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Second revised version of: "A hazard model of subfreezing temperatures in the United Kingdom using vine copulas"

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Dear author and editor,

overall I found the paper improved. In particular, I am satisfied with the changes that were done with respect to my previous comments. I would suggest trying to improve the quality of the writing, maybe asking some colleague to read the paper would help (as this is a single author paper). While I think that the paper improved with respect to the previously presented analysis, the author introduced a non-stationary model. I like the idea of employing this model, but I have some main comments about it. According to me, the author should provide a discussion about some issues. However, if the discussion cannot be satisfying, I would suggest considering the publication of the paper without the non-stationary model.

Main comments about the non-stationary model.

- I have concerns regarding using the CO₂ as a predictor for the local UK warming. While global average warming is correlated with CO₂ concentration, the global average warming can differ from the average local warming and even more from the local (potential) warming of the extreme cold events (being the latter the focus of the study). Therefore, I think that using a predictor of global warming (CO₂) as a predictor for changes of the local cold extreme temperature in the UK is problematic. Especially when the author extrapolates information about the future climate based on changes in CO₂. The author himself highlights that under global warming (increasing CO₂) the Arctic amplification has been occurring, and this might lead to non-linear responses of the local UK climate. In particular, it is under debate whether very cold winters might be experienced in the UK in the future as indirect result of the increasing average global temperature. Thus, it is clear that potentially relevant physical mechanisms are not included within the selected predictor (CO₂). This, might lead to a mismatch between the predicted future climate and the real future climate. Thus, personally, I would not use the CO₂ as a predictor at all, and especially I would not present results for the future. An alternative would be to strongly highlight the limitations or - if possible - justify the use of this predictor, and in any case to have a dedicated discussion on that. I believe that the decision is up to the editor.
- Also, the non-stationary model does not consider potential the non-stationarity in the dependencies described by the joint pdf, and this would need to be highlighted better (at the moment this is only discussed at the end of the results for NAO only, and not for CO₂, while this would need to be said in the methods too).
Also, according to my understanding, the dependencies described by the copula in this model are not typical spatial dependencies between the locations as in the stationary model, and this is not discussed anywhere. (The marginal pdfs include predictors through linear models. Thus, to my understanding, the uniform variables

(obtained from the marginals) modelled by the copula are not the usual ranks associated with the AFI in each location. It seems to me that the uniform variables are rather the ranks of the residual of the linear models. However, this in the parenthesis is only my interpretation, while I think that the reader should not interpret this, rather it would be better if the author provides an explanation.) I think that a discussion on this is needed, such that it is made clear what the model is actually modelling (also considering whether this might lead to limitations).

Minor comments:

P1 I9, rephrase

P1 I10, “such an event”?

P2 I4, I would delete “entire”. As also the author says later, the NAO affects mostly the weather around the Atlantic basin. The AO, on the other hand, influences the weather over the entire Northern Hemisphere.

P3 I18, this sentence about the wind is confusing me and seems, to me, out of context.

P5 I13, at the end: “period” -> “analysed period”?

P6 I1 I would start with “Based on the AFI, the winter ...”

P10 I5, it should be made clear in the text that also the copula is stationary.

P11 I11 Please, consider to specify the percentages for μ , P_0 , and in total separately if you think that this would lead to some interesting additional considerations.

Paragraph P15 I4

- Should “RVM” be “pdf”?
- Sentence “Performing...”, rephrase
- “long enough to neglect the Monte Carlo uncertainty”

(P15 I15 Thought: it could have been possible to employ a more standard RCP scenario for future, defined based on the DeltaF, rather than on the change in CO₂.)

P16 I14

“Instead of 100K”, please make a clear link to what is said at the beginning of the section, where you say that 100K would be necessary to reduce the Monte Carlo uncertainty.

Probably you could simply move the Paragraph on P15 I4 here.

P16 I14

I would start the sentence with “In order to investigate...” and then say that you separate the uncertainties.

Section 4.1,

- Please, make clear that you are referring to the mean return period when you say “stochastic set”
- I guess that you are simulating from the vine and then transforming the simulated uniform variables to the “real” via the marginal models containing the predictors/covariates. Here you fix the CO2 predictors to certain values, but what value is given to the NAO predictors? Please, make this clearer. Also, please make this procedure clear in the method section.

Fig10 The grey uncertainties are not well visible on my printed version. Please, check if this is only a problem of mine.

P20 I11 “originates”, I would write “appears to originate”

Table 4 missing the unit: “years”

P21 I1 see the main comment on CO2 above

P21 I12 “return period of 1 in 39 years”, is it not enough to write “return period of 39 years”? Also in other parts of the paper?

P21 I12 How is it a positive and negative phase defined? What is the value of NAO that is used to sample the data? In the figure it is written $>$ or $<$ 1, but do not you use a unique value, e.g., 1 and -1? Please, make this clear.

P23 I3. The predictor influence on the dependence is not considered. Indeed it is specified here only for the NAO, but not for the CO2.

P24 I9 the occurrence has increased? The return period has increased. The same in the abstract.

P24 I16 “such extreme events”, not clear which extreme events.

Best regards.