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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Improving snowfall representation in climate simulations via statistical models informed by air temperature and total precipitation" *by* Flavio Maria Emanuele Pons and Davide Faranda

Anonymous Referee #2

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Ponds and Faranda present and assessment of statistical models in order to better represent the snowfall using data from gridded-climate products. The assessments include Europe and comprise the winter months of December, January and February. ERA5 reanalyses between 1978 and 2018 and IPSL-WRF between 1979 and 2005 were used to evaluate the statistical models. Overall, the methods presented improve the representation of the snowfall, and due to the characteristics of the methods, an important advantage is its applicability to larger areas using gridded climate products. Although the overall research fit in the journal, there are several points that need to be

addressed. More importantly, I think that a better rationale for this works is necessary as well as some changes in the structure and/or a reduction in the length. In view of this, I suggest mayor revisions. I do really hope that this review will be useful to the authors to improve their manuscript.

It is not clear why is important to use these statistical models. Extreme events and its impacts (e.g. February 2018) are barely mentioned in the Introduction and also the limitations in represent snowfall events of the Climate models using emission scenarios. What is the main goal of applying these methods? For the experiment presented, it seems that is to find a better representation in climate change models scenarios. But, It is applicable to weather forecast as GFS for instance? It would be useful to have a better idea of why this work is important. To extend the rationale of the work presented.

Some sections are hard to read. The Introduction seems more appropriate for a review paper, maybe avoid the details of each earlier model. Method section also seems lengthy and hard to follow, I suggest to try to go to the point. Results also contain information that is not appropriate for this section (see below). I suggest re-restructuring the manuscript, and move some sections to a Supplementary Material in order to reduce the length of the manuscript. Maybe a Discussion section will do the manuscript more clearer moving some Results and Conclusion paragraphs to this section, allowing concentrate (and reducing) the Conclusions to the main findings.

Additionally, please note that some literature refers to the "separation of snowfall" as "precipitationâĂŘphase partitioning methods (PPMs)" (e.g. Harder and Pomeroy, 2014). Consider in to use this terminology.

Other comments (line number indicated):

16: This was already mentioned in Line 12.

25-30: Add a more specific statement about why statistical models are better than physics parameterization or simplifications in climate models. Is this just a thought? or

Do you have more evidence of this? some references?

50: “Hemispheric”.

58-59: Worth to mention that solid precipitation also depends on relative humidity and could be useful to estimate the mixed-phase or sleet (e.g. Ding et al., 2014).

116: Change “Section ??” to “Section 4”.

125-128: This paragraph sounds like a better justification of the work and fits with the overall aim of the journal. Consider to move to the Introduction section and explain a bit more the justification of this study.

134-135: The election of these months is a bit contradictory if the aim is to analyse extreme events causing disruption. Of course, extreme events largely occur in winter, but over other seasons (end of Autumn, the beginning of Spring) extreme snowfall events could occur. For instance, the so-called “Beast from the East” in 2018 was between the end of February and beginning of March. If analysis months is not extended maybe change the title to “Improving winter snowfall representation. . .”

147: Delete extra “( )”.

241: Numbering missing.

246: Delete extra “( )”.

509: Fig. 2 is mentioned here before Fig. 1. Also change to “Fig. 2” to be consistent with the rest of the manuscript.

526: You refer here to Fig. 2 I guess not Fig. 1.

551: Most of section 4.2 seems more appropriate to the Methods section (or Supplementary Material). For instance, lines 575-585 is an explanation of the Kruskal-Wallis H test, lines 593-601 is an explanation of the U-test. Here, you must show the results after applying these tests.

536: Numbering missing.

737-755: These paragraphs are a repetition of previous statements already presented in the Introduction and related to limitations of previous methods, observational data and the physics of the climate models. I think is not necessary to repeat in the Conclusions.

Figure 7d): Legend is not visible.

Figures: please add units and names to the axis where these are missing.

References

Ding, B., Yang, K., Qin, J.,Wang, L., Chen, Y., and He, X. (2014). The dependence of precipitation types on surface elevation and meteorological conditions and its parameterization. J. Hydrol. 513,154–163. doi: 10.1016/j.jhydrol.2014.03.038

P. Harder, J.W. Pomeroy (2014) Hydrological model uncertainty due to precipitation-phase partitioning methods Hydrol. Process., 28 (2014), pp. 4311-4327, 10.1002/hyp.10214

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