

# The role of serial European windstorm clustering for extreme seasonal losses as determined from multi-centennial simulations of high resolution global climate model data

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## Response to reviewer 2

Dear Reviewer,

We are very grateful for all of the comments and suggestions you have made to our manuscript, which have greatly helped improve its quality. We hope our revised manuscript addresses the points you have raised. Please find below a response to all of your comments and questions raised. Any page and line numbers refer to the initial NHESSD document. The italicised black text are the comments to the manuscript. Our responses are in red with any changes described. An amended version of the manuscript has also been uploaded to highlight the changes. In the marked version, text which has been removed has been struck through, with new additions being in red.

*This study uses a large set of HiGEM present-day climate simulations to estimate extreme seasonal losses due to windstorms and in particular due to the clustering of windstorms. As a main result, it is shown that the clustering of storms leads to peak accumulated seasonal losses that are up to 20% larger than if the storms were randomly occurring. This result is interesting and the methodological approach to use almost 1000 years of simulated data with a fairly high-resolution model is sophisticated. Therefore I recommend the study for publication. I was just somehow disappointed about the quality of the writing. Many sentences are surprisingly fuzzy (surprising, because of the excellent team of co-authors). I therefore ask the authors to carefully revise their paper with the intention to explain things better to the hopefully large future readership. Some clarity issues are mentioned below.*

### Comments:

1. *title and, e.g., p. 1 line 5: you call your model "high resolution". I understand why, but some colleagues think that high resolution is the km-scale, and in a few years this will be reality. I find the almost 1000 years that you have available for your evaluations more impressive than the HiGEM resolution of about 1 degree. Would it not be worth emphasizing this more in the title?*

We have changed the title based on this comment and comment 1 by reviewer 1. We have decided to keep the reference to 'high resolution' in the title as we agree that as 1km scale regional climate model are planned for the next decade, global coupled climate models will not be run at that resolution for some time. Hence, we have also specified that it is a 'global climate model' to retain that distinction. The title now reads 'The role of serial European windstorm clustering for extreme seasonal losses as determined from multi-centennial simulations of high resolution global climate model data'.

2. p. 1 line 3: "affect one area in a period of time" not sure that this is the best definition of serial clustering?

This has been changed to 'affect one specific geographic region in a short period of time...' in order to improve the definition.

3. p. 1 line 7ff: this is a very long a complicated sentence. Please rephrase. How can a "loss-based metric" be "based on meteorological variables"? Then it is a metric for loss, but not loss-based?

This sentence has been rephrased for clarity. It is now as follows: 'The role of windstorm clustering is investigated using a quantifiable metric (storm severity index (SSI)) that is based on near-surface meteorological variables (10-metre wind speed) and is a good proxy for losses. The SSI is used to convert...'

4. p. 1 line 9: here it is completely unclear that the 918 years do not correspond to one long transient millennium simulation but rather to a large ensemble of present-day climate conditions. Please clarify.

The wording of the abstract has been reworded to clarify this. It now reads '918 years of a present-day ensemble of coupled climate model simulations from...')

5. p. 1 line 13: "return periods" of what?

We have changed the end of this sentence to '...to the seasonal windstorm loss as a function of return period.' to clarify this.

6. p. 1 line 16: it is difficult to understand what you mean by "random realizations of the HiGEM data". This first sounded to me as if you were running HiGEM with randomly perturbed physics. Since this sentence, in my opinion, is the key result of the study, it is important to explain this "random realization" much better in the abstract.

This has been rephrased to add clarity. It now reads '...20% larger than the accumulated seasonal loss from a set of random resamples of the HiGEM data'. Changes have also been made in the text to refer to the raw HiGEM data being 'dynamically consistent' in order to be clarify the difference in the data.

7. p. 1 line 18: usually you give values in %, so why here not 25-50%?

We have changed all references to this result to be in % for the conclusions and abstract to be consistent with the other results in these sections. In the main text results are referred to as decimal to be consistent with the figures.

8. p. 2 line 3: US \$?

Yes, the values do refer to US \$. We have clarified that all values are US \$.

9. p. 2 line 7: "space of time" → "time interval"?

We have changed the wording, however it has been changed to 'in a short period of time'.

10. p. 2 line 26: "amounts of RWB" → maybe "frequency of RWB"?

The sentence has been changed, it has been changed to 'flanked by the presence of anomalous Rossby wave breaking (RWB)...'.

11. p. 3 line 4: word missing after "comprehensive"

This was also mentioned in comment 4 from reviewer 1. The words 'spatial coverage' have been added.

12. p. 3 line 17: *I am lost with this question. Do you mean "Does windstorm clustering contribute more to losses in seasons with large accumulated losses?"*

The question has been changed and now reads as 'Does windstorm clustering contribute more to losses in Europe for winter seasons with large accumulated losses?'. This change was also suggested in comment 5 by reviewer 1.

13. p. 3 line 20: *what is an "increasing return period winter season"?*

This is to refer to an increase in the return period of the total accumulated seasonal windstorm loss. This sentence has been rephrased to the following... 'Finally the importance of clustering for seasons with large accumulated European windstorm losses is addressed'.

14. p. 3: *is it a good idea to mix 4 x 59 years of transient simulations with decadal hindcasts? How can you justify that this leads to a good statistical distribution of present day climate variability? Wouldn't it be better to only use the decadal hindcasts as a more homogeneous dataset?*

The aim through using this mix of data is to utilise a large sample of present day climate. As we are treating each year as independent from the others this is a good approach. We have analysed the number of storms per year for all the data to ensure that there are no trends present. Hence all the data is suitable for the purpose which we are using it for.

15. p. 3 line 28: *I failed in understanding how you get in the end 918 years:  $4 \times 59 = 236$  years are from the transient runs; how do the remaining 682 years distribute across the 4 ensemble members initialized between 1960 and 2006?*

The remaining data is structured for the 4-member ensemble decadal hindcasts. The first set of hindcasts is initialised every 5 years from 1960 to 2005 (i.e. 1960, 1965, 1970, ..., 2005) and is run for 10 years. This makes up 400 years of data. The remaining years come an additional set of ensemble hindcasts initialised throughout the 1960-2005 time period.

16. p. 5: *there is unnecessary repetition between lines 9ff and 24ff.*

We agree. The sentence 'Changing the value of  $V^{98}_{i,j}$  provides a sensible threshold...' has been moved into the bulleted section of the SSI description. The sentence 'Following the method of...' below the equation has been removed.

17. p. 6 lines 5 and 6: *why are these probabilities? it seems that these are losses.*

These are stated as probabilities as these curves refer to the probability of seeing an event of the specific loss size in a particular year. We have used this naming convention in order to follow the standard that is used in the insurance industry, as is stated in the text.

18. p. 6 line 8: *spelling of SSI*

This has been corrected.

19. p. 6 line 14 and throughout the paper: *most likely also in NHESS "Figure" is written with a capital F.*

We have changed all instances in the text to have a capitalised F.

20. p. 6 line 27: *unclear, "storm track numbers" in what time period?*

The dispersion is calculated using the storm track density in units of the average number of storms per month in a single DJF period. A change has been made to the text so it now reads "This relates the variance ( $\sigma^2$ ) in storm track density (average number of storms per month

in a single DJF season) to the mean ( $\mu$ ) storm track density'. This was also made in comment 8 by reviewer 1.

21. p. 7 line 28: why "so"?

The 'so' has been removed from this sentence.

22. p. 8 line 17: "M. and G. (2009)"??

This was an error and the reference has been correctly modified to be that of 'Della-Marta and Pinto (2009)'.

23. p. 8 line 23: not sure that predictability can have skill, maybe "skill in ... predictions"

We agree, the sentence has been rephrased to 'the model has skill in seasonal to decadal predictions...'.

24. p. 8 line 27: "contrast" -> maybe "agree"?

This has been changed in the text to now read 'agree well...'. This was also suggested in comment 12 by reviewer 1.

25. p. 9 lines 14-30: here the link to the rest of the paper is not immediately clear and there seems to be some repetition between the two paragraphs. I suggest to merge and shorten them and to explain the reader why this analysis of RWB is relevant for the main part of this study.

A further clarification has been added to the start of the first paragraph in an attempt to frame this section of analysis better with the rest of the paper. This reads as 'It was shown in Priestley et al. (2017a) that clustering events occurring at different latitudes of western Europe are associated with a specific set of dynamical conditions. The clustering periods identified are characterised by a strong and extended...'. Further on we have also added the following 'This analysis has been repeated for all 918 years of HiGEM data in order to assess how well HiGEM dynamically represents these events (figure 2). The same analysis of ERA-Interim is shown in the supplementary material (figure A1)' in order to add further motivation for this section of analysis.

26. p. 10 line 10: how can 10-m winds be compared/contrasted with 925-hPa winds?

This is to illustrate a common difference in datasets with wind speed biases at the surface (10-metres) and low atmosphere (850, 925 hPa). We have amended the sentence in order to clarify this. It is now as follows 'Similar differences have also been found in the biases of 10-metre and 925 hPa wind speeds in four reanalysis datasets (Hodges et al. 2011)'.

27. p. 10 line 20: here "HiGEM" is used as a symbol for wind speed (if I understand correctly), which is strange. Before you used  $v_{i,j}$  - the same could be used again. Instead of " $v_{bc}$ " I suggest to use e.g.  $\bar{v}$ . And  $\overline{p_{98}}$  is not explained.

The use of  $p_{98}$  has been changed to  $V_{i,j}$  in order to make equation 3 consistent with equation 1 and also the rest of the text. We prefer to keep the HiGEM<sub>bc</sub> notation. A clarification of the overbar has been added in the text, which reads '...wind speeds by the spatially averaged offset in the  $V^{98}$  field between ERA-Interim ( $V^{98}_{ERA-i}$  (overbar)) and HiGEM ( $V^{98}_{HiGEM}$  (overbar)) for all land grid points...'.

28. p. 10 line 24: west -> east?

Yes, this has been changed to 'east'.

29. p. 10 line 26: delete "how"o

The 'how' has been removed.

30. p. 11: *I am not an expert in statistics, but is it a good idea to calculate return periods of 50y from a 36-y dataset?*

This is the reasoning for discussing the 50yr return period. We discuss the fact that making estimation of losses of greater than 50 yrs with a reanalysis product is almost an impossibility. Hence, using a model like HiGEM with 918 years of data is worthwhile.

31. p. 11 line 24: *I don't understand "the increase in the largest event is by ... 100%"*

The wording of this sentence has been changed to improve the clarity. The sentence is now 'For example, the 918 year return period season in HiGEM is approximately twice the magnitude of the 1 in 36 year season in ERA-Interim.'. This correction is made with suggestions also from comment 16 from reviewer 1.

32. p. 11 line 33: *what is "very marginally"?*

This sentence has been slightly rephrased in accordance with a similar comment from reviewer 1 in order to improve the clarity of this sentence. Details can be seen in the response to the comment from reviewer 1.

33. p. 12 line 4: *should read "clustering to the ..."*

This has been changed to 'clustering to the...'.

34. p. 12 line 5: *to me the notation "AEP\_random" looks a bit like computer code. Why not AEP with subscript r?*

We prefer to keep the initial notation of 'AEP\_random'.

35. p. 12 line 29: *spelling of "entire"*

The spelling has been corrected.

36. p. 13 line 1: *"as a result" can be omitted*

The wording 'as a result' has been removed.

37. p. 14 line 17: *not sure that I understand "scaling by 18.75%". I would understand "scaling by a factor of 1.1875" or "uniform increase by 18.75%"*

All mentions of the scaling have been changed to 'uniform increase' in the text.

38. p. 14 line 31: *to me, the 10-20% effect of clustering is surprisingly small. I find it an interesting result that this effect is not larger. Maybe you can discuss this a bit more. I then find the "strong implications" on p. 15 line 2 a bit exaggerated, since 10-20% might be below the general uncertainty level (for instance in the evolution of population density).*

The 10-20% has large implications. If you consider that the average insured loss from European windstorms is ~€1.5 billion then the difference of 10% would be an underestimation of losses by €150 million. This is just the average for years within recent living memory, so for the larger loss seasons which are present in HiGEM, this would equate to a considerably larger value. Hence a misrepresentation by loss modellers would result in a vast underestimation of losses in a monetary sense. The monetary implications have also been included in the text. We have also removed the word 'strong' from 'strong implications'. With regards to the uncertainty of population density we have done additional analysis to show this result is actually rather insensitive to the population density scaling (see figure 1 below) and the same conclusions can be made. With this in mind, we do not

believe any differences in evolution of population density in recent years for central and western Europe over the last 30 years (for which the population density is available) would result in different results to those we have presented.

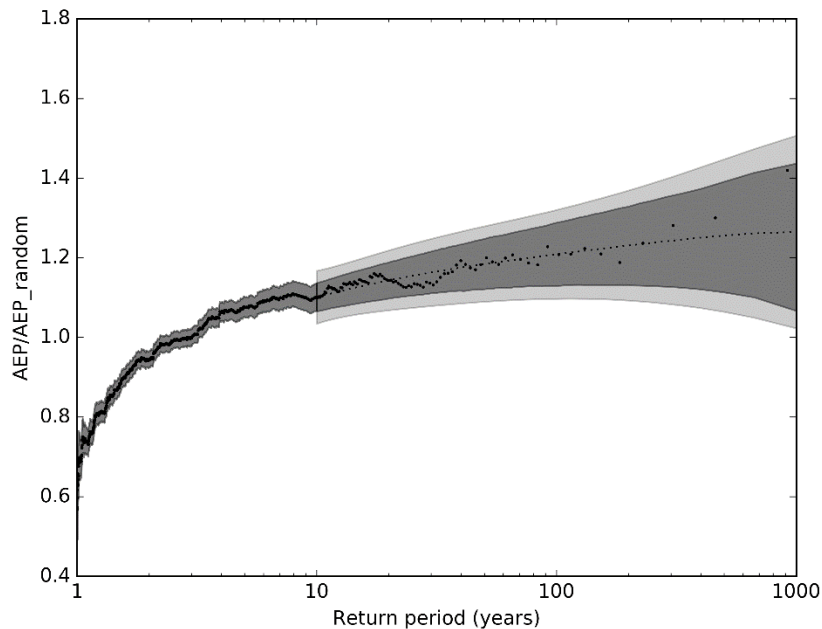


Figure 1. As figure 7 in Priestley et al. (2018), but with no population density scaling applied.

## References

- Della-Marta, P. M. and Pinto, J. G.: Statistical uncertainty of changes in winter storms over the North Atlantic and Europe in an ensemble of transient climate simulations, *Geophysical Research Letters*, 36, <https://doi.org/10.1029/2009GL038557>, l14703, 2009.
- Hodges, K. I., Lee, R., and Bengtsson, L.: A comparison of extratropical cyclones in recent reanalyses ERA-Interim, NASA MERRA, NCEP CFSR, and JRA-25, *Journal of Climate*, 24, 4888–4906, 2011.
- Priestley, M. D. K., Pinto, J. G., Dacre, H. F., and Shaffrey, L. C.: Rossby wave breaking, the upper level jet, and serial clustering of extratropical cyclones in western Europe, *Geophysical Research Letters*, 44, 514–521, <https://doi.org/10.1002/2016GL071277>, <http://dx.doi.org/10.1002/2016GL071277>, 2017a.
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