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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Relative importance of increased atmospheric CO_2 concentration and local moisture deficit to hot extremes" *by* Ajiao Chen et al.

Anonymous Referee #3

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The manuscript analyses the relative importance of global atmospheric CO2 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.

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

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

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.

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

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.

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

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.

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.

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.

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