2.6) members ensembles having the same statistical properties. For the same reasons motivating the previous comments, the impact of adding/removing a model from the ensembles should be carefully evaluated. To be on the safe side, I would suggest to reconsider the RCP2.6, RCP4.5 and RCP8.5 scenarios only using those models that are available in the RCP6.0 and discuss about the presence/absence of significance differences in the results. In the occurrence of significant differences I would try to identify and describe those models significantly reshaping the ensemble distribution;

l. 12-18 p. 4: the definition of variances might be clarified by labelling each sigma with a different subscript, either referring to internal variability, model uncertainty, scenario uncertainty;

l. 15 p. 4: following above comment, it should be specified how to deal with the model uncertainty when the ensemble numerosity is lower than 14, e.g. in the RCP6.0 n=7?

l. 30-31 p. 4: to me it is not clear how the authors deal with uncertainty ranges including the zero value for the slope. Could you please expand this statement?

l. 5-6 p. 5: the authors might want to comment on the fact that spatial averaging over northern high latitudes is not the same as spatial averaging at lower latitudes, and this shall be considered when discussing the significance of results at different latitudes. I wonder if one could compare circles of latitude somewhat weighting the likelihood of the changes with the cosine of latitude or the surface area covered by each circle.

l. 19-22 p. 6: the authors mention the different shapes of the uncertainty distributions for different SREX regions in P and P-E regression slopes. Could you please specify whether you refer to the P, P-E or both variables. Otherwise these statements appear a bit arbitrary and one might want to consider removing them;

l.1 p. 7 (and l. 25 p. 8): please specify the meaning of “significantly”;

l. 4-6 p. 7: the authors list here a number of SREX regions characterized by larger/smaller internal variability, model uncertainty, scenario uncertainty compared to other regions. I think some more explanation might be welcomed here, rather than just listing the findings over the various regions. Why these regions, rather than others? For instance, the large model uncertainty over northern high latitudes might be related to the more relevant signal (“very likely increase” in precipitation), whereas the large internal variability over the two sides of the Tropical-Northern Pacific might reflect some relatively well understood mechanisms of inter-annual variability, such as the QBO (cfr. Labat et al., 2004, Geophys. Res. Lett.);

l. 14-16 p. 7: repetition of l. 2-4 p. 3, consider removing;

3 TECHNICAL COMMENTS

l. 25 p. 3: remove one “in”;

l. 17-18 p. 4: replace “coefficient” with “coefficients”;

l.23 p. 6: replace “causes” with “cause”;

l. 14 p. 8: replace “extent” with “extend;

Table 2: the acronym for Northern Australia should be NAU (instead of NAS);

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