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Interactive comment

Interactive comment on "Euro-Atlantic winter storminess and precipitation extremes under 1.5 °C versus 2 °C warming scenarios" *by* Monika J. Barcikowska et al.

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We thank referee #2 for the constructive review. We will update our analysis with new present climate runs, which were not available previously. Unlike the historical 1979-2005 simulation, the new runs now follow the HAPPI experiment protocol, i.e. they constitute an ensemble of decadal runs in 2006-2015. Therefore they are more suitable for the analysis investigating changes between the present and future climate. This approach is also more suitable to address the main reviewers concerns on the 'non-linear effects of the derived climate change, shown in the study. We will provide explanations below.

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Discussion paper

A: Based on their analysis of the high resolution time slide experiments, the authors state that most of the changes due to anthropogenic forcing up to the 2C level only start occurring after the 1,5C threshold has been surpassed, and claim this may be due to non-linearities in the climate system (page 17, around line 30). While this may well be true to some point, I find it very hard to believe that while the climate change signal e.g. DJF MSLP and 850 hPa winds up to a 1,5C warmer world is practically zero (cf. Fig. S1a), we become a very strong response when we add the additional 0,5C (cf. Fig. S1b). The same could be argued for the other fields.

In my view, such a strong difference for such a small increment of external forcing could only come from (a) internal variability (for example, storminess is at a low level of its decadal variability in the period chosen for the 1,5C experiment, while it is on a high level on the period chosen for 2C) and/or (b) there are some issue in the set up of the high resolution simulations which have lead to these differences, and not the small change in the forcing. This does not turn the results per themselves wrong, but means that the authors may be misinterpreting (or at least over-interpreting) their results. In can be that the problem is related with the SST set up, as the authors shortly discuss in section 2.1. For example, how does the climate change signal look like for the (hopefully transient) lower resolution GCM simulations? How does the climate change signal look like for the single ensemble members? This would be important to analyse in detail to identify if the changes in precipitation and wind are continuous over time (plus natural decadal climate variability) or if indeed some strong "non-linear" effects occur. It is very improbable that the AMOC will collapse in a two degree warmer world. What might be important here is the thermodynamical effect (Clausius-Clapeyron), primarily for precipitation intensity. This is very important to rule out that the obtained results are not simply caused by some issue with the set up of the model simulations and clearly relate the changes to the increases of anthropogenic forcing.

AU: Thank you for the comment. Indeed, the "non-linear" effects of the derived changes, when comparing first half a degree warming (+1.5C minus present) with the additional half a degree warming (+2C minus +1.5C), are likely associated with the simulations set up. i.e. an asymmetry in the aerosol forcing between the present and future climate scenarios. The changes associated with warming at the 1.5°C level stem from an interplay of a number of forcings, including strong aerosol reductions, while an additional half a degree warming is solely a consequence of CO2 increases and ocean warming. We mentioned this in the manuscript, but we will enhance this message in the revised version. This will be also supported with an updated analysis, which includes new present climate simulations. These simulations follow the HAPPI protocol (unlike the previous ones) and are more relevant to address the nonlinearity issue.

Minor comments:

#1 Page 2, line 34: regarding the role of cyclone clustering for the wind and flood impacts, the authors could refer for example to Priestley et al. (2017), which deals specifically with this topic. AU: Thank you, we can apply this suggestion.

#2: Page 3, around line 10: The "poleward" shift of the storm tracks due to enhanced anthropogenic forcing is correct on zonal average, but this is not always true for Europe: while this is clear for the summer half year, the results for the winter, particularly DJF (the focus here), are different – see your own text in page 4, line 10. Please check e.g., the discussion in Zappa et al (2015), and also their Fig.1.

AU: Yes, we will take this comment into consideration while correcting the manuscript.

#3: Page 4, line 12: The results by Zappa et al. (2013b) do not contradict the results described in many of the papers mentioned in the previous sentence. For example, the results from Bengtsson et al. (2006), Pinto et al. (2009) or Zappa et al. (2013b) only show small differences in detail regarding the intensification of cyclone activity over the British Isles and the strong decrease in the Mediterranean. Thus, "however" is not the correct word here, maybe "Morevoer" would be more appropriate.

AU: Thank you for the correction.

#4: Page 4/5: The authors describe their reasoning as if it would be the very first time that high-resolution global climate modelling is performed at _25km resolution. This is not correct – for example, there is a whole EU project regarding high resolution modelling to study impacts of climate change for Europe (PRIMAVERA, https://www.primavera-h2020.eu/about/objectives/). It is strange not to mention this at all in the introduction, nor any of the related papers (e.g. Schiemann et al., 2017). The MIROC and MRI groups has also been working high-resolution climate models for several years (e.g. Murakami et al., 2011), which is not really discussed– tough Kitoh and Endo (2016) is shortly mentioned. Please note that another possibility would be to use RCMs as ESMs (e.g. Sein et al., 2015). Please enhance.

AU: Thank you for the correction, we will certainly clarify and enhance the Introduction.

#5: References: many issues, both in the reference list and in the text. For example, Zappa et al (2012) is surely one of the two Zappa et al. (2013) papers, Feser et al (2014) is probably Feser et al. (2015). Pinto et al (2009) is three times in the reference list, and several references mentioned in the text are not included (e.g. Barcikowska et al. 2017) are not included in the Reference List. Please enhance.

AU: We will check again all references.

#6: Figures: it would be very good to had labels to the isolines in the figures, particularly in Figures 8 and 9. It is not enough to mention the values in the captions. Please enhance. AU: We will also include the suggested additional references.

Please also note the supplement to this comment:
https://www.earth-syst-dynam-discuss.net/esd-2017-106/esd-2017-106-AC3-supplement.pdf

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