Earth System Dynamics Discussions

Earth Syst. Dynam. Discuss., author comment AC2
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Reply on RC2

Josep Cos et al.

Author comment on "The Mediterranean climate change hotspot in the CMIP5 and CMIP6 projections" by Josep Cos et al., Earth Syst. Dynam. Discuss., https://doi.org/10.5194/esd-2021-65-AC2, 2021

Dear Referee,

The authors want to thank you for your time and the revision of our manuscript. The comments will help us improve the content and robustness of our work. The most important message we extract from the review is that the methods followed to show precipitation projections should be clarified and modified to make our work more consistent.

Nevertheless, the authors have a slightly different point of view regarding some of the comments. Therefore, we would like to take advantage of the discussion period to have a conversation on some of the points (italics refer to your comments):

1. Regarding precipitation: The eastern Mediterranean is characterized by almost completely dry summers and precipitation mostly during winter. Therefore, including it in JJA in the precipitation calculation over the whole Mediterranean basin seems to me problematic. This is seen in the verification against observations for (Fig. S5), and in the lack of robustness and significance in changes (Figs.4). I strongly suggest that the authors do their calculations (not only verification, but all the calculations) considering the very significant differences in precipitation between different regions over the Mediterranean basin.

From our point of view, this shouldn't be an issue as the area aggregations we have applied are computed with the absolute rather than the relative values. Nevertheless, this was not the case for the MedHS scatter plots. We will redo them following your suggestion.

2. S5, at least the comparison to observations, should be part of the article body and not a supplement. This is part of the heart of the paper: how to quantify the veracity of the simulations and the results seen in this figure are strongly correlated to the remark #1.

Thanks for the suggestion, we agree that the comparison to observations should be

displayed in the body of the article. We will move the historical trend comparison to the main text.

3. *Due to the complexity of the precipitation analysis I would also suggest limiting the manuscript to temperature only, without the need of additional calculations for precipitation as in remark #1. For instance, as stated in lines 365-366 "Precipitation weighted projections are not shown in this study as we have no proof that the diagnostics used to assess temperature are relevant to evaluate the models precipitation response." Diagnostics should turn clearer by dividing the Mediterranean basin following precipitation climatic characteristics in the different seasons.*

From the authors' understanding of the hotspot, it is important to include the effect of precipitation. Regarding the weighting method and precipitation, it is an issue that is yet to be resolved as diagnostics used to constrain temperature are inadequate for precipitation. We plan to further investigate this issue in the future.

The authors think that splitting the Mediterranean domain escapes the aim of assessing the region as a whole and as defined by the IPCC. Going back to our answer to point 1, the area-averaged relative changes have been aggregated from the single grid-points absolute values, and therefore, results shouldn't be affected by large relative changes in arid regions.