



EGUsphere, author comment AC1
<https://doi.org/10.5194/egusphere-2022-321-AC1>, 2022
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Reply on RC1

Fulden Batibeniz et al.

Author comment on "Countries most exposed to individual and concurrent extremes, and near-permanent extreme conditions at different global warming levels" by Fulden Batibeniz et al., EGU Sphere, <https://doi.org/10.5194/egusphere-2022-321-AC1>, 2022

We would like to thank the reviewers for their valuable comments. We have addressed all major and minor comments of the reviewers through appropriate changes and hope that the revised manuscript satisfies the reviewers' concerns.

The Response to the Reviewers file provides complete documentation of the changes made in response to each comment. The document is designed so that the changes that have been made in response to each comment can be immediately read and understood, independent of the other comments and responses. While this comprehensive comment-by-comment explanation requires some duplication of material throughout the document, our intention is that it helps to evaluate precisely how each comment has been addressed.

Reviewers' comments are shown in **bold**. The authors' response is shown in plain text.

Summary of edits

Here we would like to summarize the changes we made in the manuscript. We did considerable updates in the used methodology. For extreme wind event calculation, we now use daily maximum wind instead of daily wind and for drought events' calculation we now use daily data instead of monthly soil moisture data. We also changed our definition of concurrent events, and only marked the months where the extreme pairs occurred on the same day, whereas before it was on the month level.

We would like to also note that we changed the methodology of extreme indices and percentile calculation. It is mentioned previously in literature that percentile based indices for climate change detection may create artificial jumps at the beginning and end of the reference period (Zhang et al., 2005). These discontinuities can lead to an artificial frequency increase outside the reference period (e.g., at warming levels). We used the bootstrap resampling procedure proposed by Zhang et al. (2005) to overcome this problem. Indeed, this procedure improved our results in terms of inhomogeneities occurring outside the reference period. We show this improvement with a figure in the supplementary.

Some of the above-mentioned calculations and text editing are still ongoing. We are also revisiting the text for every section to address all the questions/concerns of the reviewers.

Reviewer #1

This is a valuable study confirming, and bringing nuance to, the mounting evidence that extreme events – including compound extremes – are becoming more likely in a warming climate. The analysis of individual (soil moisture drought, heatwave, extreme precipitation and wind) and their concurrence is indeed interesting, but the paper suffers from a great deal of ambiguity making it hard to follow. For example, it is key for the audience to understand the global warming level scenarios, but the provided description lacks the required details (from text: “The warming levels are defined as the first 30-year period where global mean temperature anomalies exceed the given temperature (e.g. +2.0°C).” Similarly, the abstract provides details of how more frequent extremes will be, but does not clarify compared to what. Furthermore, coherence of the text and relevance of the presented materials to the main topic of the paper – specifically in the introduction section – needs improvement. In the following, I provide specific comments that hopefully are useful as the authors revise their manuscript:

Abstract: Needs more specific details about the presented statistics. View this from the lens of a general audience that might not be climate scientists. Anyone should be able to understand this synopsis of the paper, and at this point the text is too vague. For example: in Line 18, more frequent compared to what? Also, it might be helpful to define the period used to calculate extreme events’ stats associated with various warming levels.

We would like to thank the reviewer for their overall positive evaluation of our manuscript. We have made substantial changes in the manuscript in the light of these comments and hope that these revisions have addressed all the major or minor concerns.

In response to the reviewer's concern about the warming levels, we now explained the global warming levels extensively with a figure and text in the methodology section, which we believe will help readers to understand global warming levels better. Warming levels are 20-years periods unique to each model due to different climate sensitivity and internal variability and thus timing of the global warming levels. We first calculate the annual average global temperature. Then, we subtract the average global temperature of the reference period 1850-1900 (pre-industrial period) from every year between 1850-2100 and take the 20-years running mean. The first year a certain anomaly such as 1°C, 1.5°C, 2°C, and 3°C is exceeded is the central year of the warming level period and the warming level period is obtained by subtracting 10 and adding 9 to the central year. For example, IPSL-CM6A-LR reaches 2°C warming in 2036 so the period selected for this model is 2026-2045. On the other hand, MRI-ESM2-0 reaches 2°C warming in 2040 and the period selected is 2030-2049. In previous analysis warming levels included 30-years but now we changed it into 20-years consistent with the chapter on weather and climate extremes of the IPCC AR6 (Chapter 11; Seneviratne et al., 2021).

We agree with the reviewer's comment about the abstract, and we will clarify the ambiguous sentences. We also believe understanding warming levels will remove the concern about the ambiguity in abstract regarding the frequency increase. We give all the frequency statistics with respect to the reference period where we can differentiate anthropogenic climate change from current (+1 °C) and future global warming levels (+1.5 °C, +2 °C, +3 °C). Only exception is the population exposure section where we use +1°C as our comparison period for future warming levels.

Introduction: There are much information that is not relevant to the paper. For example, you are not addressing vulnerability and adaptive capacity in this paper, and I wonder how paragraph starting in line 47 builds up the foundation

for your research question? I would use this information in the discussion, but not introduction. Also, information presented in many paragraphs are not aligned with the topical sentence. For example, I don't know how naming specific countries in lines 40-42 can be helpful in this section. Furthermore, lines 42-45 are neither relevant to the study topic nor aligned with the topical sentence. In general, the introduction section can be sharpened to interest the audience.

Thanks for the comment. We will revisit the introduction section and make substantial changes to sharpen our message.

Lines 89-90 read: "we investigate here for the first time the human exposure to these concurrent extremes in addition to individual extremes." -> This sentence is not correct. See for example:

Concurrent: Liu, W., Sun, F., Feng, Y., Li, C., Chen, J., Sang, Y.F. and Zhang, Q., 2021. Increasing population exposure to global warm-season concurrent dry and hot extremes under different warming levels. Environmental Research Letters, 16(9), p.094002.

Individual: Alizadeh, M.R., Abatzoglou, J.T., Adamowski, J.F., Prestemon, J.P., Chittoori, B., Akbari Asanjan, A. and Sadegh, M., 2022. Increasing Heat Stress Inequality in a Warming Climate. Earth's Future, 10(2), p.e2021EF002488.

And many more studies, including some from the authors' group.

We thank the reviewer for bringing these papers up. We were not aware of these papers. We now will add the mentioned references to the revised manuscript and revise the last part of the introduction in the light of these comments.

Sections 2.5. & 2.6. and across the manuscript: The definitions of individual extremes and compound extremes are confusing. I learned half-way through the manuscript that drought refers to a drought that is not concurrent with heatwave. The entire paper needs to be revisited to clarify what each of the extremes (individual or compound/concurrent) refers to. Also, and importantly, the temporal resolution of extremes needs clarification. I understand how droughts are monthly and heatwaves are daily, and how the authors label a month as observing concurrent drought-heatwaves, but I struggle with how concurrent extreme precipitation and wind is defined. As it stands, it seems like a month that has one of each event is labeled as observing concurrent extreme precipitation and wind, which is not correct (at least in my opinion). The impact of extreme precipitation and wind are most pronounced at the daily scale (or even hourly, but let's stick to daily), and that should be the temporal scale of the analysis. If one occurs at the beginning of a month and the other occurs at the end of the month, that month should not be tagged as having observed a concurrent extreme precipitation and wind.

We appreciate this suggestion. In the light of this comment, we now made substantial changes in the manuscript. In response to reviewer's concern on concurrent event definition; in principle you could have an extreme precipitation event at the beginning of the month and extreme wind event at the end of the month both occurring in the same location. Even though two of those hazards don't interact with each other, they contribute to the overall exposure of that grid cell/location. However, to overcome this concern we also changed our definition of concurrent events, and we only marked the months where the extreme pairs occurred on the same day.

Line 179 reads "Drought events, on the other hand, tend to decrease for higher

GWLs in MHC and STC.” This is confusing/misleading for the reason mentioned above. Similarly, lines 223-224 read “Interestingly, STC sees a small decrease in individual drought events in most months for 3°C warming.” Which is again misleading due to the definition and lack of clarity of the text.

Thanks for pointing this out. We now will edit the text in the light of this comment. We will rephrase some sentences to clarify the text and instead of individual we will use isolated events to avoid confusion.

On a technical note, how reliable are Rx1day simulations/projections? How about wind?

The performance of CMIP6 in simulating precipitation extremes is lower than extreme temperature in China (Zhu et al., 2020). It is found that compared to CMIP5, CMIP6 produced climatological patterns and the interannual variation of extreme precipitation indices better in China (Zhu et al., 2020) and in some parts of the United States (Srivastava et al., 2020). Additionally, improved performance of CMIP6 over CMIP5 is found for wind speed data for the Bay of Bengal (Krishnan and Bhaskaran, 2020). We are not aware of any other papers focusing on evaluation of extremes produced from CMIP6 globally. We are aware of the uncertainties CMIP6 can produce; however, it is the best and valid option to investigate extremes in the future climate. We plan to add this caveat to the conclusion section.

In general, it would be helpful to discuss why certain temporal and spatial patterns are projected. For example, lines 226-228 (among others) can benefit from this.

We agree that the frequency increases in months needs to be discussed therefore we will add new text to the discussion section.

I struggled to understand how population projections are used in this analysis. It seems like the 2015 population data was used only. Please clarify the text.

Thanks for the insight. We revisited and clarified the text for the population counts section. Indeed, we only use 2015 Gridded Population of the World version 4 (GPWv4) data to calculate the population exposure in section 3.4. Therefore, we removed information related with population projections of shared socioeconomic pathways (SSP5) to avoid confusion.

I also struggled to understand how the number of events per person on the country basis was calculated. Are you calculating exposure (person-days) and dividing it by the country population? In any case, please clarify. Also clarify what you mean by certain counts of extremes per person. What temporal span does this refer to? Annual? Decadal? 30 years?

The temporal span of this analysis is 20-years ($20 \times 12 = 240$ time-steps). We multiply the population with hazards (binary) at each grid cell. Sum all the values on the country level and divide it with the total population of that country. The obtained value is the number of events (or months) per person in that specific country which cannot be more than 240. We now explained it in more detail in the manuscript.

Lines 303-304 read “The number of events per person increases gradually across the globe except tropical countries in the African continent and India.” While it might be beyond the scope of this paper, it would be interesting to discuss how the decomposition of population dynamics (if it is considered here) and count (e.g., country population) vs extremes frequency trends contributed to these

statistics.

Thanks for the suggestion. We now plan to include this into the discussion.

Are you using multi-model mean or median? Line 173 says "mean" and line 327 says "median"

We are using multi-model mean for Venn diagrams and multi-model median for the rest of the analysis. In Venn diagrams, we calculate $(A \cap B)$, $A - (A \cap B)$, and $B - (A \cap B)$ (sets) for each model separately. To avoid showing different shares from different models for each set, we illustrate the mean.

Lines 339-341 read "Northern parts of South America especially Bolivia, Chile, Paraguay and Brazil, South Africa, the United States of America, Australia and Mexico are also very vulnerable to this change.". Confusing sentence. The value of naming specific countries in some context and referring to regions in other contexts is not clear to me

Thanks for the comment. We agree this is confusing, we will revise the text.

Lines 395-396 read "Therefore, using population projections to investigate the human contribution to the change could help understand future risks more". Not clear

Thanks for the insight. We rephrased the sentence in a way to be clearer to the reader.

Minor comments:

Line 39: "and"->"that"

Thanks. We fixed it.

Compound extremes in this paper refers to concurrent extremes, if I understand correctly. It might be helpful to be specific throughout the paper.

Thanks for the insight. We are now using concurrent extremes throughout the paper.

Reference:

Krishnan, A. and Bhaskaran, P. K.: Skill assessment of global climate model wind speed from CMIP5 and CMIP6 and evaluation of projections for the Bay of Bengal, *Clim Dyn*, 55, 2667–2687, <https://doi.org/10.1007/s00382-020-05406-z>, 2020.

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Zhang, X., Hegerl, G., Zwiers, F. W., and Kenyon, J.: Avoiding Inhomogeneity in Percentile-Based Indices of Temperature Extremes, *Journal of Climate*, 18, 1641–1651, <https://doi.org/10.1175/JCLI3366.1>, 2005.

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