Authors' response to interactive comment of the Referee #1

Black text: Referee comment

Blue text: Authors' response

We greatly appreciate your comments and suggestions for improving our manuscript. Our response and revision plans are provided below.

1. I have taken a bit more time than I normally do to review this work, because the study is interesting but also because I felt there might be some potential issues with the analysis that I wanted to get clear. The manuscript by Chen and co-workers focusses on the occurrence of hot extremes and their relation to global warming on the one hand, and local soil moisture conditions on the other. This is relevant because both factors have been shown in previous studies to play a role in the occurrence of, and trend in, hot days. Simply put, the authors analyze where the correlation between heatwave days and global CO_2 levels is higher, and where the correlation with local soil moisture deficit is higher. The main question for me as a reviewer is whether such a comparison makes sense, and is fair.

Thanks for spending time on reviewing our work, and we are very glad that you are interested in this study.

To address the issue that you are concerned with, we would like to explain that our conclusion with respect to which factor is more important in influencing hot extremes is not based on comparing the absolute value of correlation coefficients. We applied the dominance analysis (Azen and Budescu (2003) (https://doi.org/10.1037/1082-989X.8.2.129)) to compare the relative importance of different influencing factors to the occurrence of hot extremes. The dominance analysis method became popular in social science, it can compare the relative importance of predictors in multiple regression. Recently, this method has also been applied in natural sciences. Even though the predictors are correlated, the dominance analysis can estimate their relative importance. For example, Asoka et al., (2017) (DOI: 10.1038/NGEO2869) used it to investigate the relative contribution of monsoon precipitation and pumping to changes in groundwater storage in India. We hope this explanation addresses your concerns, and in a revised manuscript we will provide more detail of the dominance analysis as follows:

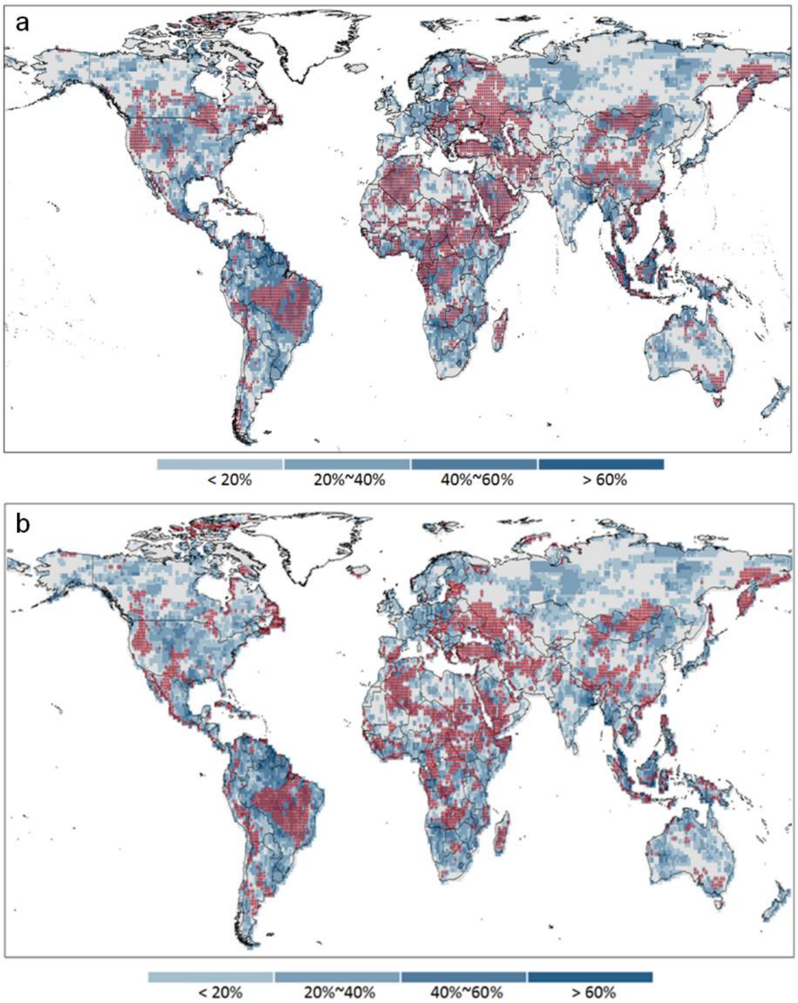
"Stepwise multiple linear regression (Draper and Smith, 1998; Clow, 2010) is used to determine the significant (5% significance level assessed by an F-test) predictor variable in explaining NHD temporal variability for each grid cell. Next, the dominance analysis approach (Budescu, 1993; Azen and Budescu, 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."

2. I feel there are several issues with this approach, related to the variables selected for the analysis, as well as with the data used to calculate the correlations. Firstly, the selection of yearly-average, global CO_2 levels is somewhat arbitrary. Yes, they drive global warming, but nobody would claim that the yearly variation of CO_2 would directly influence hot days at any particular location. The link is simply too indirect and weak, since it depends not only on how CO_2 levels influence global temperatures, but also circulation. CO_2 is also only one of the greenhouse gasses. In a way, it would be more logical to use global average temperature deviations instead, since these at least reflect the effect of ENSO and other climate variability that is known to affect the occurrence of hot extremes. But even there the weak is link, since in many regions the year-to-year variability in heatwaves is much more closely linked to indices such as ENSO, NAO etc. than it is to global CO_2 or local soil moisture. Again I am not talking here about the fact that heatwaves increase with global warming, and that this increase correlates with global CO_2, but the point is that the absolute value of the correlation coefficient is meaningless and should not be compared to other correlations because it results from arbitrary choices.

We agree that many factors (including atmospheric circulation patterns) together determine hot extreme occurrence. Global warming and soil moisture examined in this study are two of them. You are concerned that the links between the occurrence of hot extremes and the two factors are weak. We would like to address this concern first, followed by explanation of why global CO_2 concentration is used as proxy data for global warming in our analysis, and why the circulation pattern is not directly included in our analysis.

This study examines number of hot days in summer, which tend to occur under clear sky conditions during daytime. Standard 2-metre-height air temperature data were analyzed here. Physically, the heat source for near-surface air during daytime is directly from sensible heating of the underlying surface. Local soil moisture conditions strongly influence partitioning of net radiation on the surface into sensible heat and latent heat. Thus, it is reasonable to expect an association between number of hot days and soil moisture. On the other hand, global warming increases heat storage in the atmosphere, which tends to increase the likelihood of hot day occurrence. Thus, we can expect an association between number of hot days and a global warming variable.

We agree with you that global average temperature deviation is a rational proxy to be used in this study, which can reflect effects of both global warming and climate variability on the occurrence of hot extremes. We tried deviation of global average temperature before, the result looks very similar to what CO_2 concentration shows (see the figure shown below, dominance analysis based on temperature (a) vs. CO_2 (b)).



The consistency between the two maps is not surprising, as the major component of greenhouse gas, CO_2 is reported to have the highest positive radiative forcing (73%) of all the human-influenced climate drivers (IPCC (2013)).

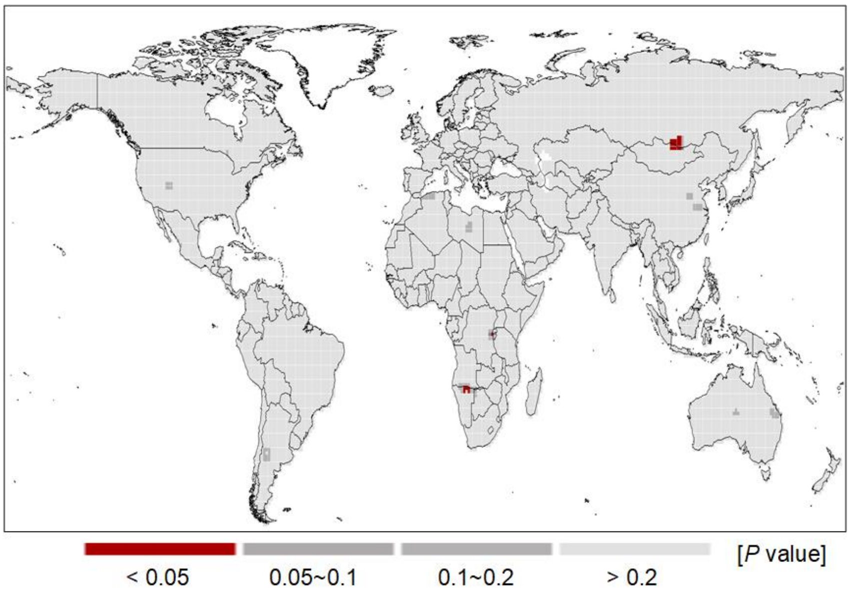
We agree that when and where a hot extreme event occurs is determined by the atmospheric circulation (weather), which may include climate oscillation (e.g., ENSO) effects. But this process is in general chaotic, beyond the control of human society. CO_2 concentration as well as soil moisture are more likely affected by human activities. Thus, results in this study are expected to provide practical advice for society, i.e., global measures on reducing greenhouse gas emission and adaptive land management in some regions with increasing moisture deficit, to mitigate heatwaves.

We appreciate your comment, which reminds us that clearer explanation on why we selected those variables is needed in our manuscript. Besides, in order not to mislead readers that we ignored the strong effect of atmospheric circulation on hot extremes, we will clearly state that the aim of this study is to compare the relative importance of CO_2 concentration and soil moisture to the occurrence of hot extremes instead of identifying the dominant driver among all influencing factors.

3. A second problem is caused by the data used. From what I was able to find about the GRACE TWS dataset used, it seems that the different timeseries have been detrended. This is

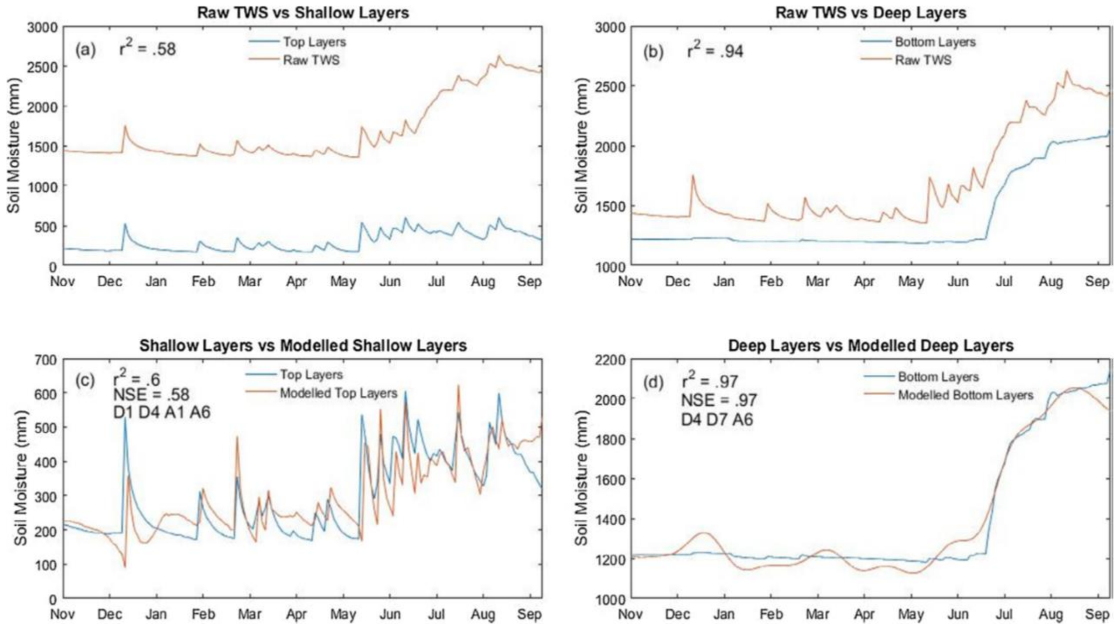
problematic in the view of the main research question, because any temporal trend in storage due to climate change will now be attributed to changes in CO_2 only. This also shows that CO_2 and soil moisture impacts will be difficult if not impossible to separate from observations only, since the two are not independent. A second problem is that the different decomposed signals should be interpreted with care. Even if they do reflect soil moisture at different depths as the authors claim, it will be unlikely that any storage with response timescales of several months will significantly impact energy balance partitioning at the land surface, and air temperature. In case of the significant correlations found by the authors, Occam's razor should be applied first: the more simple explanation is that both are affected by persistence in the atmospheric forcing signal due to persistence in atmospheric circulation. Only is this can be excluded as explanation should the correlation be interpreted as the result of direct soil moisture effect on air temperature.

Yes, all 'detail' components (D1-D4) of GRACE TWS have been detrended, but in all 'approximate' components (A1-A4) the trends are contained. Since D1-D4 and A4 (the sum of all these components equals to the raw TWS signal) are all included in dominance analysis in this study, variation and trends in heatwave days can be attributed to both CO_2 concentration and soil moisture. In fact, the two variables Global CO_2 and GRACE TWS adopted in this study are mostly independent. Here we provide a map (see the figure shown below) to show the correlation between CO_2 and TWS (only p value is shown). There are very few grid cells (marked in red) showing significant correlation (p=0.05) between CO_2 and soil moisture. This result and the relevant explanation will be added into the revised manuscript. We believe this can address your concerns about the independence between the two variables.



Regarding your concern on how we use wavelet decomposed TWS time series in our analysis, and particularly your questioning on the relationship of multiple-month signal and heat extreme occurrence, we would like to provide the following explanation.

The approach of using decomposed GRACE TWS to estimate various water storage components was proposed by Andrew et al., (2017) (http://dx.doi.org/10.1016/j.jhydrol.2017.06.016). The method was demonstrated based on time series of soil moisture measurements from near surface to a depth of six meters (see the figure shown below). The depth integrated total soil water storage is mathematically used to mimic the behavior of GRACE raw TWS. This “raw TWS” is then decomposed into sub-time series at discrete time scales. These decomposed time series are then used to compose shallow and deep soil moisture time series. It is clear that even shallow soil moisture has a component of long-time scale, although its proportion is smaller than that in deep soil.



Copied from Andrew et al., (2017). Results using the wavelet decomposition and stepwise regression method for estimates of soil moisture at different depths. Plots a and b show the depth-integrated soil moisture vs. the shallow and deep layers. Plots c and d show the estimations and observations of soil moisture for the shallow and deep soil layers.

In addition, Andrew et al., (2016) (doi:10.5194/hess-2016-545) reported that vegetation responses to terrestrial moisture changes are of multiple months scales. Grassland-dominated areas are more sensitive to higher frequencies of moisture storage changes while plants with deeper rooting systems (e.g., forests) are more sensitive to moisture storage changes of longer time scales. Therefore, it is possible that moisture storage with response timescales of several months could impact energy balance partitioning at the land surface.

Based on the explanations above, we believe it is reasonable to examine the effect of soil moisture on air temperature. As you mentioned that the effect of atmospheric circulation on the occurrence of hot extremes could be stronger than effects of CO_2 concentration and soil moisture, it is necessary to clarify in our manuscript that the aim of this study is to compare the relative importance of the two variables we selected rather than identifying the dominant driver among all influencing factors. We will modify the relevant sentences in the revised manuscript.

4. In conclusion, I believe the separation between CO_2 and soil moisture effects on hot extremes cannot be done from correlation analysis on observations along the lines of the analysis presented here, but would require dedicated experiments with coupled climate models. Since in my view the conclusions of this work on the relative importance on both processes are not sufficiently justified by the evidence presented, I have to conclude that the current version of this work is unfortunately not suitable for final publication in HESS.

We agree that coupled climate models are very useful in dedicated numerical experiments and analyses. However, we believe that before these models are applied it is essential that data driven analyses are performed to learn from the data what the relative importance is of different variables. As we explained above, the dominance analysis method we used in this study, can quantify relative importance of predictors in a multiple regression. In addition, correlation between CO_2 and TWS signals are very weak as shown in our reply to your previous comment. We will add that figure and relevant descriptions into the revised manuscript in case readers have the same concerns as you.