

***Interactive comment on* “The role of European windstorm clustering for extreme seasonal losses as determined from a high resolution climate model” by Matthew D. K. Priestley et al.**

Anonymous Referee #2

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Review of paper

"The role of European windstorm clustering for extreme seasonal losses as determined from a high resolution climate model"

by M. D. K. Priestley et al.

submitted to NHESS

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

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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:

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?

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

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?

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.

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

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

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is important to explain this "random realization" much better in the abstract.

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

p. 2 line 3: US \$?

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

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

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

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?"

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

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?

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?

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

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

p. 6 line 8: spelling of SSI

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

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

p. 7 line 28: why "so"?

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

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

p. 8 line 27: "contrast" → maybe "agree"?

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.

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

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 " bc " I suggest to use e.g. \hat{v} . And p_{98} -overbar is not explained.

p. 10 line 24: west → east?

p. 10 line 26: delete "how"

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?

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

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

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

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

p. 12 line 29: spelling of "entire"

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

p. 14 line 17: not sure that I understand "scaling by 18.75%". I would understand

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"scaling by a factor of 1.1875" or "uniform increase by 18.75%"

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).

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