Authors' response to RC4

Black text: Referee comment

Blue text: Authors' response

The manuscript analyses the relative importance of global atmospheric CO_2 concentration and local soil moisture for the number of hot days of the hottest month (NHD). Much of the manuscript is focused on the analysis of the relationship between decomposed GRACE TWS and NHD. I agree that this is an interesting and novel aspect at the core of the manuscript, which should potentially be in the title (e. g. Relevance of TWS deficit and increased atmospheric CO2 for hot extremes).

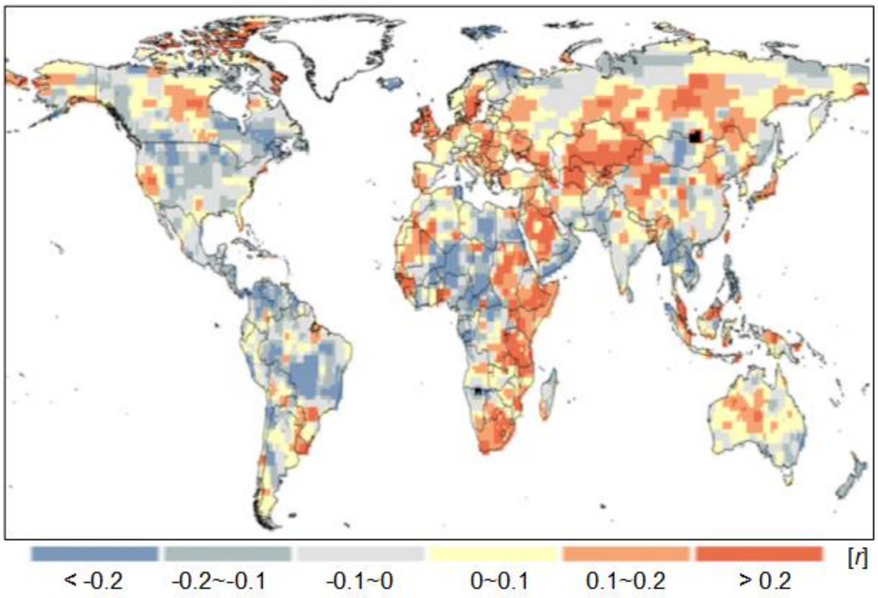
Overall, the manuscript is well-structured and clear. Nevertheless, it would be useful to further clarify and improve some aspects of the methodology and presentation of the results. It doesn't seem like the best approach to focus on a direct comparison of moisture deficit and increasing CO2, given that changes in moisture deficit are also influenced by increasing CO2. Also, the computed correlations are taking into account both the trend and interannual variability. Looking separately at the trend and at the detrended interannual variability could be a good option. My hypothesis is that the trend in increasing CO2 correlates better with the trend in NHD, whereas the detrended interannual variability in NHD is clearly more related to the detrended interannual variability in local moisture deficit (the detrended variability in CO2 is likely very small). This can provide helpful context to better understand statements such as the conclusion in lines 180-182. One should not interpret these results as increasing CO2 is not too important for hot extremes; the fact that the correlation is not very high is likely because the detrended variability in CO2 is likely rather small and the study period relatively short.

We greatly appreciate your constructive comments for improving our manuscript, and your suggestion of an alternative title. Physically, it is root-accessible moisture that influences near surface air temperature. The wavelet decomposed TWS (raw TWS includes surface water, soil moisture, groundwater, ice, and snow) is applied here as one proxy of soil moisture. From this point of view, we believe the original title is more appropriate.

We agree with you that the impact of CO_2 concentration is more likely on the trend of NHD, while the soil moisture is on the interannual variability of detrended NHD. Dominance analysis we adopted in this study is able to quantify their relative importance in influencing the occurrence of hot extremes, even if the two predictor variables are correlated (detailed in our reply to your comment 2).

To address your concern that changes in moisture deficit might be influenced by increasing CO_2, we have examined the correlation between TWS and CO_2 in the figure shown below. Result shows that the two variables are mostly independent as there are very few grid cells (marked in black) having p values no larger than 0.05.

Our analysis is based on (reconstructed) TWS, thus it only covers a short period (1985-2015). We agree with you that it is very likely that over a longer period, the relative importance of CO_2 may increase against soil moisture deficit. This can be examined when more TWS data become available in the near future. To make this message clear, we stated in the manuscript that "We note that under continuing increase of greenhouse gas forcing, hot extremes are expected to be dominated by increased CO_2 concentration over larger areas in the future. Hence, global measures for reducing emissions are essential in combating current and future expansion of hot extremes."



Correlation coefficient (*r*) between raw TWS and CO_2. *p* values of grid cells marked in black are smaller than 0.05.

Specific comments

1. In the first paragraph of the introduction, it could also be noted that "local moisture deficit" and "increased atmospheric CO2 concentration" are not fully independent. A recent study attributed the observed pattern of intensification of the dry season to human-induced climate change (mainly corresponding to increasing CO2) (Padrón et al., 2020).

Padrón, R.S., Gudmundsson, L., Decharme, B. et al. Observed changes in dry-season water availability attributed to human-induced climate change. Nat. Geosci. 13, 477–481 (2020). https://doi.org/10.1038/s41561-020-0594-1

This is an interesting and relevant study to our manuscript. It is not surprising that the two variables have some correlation. This is why we adopted the dominance analysis in our study. Following your earlier comment, we did look at the correlation of the two variables over the study period (1985-2015). The TWS and CO_2 is shown mostly independent. This seems to be inconsistent with what is shown in Padron et al (2020). Here are a few possible reasons to explain this apparent inconsistency: (1) Padron et al., (2020) identified dry-season water availability as annual minimum monthly P-ET where the dry-season is not always consistent with the hottest month identified in our study; (2) P-ET is not the same as soil moisture; (3) Padron et al., (2020) is based on data in 1902-2014 while our study focuses on the period of 1985-2015.

We will include Padron et al 2020 in the introduction section so that readers are made aware of this relevant research.

2. Section 2.2. I would argue against a focus on statistical significance when presenting the results (see Amrhein et al., 2019). See specific suggestion in comment 7. Also, please clarify the maximum allowed complexity of the regression models when doing the stepwise multiple linear regression; are interaction terms included? Which are "all the possible subset regression models"? I would encourage the authors to include an example of the dominance analysis for a specific grid cell either in section 3.3 or in the supplement.

Amrhein, V., Greenland, S., and McShane, B. Scientists rise up against statistical significance. Nature. 567, 305–307 (2019). https://doi.org/10.1038/d41586-019-00857-9

Thanks for recommending the useful reference. We agree to redraw Fig. 6 by using correlation coefficients instead of *p* values. The new figure is shown in the reply to your comment 7.

There are 6 predictor variables (D1, D2, D3, D4, A4 and CO_2) included in the stepwise regression for each grid cell. All wavelet decomposed TWS series (D1-D4 & A4) are independent. TWS and CO_2 are also mostly independent as we replied above. In fact, even if those input variables are not independent, the dominance analysis can distinguish their relative importance as described in Azen and Budescu (2003) (https://doi.org/10.1037/1082-989X.8.2.129). We admit that the description of statistical analysis method especially the dominance analysis in the original manuscript is not clear enough. We will improve it in the next revision as follows:

"Stepwise multiple linear regression (Draper and Smith, 1998; Clow, 2010) is used to determine the significant (5% significance level assessed by an F-test) predictor variable in explaining NHD temporal variability for each grid cell. Next, the dominance analysis approach (Budescu, 1993; Azen and Budescu, 2003) is applied to compare the relative importance of those selected variables. The total variance among a set of predictors can be fully partitioned by dominance analysis even if the predictors are correlated (Vize et al., 2019). The issue of collinearity among predictors is addressed by examining the unique variance accounted for by the predictor across all possible regression sub-models involving the predictor. Dominance analysis is completed through an exhaustive set of pairwise comparisons among the predictors. The comparisons can be examined by three types of dominance: complete dominance, conditional dominance, and general dominance (Nimon & Oswald, 2013). To be completely dominant, a predictor must account for a greater amount of outcome variance than another predictor across every sub-model comparison. The conditional dominance of different predictors is conditional on what sub-model level is being examined. We applied the general dominance in this study, which is determined by taking the average amount of variance accounted for by a predictor across all sub-models and comparing it to other predictors. General dominance weights can be calculated for each predictor in a set and represent the relative proportion of R^2 attributable to a predictor."

As for your requirement to provide an example of dominance analysis for a specific grid cell, we would like to explain that we will provide more description of the method in the revised manuscript (the corresponding text is shown above). We hope that the new description of our analysis process is clear. On the other hand, the dominance analysis code is provided by the authors who proposed this method, which can be accessed from Azen and Budescu, (2003) (https://doi.org/10.1037/1082-989X.8.2.129).

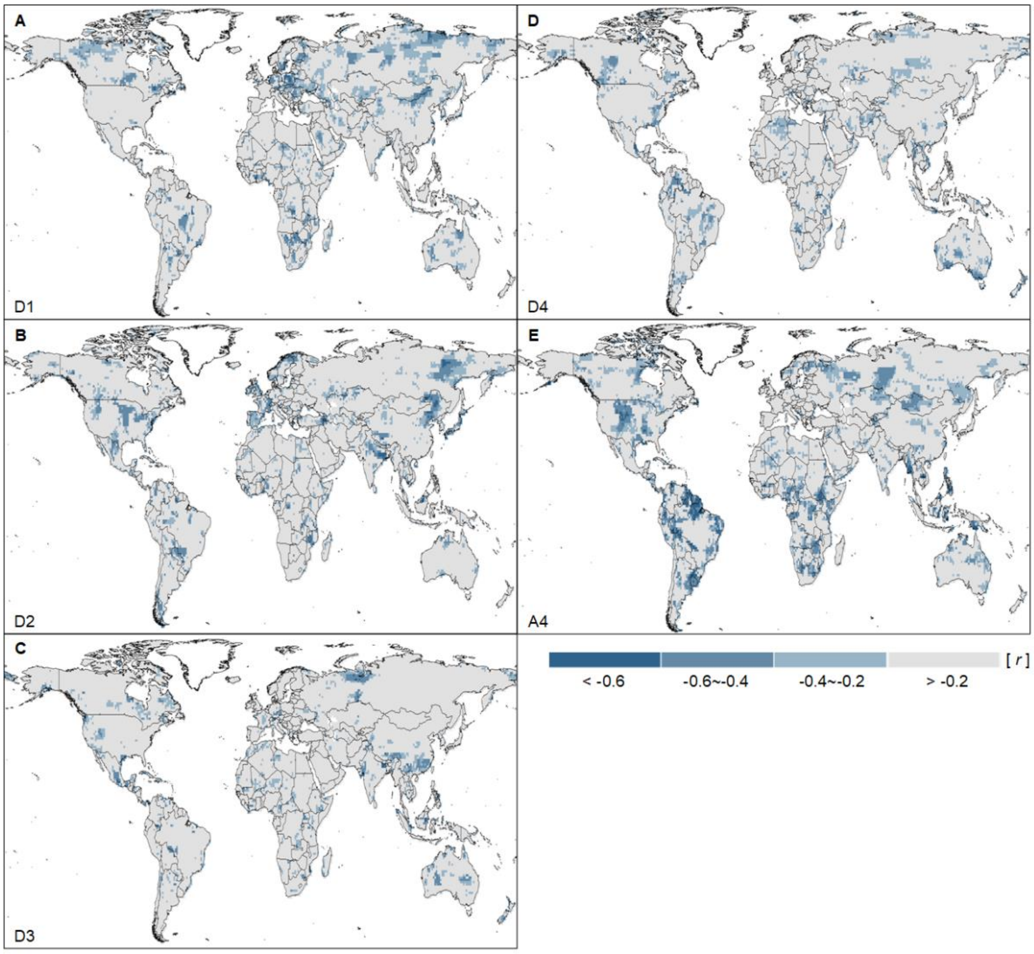
3. Section 3.1 seems a bit short. Here it could be useful to mention possible confounding effects, for example, the positive correlation in Brazil between CO2 and NHD in Fig. 3b, could result from a higher NHD driven by changes in land cover (deforestation), which also coincide with the increasing trend in CO2.

We agree that more discussion on the results shown in Fig. 3 should be provided in Section 3.1. We will add the following discussion into the revised manuscript: "The effects of increased atmospheric CO_2 concentration on the occurrence of hot extremes are relevant for particular topographic and climatic conditions. For example, significant correlations are observed in extreme dry regions (e. g., the Sahara), mountain ranges (e. g., the Andes in South America), and plateau sections (e. g., the Mongolian Plateau and the Tibet Plateau)."

We will also mention possible confounding effects as you suggested: "Since deforestation can effect surface energy partitioning by reducing evapotranspiration, positive CO_2-NHD relationship in some areas of Southeast Asia and Brazil (Hansen et al., 2013) could result from the coincidence of deforestation-induced higher NHD and increasing CO_2 concentration." We appreciate that you brought this possible mechanism to our attention, which we didn't realize.

4. In Fig. 4d clarify which sub-component of TWS is used. If it is a different subcomponent for every grid cell it is perhaps useful to have a map in the supplement.

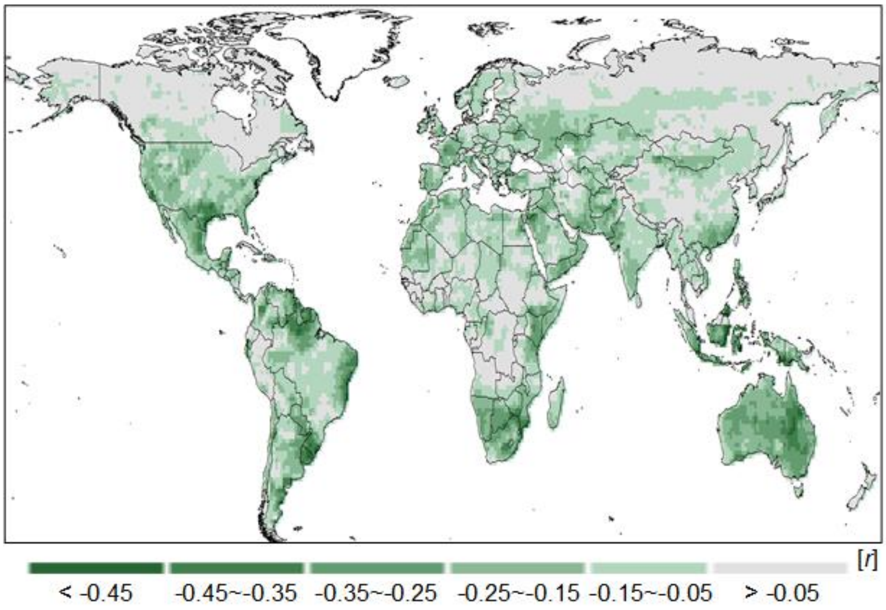
We will add a map showing which sub-component of TWS is used in Fig. 4d in the supplement as you suggested. The new figure is shown below:



Correlation between NHD and decomposed TWS components: (A) NHD-D1; (B) NHD-D2; (C) NHD-D3; (D) NHD-D4; (E) NHD-A4

5. Lines 133-135. Expand or provide more evidence against the potential confounding effect of both soil moisture decreasing and NHD increasing as a result of higher incoming radiation (clear sky days). It may not necessarily be the case in all identified regions that lower soil moisture is exacerbating an increase in NHD. An additional analysis of the correlation between soil moisture and the evaporative fraction (i.e. latent heat / net radiation, these variables are also likely available in the employed reanalysis product) could clarify if there is indeed an effect of soil moisture limitation resulting on a higher partitioning towards sensible heat flux and therefore increased NHD.

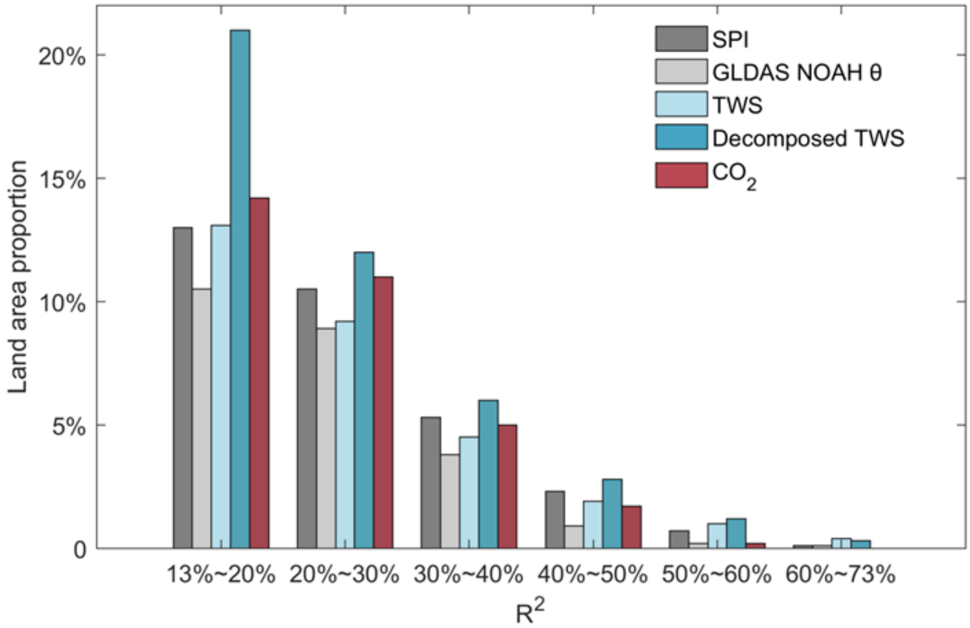
Thanks for providing the idea that analyzing the correlation between soil moisture and the evaporative fraction to clarify the effect of soil moisture on surface energy partitioning. We now completed the analysis as you suggested (see the figure shown below). Regions with relatively higher TWS-EF correlation are generally consistent with those identified moisture deficit dominant regions shown in Fig. 7, such as the US, South Africa, and Australia. We will add the additional analysis you suggested into the revised manuscript to improve the discussion on causal relationship between soil moisture deficit hot extremes.



Correlation between TWS and the evaporative fraction.

6. Fig. 5 could benefit from also including the R2 for CO2 concentration.

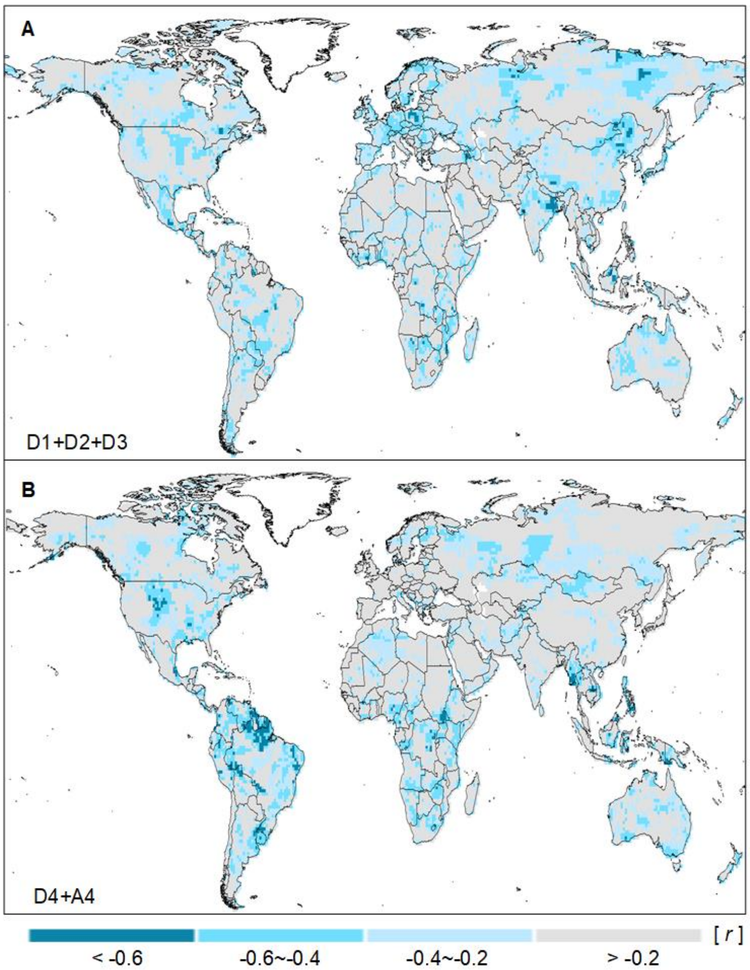
We agree to follow your suggestion to include R^2 for CO_2 in Fig. 5 in the revised manuscript. The new figure is shown below.



Histogram of the explanatory power (significant regression R^2) on NHD variability by using SPI, GLDAS_NOAH θ, raw TWS, decomposed TWS and atmospheric CO_2 concentration during 1985-2015.

7. It would be better to show the actual correlation values in Fig. 6. A two-panel figure with one map for shallow and one for deep soil depth could be a good option. Hatching could indicate which case has higher correlation.

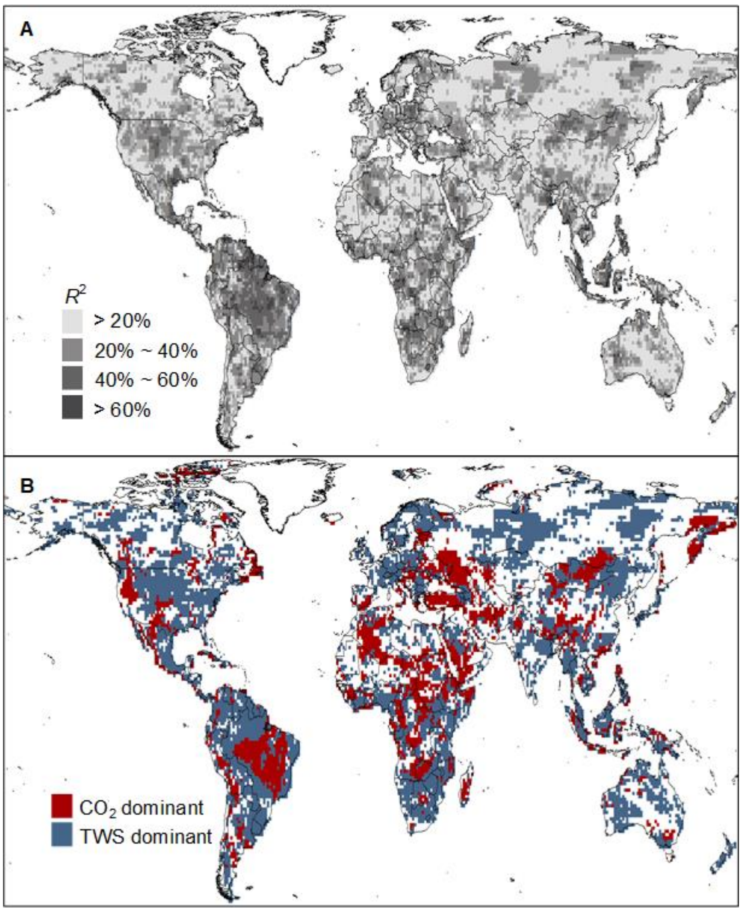
Fig. 6 has been modified as you suggested. Correlation between NHD versus shallow (D1-D3) and deep (D4 and A4) soil moisture are shown in two panels. The corresponding description and discussion will be modified as: "Compared to D4+A4, D1-D3 shows higher correlation with NHD in parts of Asia and Europe, those regions are reported to have relatively shallower plant rooting depth (Fan et al., 2017). The central part of North America, the northeastern part of South America and the northwestern part of Southeast Asia are reported to have deeper plant rooting depths (Fan et al., 2017), where interannual variability (D4 and A4) of TWS seems to be more important than its seasonal variability (D1-D3) in explaining NHD temporal variability. This implies that plant water uptake from deeper soil plays an important role in θ-NHD coupling. However, D4+A4 also show stronger correlation than D1+D2+D3 with NHD in areas without deep roots, including the northern and southeastern parts of South America, parts of the Southeast Asia. This is because those regions have shallow groundwater table depth (Fan et al., 2013). It implies that in areas where groundwater is shallow, groundwater dependent ecosystems may contribute to heat mitigation, which is worthy of future investigation."



Correlation between NHD versus shallow (D1-D3) and deep (D4 and A4) soil moisture.

8. Interpretation of Fig. 7 may also benefit from having two panels. One indicating only the total explanatory power of the joint influence, and another differentiating the regions where either factor is deemed more important. I understand this is a matter of personal taste.

We have modified Fig. 7 as you suggested which is shown below. However, we think the original one seems to be more concise because all information can be shown in only one figure. We would like to explain that throughout the manuscript we use blue and red to represent moisture and CO_2, respectively. Therefore, in terms of color scheme, we believe the original Fig. 7 looks better. But if the reviewer and editor still suggest us to use the modified one, we are happy to do so.



(A) Spatial pattern of the total explanatory power of the joint influence of atmospheric CO2 concentration and soil moisture on hot extreme occurrences. (B) Spatial distribution of more important influencing factor for the occurrence of hot extremes between increased atmospheric CO2 concentration and local moisture deficit.

9. Lines 173-176. It is likely related to the fact that low-middle-income regions are in the tropics, which are generally not water limited, and therefore tend to be more sensitive to increased CO2. I do not find it necessary to have Fig. 8 in the main text.

We agree. Since we did not provide the mechanism for the relationship between income levels and the occurrence of hot extremes, we decide to remove Fig. 8 and the corresponding texts. This action is also consistent with what the Reviewer #2 suggested.