

Earth Syst. Dynam. Discuss., author comment AC1  
<https://doi.org/10.5194/esd-2021-65-AC1>, 2021  
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## Reply on RC1

Josep Cos et al.

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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-AC1>, 2021

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Dear Referee,

Thanks a lot for the thorough revision of our manuscript as the majority of comments will help us improve the content and readability of our work. The most important message we extract from the revision is that the way the methodology and results are written could be improved (comments 1 & 2) and, therefore, we will apply the suggestions. Additionally, as suggested, we will add some discussion regarding the underlying mechanisms of the Mediterranean hotspot.

There are some methodological issues pointed out by the reviewer that arise from the authors not having successfully conveyed the methods in the text. Therefore, we proceed to clarify these issues and they will be kept in mind when re-writing part of the text (italics refer to your comments):

*... Additionally, in one sentence the diagnostics are said to be "the 20-year PSL and TAS climatologies" and in the next you say that the diagnostics are computed over the 35-year period. Two different time periods are also used for  $D_i$  and  $S_{ij}$ , which is confusing (see also my next comment) Also, what observational reference do you choose (in DIFF)? The mean calculated over all observation/reanalysis products?*

We always use the 35-year period in the diagnostics used to compute  $S_{ij}$  and  $D_i$ . The 20-year periods are used only when showing the projected results. Therefore, the two periods aren't mixed within the weighting method. What might have caused some confusion is that the periods 2041-2060 and 2081-2100 are used when finding the magnitude of the optimal shape parameter  $\sigma_d$  (we need to check how the multi-model ensemble reacts to different values of  $\sigma_d$  to assess if it would cause an under or over-constraint of the future projections).

*Baseline periods. The fact that you use two different periods is confusing. First, 20 years is a bit short to calculate averages. 30 years is usually preferred. You mention that 20 years of data are heavily influenced by inter-annual variability; that is true for trends, but for averages also. The extra 10 years of observations should also be used to assess GCM performance. Since you have to merge the historical and RCP8.5 simulations in CMIP5 to calculate trends for the 1980-2014 period, why not merge them to calculate averages also? The issue with having those two reference periods is that you mix them in the calculation of weights, which is not very consistent.*

The fact that we used 20 year periods to show mean changes is consistent with the work conducted in the IPCC AR6. As answered in the previous point the weighting method, the verification of the models against observations and the display of the future projections are independent things. For the weights we use the 35-year historical period to ensure that the trend is well represented, the same is true for the verification with observations and finally, when showing the future projected changes we follow the IPCC guidelines to display changes against the baseline period.

*Discussion. Despite the emphasis on the "hotspot" aspect, the discussion contains no information on the physical mechanisms responsible for the existence of the Mediterranean hotspot. Some literature exists on the topic (e.g., Brogli et al. <https://doi.org/10.1175/JCLI-D-18-0431.1>, Tuel et al. <https://doi.org/10.1175/JCLI-D-20-0429.1>) Please consider adding a short discussion on the comparison of the hotspot between CMIP5 and CMIP6, and the links to the known/likely physical mechanisms.*

Thanks a lot for the suggestion, we will consider adding information about the mechanisms that drive the hotspot in the revised manuscript's discussion.

Finally, there are a couple of suggestions/comments where the authors' points of view differ from the reviewer's:

*Trend significance. It seems that to detect statistical significance the authors are implementing a t-test to determine whether the ensemble-mean average trend (in TAS or PR) is significantly different from zero. But that is not really appropriate. Trend statistical significance should be assessed for each model separately based on its inter-annual variability. The spread in trend values across models is not related to the magnitude of the trends themselves. For instance, there can be a large spread across models (+1, +2, +5, +10°C) but each trend may be statistically significant for the corresponding model (because inter-annual variability is smaller for the +1°C model than for the +10°C model). A better definition of significance in this context might be the fraction of models for which the trend is significant (or, like robustness, to impose that the trend is significant for at least 80% of models) This could change the conclusions for HighResMIP*

While this is an interesting approach to compute the statistical significance of the results, we consider that it wouldn't correspond to the significance of the results as we show them. The results are obtained using the change in the multi-model ensemble mean. To evaluate if the changes are significant we need information about the difference in ensemble distribution between the historical and future periods, not the significance in the changes of single models. Therefore, we compare the ensemble-mean and model spread between the historical and future periods.

- *1.167 "30-45N latitudinal belt mean" -> Why not all land regions? One could argue that to make it a global hotspot one should compare against all other land areas (say of the same size). One issue also is that both the Mediterranean and the 30-45N belt contain many grid points with very small precipitation averages -> potentially large relative changes which may bias the analysis.*

We tried several options to illustrate the precipitation hotspot. We compared the Mediterranean precipitation to the global precipitation and the precipitation in the 30-45°N latitudinal belt to reach the same conclusion: the Mediterranean is projected to experience larger changes than the global mean and the regions with the same latitude. This already stands out when looking at the global maps of precipitation change. Using land-only points in the latitudinal belt does not change the conclusions because many land regions in these latitudes experience important precipitation increases (e.g., South Asia).