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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 #3

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Review:

Improving snowfall representation in climate simulations via statistical models informed by air temperature and total precipitation (2020)

By Pons and Faranda

General Comments:

In this manuscript, the authors test existing methods for the estimation of the apportionments of rain/mixed/snowfall and present a more robust way of selecting threshold temperatures to be used in single/multiple threshold models. The tests are performed based on the reanalysis ERA5 dataset and the IPSL_WRF climate projection model for the period 1979-2005, and at 0.25-degree spatial resolution in Europe, including the Scandinavian peninsula. Daily temperatures are used for the models.

In general, I find that the authors did a good job explaining the different methods and performance analyses, and as a data exercise, the procedures are somehow clear in the manuscript. However, I find that there is a disconnect between the objectives and how realistic it would be to apply these models over such domains and spatial resolution. I also find that the use of the reanalysis data as an "observation" (e.g., Figure 5) to test all the models against can easily be challenged given the uncertainties in such datasets, especially to determine snowfall in complex terrain.

I would like to point out that the type of models that the authors are applying are generally derived from meteorological data at in-situ stations, while the article refers to modeling grid scales of several km (0.25 degrees, which at 70 deg lat is $\sim$ 10 km in E and 28 km in N coordinates, https://www.opendem.info/arc2meters.html). How can the same type of models be appropriate for both scales? A gridcell of such dimensions would easily cover a mountain valley and surrounding mountains, with a very wide elevational range. Throughout my reading, I just kept wondering how such models could be applied in this spatial context. One would expect that over the domain of a single gridcell, one would potentially encounter a portion of the area to receive rainfall, while another portion would receive snowfall and a middle portion would be in the transitional zone if an event occurs around the freezing point. I find this to be very problematic in the context of the manuscript.

Another relevant issue has to do with the use of daily temperatures for the models, given that sub-daily temperature fluctuations would have marked effects on precipitation phase apportionments. Also, what would the near surface temperature be representative of in the manuscript? A mean elevation? If so, would this be realistic? I argue it is not, especially to illustrate how the proposed methods can enhance the estimation of the model's parameters.

As snow hydrologists, in our group's modeling efforts we use a similar two-temperature threshold model to estimate the precipitation apportionments at grid scales between 10-100 m in mountainous terrain, with a linear model between the two thresholds. Even at such scales, we understand that there are drawbacks to such model, but the uncertainties in precipitation amounts and temperatures are primary, and the model for the precipitation apportionments takes a secondary role. However, even at such scales, determining the performance of the model and parameters is very challenging because of the difficulties in determining accurate precipitation amounts, particularly snowfall.

Threshold values also seem unrealistic in some locations, as low as -15 or as high as 5+ C. There are examples of snowfall events at high temperatures (e.g., late June 2019 summer events in Colorado), but as a generalized modeling threshold it would seem unrealistic to have such high and/or low values. This would also highlight the issues regarding the data and spatial scales of the analysis.

Because of these issues, I am recommending that the manuscript be rejected. I ultimately consider that the results in the manuscript do not accomplish demonstrating how the proposed methods deliver improvements in model accuracy.

Specific Comments:

ll. 35-36: Odd sentence.

l. 52: typo in “observationa”.

ll. 74 & 79: I suggest using past tense to refer to findings or proposed models, as the ones in these lines (Pipes and Quick (1977) and L'hôte et al. (2005)).

l. 116: typo “Section ??”

l. 122: I suggest adding a comma before “which”, but the sentence would need changes.

l. 125: what are you calling “large scales” here? Suggest clarifying.

l. 141: “worth mentioning”.

l. 291: suggest changing “It is easy to prove” to “It can be shown”.

l. 259: Revise “The latter resulted not enough numerically stable”.

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