

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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Received and published: 9 February 2018

We thank referee #1 for his 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. With the updated/improved design of the analysis, some of the reviewer's comments may not apply to the current version of the paper. We will provide explanations below.

Anonymous Referee #1 Received and published: 17 December 2017

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1 General Comments The manuscript "Euro-Atlantic winter storminess and precipitation extremes under 1.5 °C versus 2 °C warming scenarios" by Barcikowska et al presents comparisons between storminess and precipitation in the 20th century and in the early 22nd century using newly available model simulations from the HAPPI project in different horizontal resolution. They first evaluate model results through comparing results from the model runs on different horizontal resolutions with ERA-Interim (circulation-type variables). Here, they conclude that the 0.25 degree resolution provides the best results, where atmospheric features are presented superior to the lower horizontal resolution model simulations. In the following, 0.25 degree model precipitation is compared with data from the EObs and GHCN datasets, where the authors find very good agreement. Afterwards, the authors investigate the differences between the scenarios under 1.5 and 2 °C warming, whereby they present changes in the mean-state of the large-scale atmospheric circulation and precipitation, in daily and sub-daily precipitation and wind extremes, and in storminess for the 0.25 degree run. Overall, the manuscript deals with an important subject and combines different aspects of how storminess and precipitation changes under 1.5 and 2 °C warming, also with regard to making the model simulation finer. The manuscript clearly conveys this subject, but nevertheless suffers from several major aspects that need improving and/or further clarification, before it is ready to be published.

1. The HAPPI ensemble consists of several model runs from different modeling centers (not mentioned in the text). I understand that you concentrate on the CAM5-simulations, but give no reason why the other simulations are discarded.

AU: In this study, we only focus on the same model version run at different resolutions. This allows us to investigate the impacts of a very high model resolution on the representation of large-scale and regional features in comparison to a coarser resolution. Additionally CAM5 1.2-0.25 provides unprecedented opportunity to investigate extremes on subdaily time scales. We stated this in the introduction, but will make it clearer in the revised version.

2. Regarding the model resolution: The implications found for the large-scale atmospheric circulation cannot be overstated enough and put the model into the sphere of dynamically downscaled regional models with corrections for the larger scales. I agree with section 3.4 that there is room for some kind of sensitivity study here.

AU: We agree but think that such sensitivity studies should be part of a separate study. We nevertheless motivate such studies in our manuscript.

3. I also find it very interesting to see the differences between the present climate and 1.5 °C vs 2 °C warming. Your results suggest (as you wrote) that there seems to be a threshold in between, that once crossed, exacerbate storminess conditions.

AU: Yes, one interpretation could be associated with the threshold. Other interpretation, with an additional analysis including new present climate simulations, suggests that the difference might be due to the asymmetry in aerosol forcing between the present and future climate. It is a very important point and we elaborate on that in the revised version. We support our statements, by updating the analysis with new simulations of the present climate. These simulations follow the HAPPI protocol (unlike the previous ones) and are more relevant to address this issue.

4. Why is ERA Interim the reference for midlatitude atmospheric circulation? How does ERA Interim compare to other reanalyses with regard to circulation?

AU: We use ERA interim, because its spatial resolution is comparable to the high-resolution CAM5 model simulations. ERA interim has considerably higher model resolution (80 km at 60 vertical levels) than other reanalysis products. Hence it is able to resolve sharper spatial gradients than e.g. NCEP/NCAR Reanalysis. Hodges et al. 2011 compares reanalyses for extratropical cyclones and shows that the newer reanalyzes (especially ERA-Interim and NCEP-CFSR) agree (both in terms of numbers and locations) much better than the older ones (JRA-25) for both hemispheres and that intensities are higher. As our purpose is to validate the model, ERA interim serves the purpose better coarse reanalysis products like NCEP/NCAR.

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Hodges, K.I., R.W. Lee, and L. Bengtsson, 2011: A Comparison of Extratropical Cyclones in Recent Reanalyses ERA-Interim, NASA MERRA, NCEP CFSR, and JRA-25. *J. Climate*, 24, 4888–4906, <https://doi.org/10.1175/2011JCLI4097.1>

5. The changes of the SLP gradient are interesting. As you mention the NAO in the beginning of section 3, what are the consequences for the NAO index caused by the increasing SLP gradient difference? You could compute the NAO index and show how it changes as it should be a stationary process centered around 0 in the long-term.

AU: Because of the strengthening of the SLP gradient, NAO index will likely have a positive tendency, when compared +2C future with the +1.5C/present climate experiments. However under the each stabilization scenario the spatial pattern/definition of NAO is different. Hence in this experimental set up NAO could be rather investigated separately for each of the three ensembles. Therefore more thorough analysis could be done only on NAO, but in a separate study. Otherwise the transient simulation would be more relevant for addressing the reviewer's question.

6. This one is very important: The question of statistical significance has not been dealt with properly and currently is rather imprecisely given (sections 2.2, 3.2 and S4). S4 and p.12 118 ("which defies statistical significance"): What you actually show is the distribution of differences, from which you can infer a confidence interval. What you do not get are real implications about statistical significance as written.

Please repeat your analysis here. Also: please move the detailed description of your method into the method-section without repeating the details later. Regarding S4: The figure looks very choppy. Either it is showing some kind of histogram sampled for specific blocks of differences (then it should be stated clearly), or it demonstrates undersampling in your bootstrapping approach. Either way, it would be good to redo the bootstrapping with a bigger sample than just 1000. The computation is cheap and very likely results in a better representation of the distribution of differences.

AU: This is indeed an important point. This analysis would be certainly very helpful

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for understating the implications for statistical significance, at the absence of the simulations, which are design to cancel out the effect of the internal variability. Instead, we will update the analysis with new simulations and discussion. These are tailored specifically to remove the effects of different phase of internal variability, questioning the statistical significance of the derived results.

7. The meridional SLP gradient and its differences: Sections 3 and 3.2 write about the SLP gradient, but only refer to figures 4 and 5 showing the respective MSLP plots. I, as a reader, am not able to estimate the gradient and gradient differences from such plots. As you define the gradient in section 2.2, it is very difficult to relate the Azores-Icelandic pressure difference to plots of MSLP or MSLP differences, even though I know about the related atmospheric patterns. Why not just give the gradient as a number somewhere? (also for 1.5 _C and 2 _C scenarios, and the differences).

Another note to the SLP gradient: It suddenly appears at the end of section 2.2 without prior mentioning. It should be introduced a little earlier along with the other variables (p6 | 32ff) including the reason to do so.

AU: We will add the information and improve the structure of the presented results.

8. For section 3, can you provide spatial statistics, such as the pattern correlation when you describe the resemblance of simulations with observational datasets?

AU: Yes, we will update the analysis with the pattern correlation.

9. Section 3.2, p. 12, | 19 "time-average over 1979-2005": Do you take care of any secular trend, which might be imminent in 26 years of data, but may disappear in a shorter time period?

AU: That is a valid point, which we would certainly take into consideration. However, in the updated version of the analysis we no longer use the 1979-2005 run. Instead we use new runs of present climate. These simulations are more suitable as they are set to follow the HAPPI protocol. We will update the revised version of the paper accordingly.

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3. Technical comments:

Some passages of the manuscript are not concise (e.g. repetition of methods in the text, when such details belong to the method section). Sometimes the manuscript does not read well.

AU: We will edit our text accordingly to improve clarity and readability of the manuscript.

1. Please check the references. There are references clearly missing, for instance Barcikowska et al, 2017 or Gilleland and Katz, 2014; or misleading like Feser et al., 2014 (did you mean Feser et al., 2015?). There might be more that I have overlooked.

AU: We will add and correct the references in the revised version.

2. Section 3.1 feels a little superfluous and could easily be merged into the method section.

AU: The section will be shorter and more concise in our revised version, as it excludes the analysis of internal variability impacts.

AU: We will apply all corrections, as suggested below.

3. p 1 | 32: the British Isles 4. p 4 | 21: Zappa et al. (2014) have shown 5. p 6 | 4: provided by the C20C+ Detection and Attribution Project 6. p 6 | 7: the CAM5-1-1degree run [...] and the CAM5-1-0.25degree run (missing articles) 7. p 6 | 9: add an "and" before 0.3125_x0.234_ 8. p 6 | 10: remove the last ")" 9. p 6 | 18: do you need commas in front and after the subclause "using atmosphereonly models?" 10. p 6 | 22: remove comma after offset 11. p 6 | 35: zonal wind; what about meridional winds or wind speeds in general? 12. p 7 | 17: Wilcoxon signed rank test, can you add a reason why you use it? 13. p 7 | 21: a block (seasonal) maximum 14. p 7 | 22: The whole sentence with "Assumptions that our analyzed data..." needs rewriting. 15. p 7 | 26: there is something wrong with (1-1/T)th (you accidentally inserted a comma) 16. p 7 | 33-35: please add a reference AU: We will apply the correction 17. p 9 | 24: will presumably lead to a 18. p 9 | 37: either from the model bias or from observational bias

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19. p 10 | 4: high-resolution runs provide a more accurate representation 20. p 10 | 20: provided for the years 21. p 10 | 20: internal SST variations being in a different phase 22. p 11 | 18-21: reflect, generally after reductions, CO2 increase 23. Section 3.4: This one reads very well (also applies to section 2.3).

24. Figure 7: What is a fractional difference?

AU: We will provide a definition of a fractional difference, which is simply percentage of change in a relation to the mean in a reference period.

25. Figure 9: The caption talks about 50 years. As far as I understood the manuscript, shouldn't it be less years? Maybe I did not get, where the 50 years sample comes from?

AU: 50yrs sample constitute a 5-member ensemble of decadal runs. We will clarify it in the revised manuscript.

26. Figure S2: There is either something wrong with the figure caption or there is a whole figure missing.

AU: Yes, we will correct the caption and also update the figure with an analysis using new present climate simulations.

Interactive comment on Earth Syst. Dynam. Discuss., <https://doi.org/10.5194/esd-2017-106>, 2017.

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