Reply to Linda van Garderen

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We thank Linda van Garderen for her thorough reading, constructive comments and the overall positive assessment of our manuscript. In the following, we address her comments point-by-point.

General remarks

The paper is a comprehensive study of the 2018 west-central European drought focussing on soil wetness deprivation, and what that drought would have been like in a world with different levels of climate change using the pseudo global warming method and analogues. The study is technically rich, as the analysis is done with different global climate models for input to the regional model providing a cross-check for the results, as well as content-wise rich with comparison to other drought events and a climatological test of droughts in this region and elaborate placement of their findings in existing literature.

I find this research very relevant as it produces new insights in the attribution of European droughts. The analysis of the drought is well executed and technically sound, including temperature, precipitation and evaporative demand. Limitations to the analysis are also reflected on accurately. The paper is lengthy, though, and could benefit from a reduction in size (I have tried to provide some small suggestions for this in the minor comments below). The English language is of high quality and the paper reads well.

Detailed remarks

You have used "(not shown)" six times in this paper (L221, L235, L276, L279, L329, L340). This is too often and becomes a hindrance since it implies a demand of trust. Either show what you claim, rephrase, reference to a paper that proofs a comparable outcome to the one you found, or delete the claim. Please try to reduce the "(not shown)" to one or two times. Since you have a large appendix already, you might consider placing some of these aspects in the supplementary material to prevent the paper from becoming larger.

Thank you for pointing this out. We will reduce the number of *not shown*s in the revised manuscript:

L221: As a consequence, the sensible heat flux strongly increases (not shown), ..

This concerns the increase in sensible heat flux during the summer of 2018, which logically follows from the increase in evaporative demand (shown), and decrease in evaporation (shown) and soil moisture (shown). In our opinion, here no additional figure is needed.

L235: For deeper soil layers the winter precipitation is insufficient to fully replenish the soils to normal levels, and the anomalously dry conditions persist throughout 2019 (not shown).

Since the focus of this manuscript is on the top 1 m of the soil, we decided to limited the figures to variables that are directly relevant for the top 1 m of the soil. However, readers may remember that groundwater levels were lower than normal throughout winter and spring 2019. To show that this is the case in our simulations as well we'll add a figure similar to figure 1(b) in the supplementary material/appendix of the revised manuscript.

L276: This feature of the response is amplified by reduced snowmelt in spring and a larger fraction of precipitation falling as rain in autumn (not shown).

We could add a figure similar to figure 5b (water balance) for the climatological mean response to the appendix.

L279: In summer and autumn, the soil moisture availability in deeper layers and runoff decrease as well (not shown).

We can change the orientation of Fig. 3, and add a subplot with the soil moisture development in deeper layers in the main text, or we can add a figure similar to Fig. 5b (response in water balance components) for the climatological mean response to the appendix (as for L276).

L340: Percolation to deeper soil layers and runoff decrease in this period as well (not shown).

Actually this is shown later in the manuscript, in Fig. 5b. We'll add a reference in the revised manuscript.

Not all graphs are easily readable. I reviewed using a print-out and could not read Figure 1 and Figure 5a, and to some extent Figure 3. I have put a more detailed explanation below, but would encourage giving figures a check-over for readability.

We're sorry that you couldn't easily read some of the figures in the printed version of the manuscript. Apart from the color in figure 1b the other reviewers did not comment on the figures, so we trust that larger (online) versions of the figures are clear. We will provide full-width versions of the multi-panel plots in the revised manuscript.

Minor comments

L52-L56 This is 1 very long sentence. Please shorten for ease of understanding.

We'll split the sentence in the revised manuscript

L67-L69 This sentence could use a reference to back up the claims. For the Mediterranean precipitation research, you could cite Zappa et al., 2017 (http://dx.doi.org/10.1175/JCLI-D-16-0807.s1).

The response pattern over Europe described in L67-69, including the response over west-central Europe (L70-71) is shown in the references in line L71. We will move these references one line up in the revised manuscript to make this more clear.

L99-L100 Is your study trying to provide something like the 'common framework' published by Shepherd et al., 2016? (http://dx.doi.org/10.1007/s40641-016-0033-y)

L99-100: Additionally we evaluate the position of this 2018 event in the 1980-2020 period, both for present-day and for future conditions under a single warming level.

Shepherd (2016) discusses extreme event *attribution* to climate change, and we wouldn't suggest that we do anything like that (a 41-year simulation period is way to short for attribution). However, simulating a future weather analogue not only of the extreme 2018 event, but of all summers in the 1980-2020 period, provides context and a better understanding of the changes we find for the extreme summer of 2018.

L132-134 You are altering a large number of variables to create the counterfactual worlds. Could you argue why it is acceptable to meddle with the model to such a large extent without loosing physical self-consistency? In essence, you are altering the consequences of climate change (T, P, SH, etc.) instead of (only) the causes of climate change (GHG, SST, etc.). I am aware that the method requires

this to create a counterfactual world, but I would like to hear the validation in 1 or 2 short sentences (could be in discussion you find that a better fit).

The strength of the application of a regional climate model for this purpose is that the simulations are physically consistent in the interior of the model domain by design. The perturbations itself are derived from GCM simulations and are added to *all* state variables, again to ensure physical consistency. Moreover, the perturbations are fairly small (apart from temperature) and are smoothly varying in space and time. See also e.g. Prein et al. 2016 and Shepherd et al. 2018.

Of course, we built our PGW simulations on the large-scale circulation in the present-day period, and it may be that the frequency of e.g. blocking conditions changes in a globally warmer world. However, dynamical changes are highly uncertain, and there is no physical argument against the plausibility of the present-day circulation types under globally warmer conditions.

L145-146 Please add the resolutions of the individual EC-EARTH v2.3, HadGEM2-ES and MPI-ESM-LR GCM's.

We'll add a table with characteristics of the GCMs in appendix A.

L169-173 The reader might benefit from adding an equation to show this second step as well.

Ok, we'll add that!

Figure 1 The b panels are not readable at all, please change colours and either make the graph bigger or lines a little thinner. Ep is not mentioned in the caption. The legend in the b graphs is incomplete (shading is not mentioned, for instance).

Thanks for noting, we will add E_p in the caption.

The legend in figure b is spread over the top graph (colors) and bottom graph (line thickness and shading) the make full use of the space, and the colors and shading are explained in the caption. However, apparently this is not obvious to the reader, so we will combine both legends and show the legend below the graph.

We'll pick a different color for the observations to enhance the readability.

L203 I do not believe the pressure anomalies are shown? Or are they the once in the supplementary material? Please refer if that's the case or show what you are claiming or reference to a paper that shows it.

Yes they are shown as the 500 hPa geopotential height anomaly (contours in Fig. 1). We'll refer to the figure in the revised manuscript.

L261 The reader could benefit from adding which colour line to look at when referring to Figure 3a. Also, to add which colour to look at with "mean response is rather large…"

We will include in the text that 'the grey boxes show the 1980-2017 mean and inter-annual spread'.

L266 "increasing cloud cover…" is not shown anywhere, or do you mean an interpretation of solar radiation in Figure 3b? Please clarify.

The increase in solar radiation in JJASO indeed results from decreases in cloud cover (and humidity). The response in cloud cover is not shown. The response in solar radiation and relative humidity is. We will clarify this in the manuscript.

L282-L283 Figure C1 and C2 show vastly different GPH z500 patterns. Should this be mentioned or explained? Is this of significance for the analysis?

We describe this briefly in the methods section (2.2). The GCMs indeed differ in their regional response pattern. That is the reason for using perturbations derived from three different GCMs. Although the GCM is 'recognizable' in the PGW simulations, the results are overall fairly robust with respect to the GCM that is used to derive the perturbations.

Figure 3 The legend is incomplete, please also mention what the shading etc. stands for. The caption is a bit of an essay. I would suggest placing the method part that right now is at the end of the caption, including the equation, to the main text or supplementary material. From a print the graph is difficult to read, which is a pity since it shows some essential and interesting things. You could either make it bigger or reduce the thickness of the median (black or red).

As written above, we will enlarge the figure to improve the readability in a printed version of the manuscript. We will place the legend outside the subplot and clarify the shading. Concerning the caption, we prefer to leave the explanation that is required to understand the figure within the caption, for the reader to understand the figure without having to go through the text / supplement. In order to understand the text in which this figure is discussed, the explanation provided in the caption isn't directly needed and would only distract.

Figure 5a The lines are almost all the same colour, and to thick to interpret. There are even two lines that are both red. Please update the graph, make the lines thinner or the graph bigger, and choose colours that are further apart from each other. The caption is again a bit of an essay with a method at the end that could be explained in the main text or in supplementary material.

We will provide a larger figure in the revised manuscript. Note that lines with the same colors are derived from the same experiment (warming level). Shading (and deviation from the x-axis) indicates which lines belong to the climatological mean and which to the 2018 ensemble.

Also here we give preference to an explanation below the figure in order for the reader to understand the figure without having to go through the text / supplement.

L395 Just to make sure, is the comparison with E-OBS done by the paper you are citing in the previous sentence? I was trying to find a graph that shows the comparison, but if it is in that paper, please clarify this in the sentence ("the authors found…" or something like that).

No, this concerns an evaluation of our simulations, which isn't included as figure, to limit the length of the manuscript. However, we can add a graph extending Fig. 1b to 2020 in the appendix.

L490-496 I do not think you need to state the obvious, plus you are not testing these impacts in this paper. You could save space by deleting this section.

These are the implications of our results, we prefer to leave this section in.

L510-L516 (optional) You could add spectrally nudged storylines to the list of options, it will allow for drought intensification studies, but not changes in dynamics (which you claim in L571-573 is plenty) with the benefit of a very small size ensemble (van Garderen & Mindlin 2022, https://doi.org/10.1002/wea.4185). However, since I am the author and it is for a region outside of Europe, feel free to ignore this comment.

Thanks for the reference, we'll add it to the introduction of the revised manuscript. In L510-516 we specifically discuss the 2018 event.

L521-525 Could it be that the discrepancy has anything to do with altering symptoms of climate change and not causes? See also my comment for L132-L134

This is unlikely, see our response to the comment on L132-134.

L543-545 In the absolute sense the referencing is correct, since the papers do mention analogues as well. However, storylines and analogues are not the same thing, and the emphasis of the paper cited is on storylines. Perhaps succinctly place the analogues in the context of storylines without doing another literature review (which you have already done).

In Sillmann et al. (2021) a storyline is described as: "*physically self-consistent unfoldings of past events, or of plausible future events, [which] have been proposed as a way of articulating the risk in such cases where we need to go beyond a purely probabilistic climate change perspective, with an emphasis on plausibility rather than probability*".

The future weather analogues of present-day events, as simulated in our study, fit this description. However, the reviewer is indeed right that we haven't explicitly stated this in our manuscript. We will place the future weather analogues in the context of storylines in the introduction of the revised manuscript, where we introduce our approach.

The comments I made are minor, and I am looking forward to seeing this paper published.

Best,

Linda van Garderen