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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 #1

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The manuscript by Pons and Farada assesses the performance of several snowfall separation methods to reproduce simulated snowfall in the ERA5 reanalysis on a European scale by taking into account simulated near-surface air temperature and total precipitation at daily resolution. The two best-performing methods are in a second stage applied to bias-adjusted output of the IPSL-WRF regional climate model (historical period) to obtain a bias-adjusted estimate of simulated snowfall in the RCM. The evaluation reveals a satisfying representation of the PDF of the daily ERA5 reference snowfall amount in the historical period by the bias-adjusted and separated IPSL-WRF

simulation.

Overall, the paper fits well into the journal's scope. Data and methods are for most parts clearly introduced and explained. The presentation of the results has some weaknesses but is still acceptable. The major drawback of the work, however, is the unclear relevance of the work for a broader audience and for RCM snowfall bias-adjustment. Essentially, the authors search for a method to emulate the ERA5 microphysics scheme that simulates the actual snowfall flux in the reanalysis model taking into account simulated near-surface temperature and simulated total precipitation only. The two best performing methods are then applied to a different model (IPSL-WRF) to separate snowfall from total precipitation after bias-adjustment of simulated temperature and precipitation. Results look satisfying, but there is

(1) no evaluation of the ERA5 snowfall flux (which is the basic reference in the entire work, and the entire analysis is geared towards a reproduction of ERA5-simulated snowfall flux; the paper frequently uses the term "observed" for ERA5 snowfall flux, although it is essentially a simulated flux probably subject to systematic biases)

(2) no analysis to what extent the satisfying results of the application of the method to the RCM are specific for the chosen RCM and the bias-adjustment method of temperature and precipitation that was carried out beforehand (a different RCM might, even after bias-adjustment, have a completely different multi-variate structure of daily temperature and precipitation, at least a structure that is different to ERA5, and the method might not hold in these cases

(3) no discussion of potential problems with inter-variable dependencies even after bias-adjustment of an RCM (-> see, for instance, Meyer et al., HESS, 2019)

(4) no indication if the identified methods will also produce robust snowfall estimates in a future climate change scenario (which is, as far as I can guess, the basic motivation of the entire work -> a possibility to investigate such an applicability would be to split the ERA5 period into "cold" and "warm" years and to calibrate on the cold and validate

on the warm sample)

(5) no reference to differences in spatial resolution of the models employed and the fact the subgrid orography can actually have a considerable influence on simulated snowfall (or, the other way round, neglecting subgrid scale orographic variability in model bias-adjustment could results in false derived snowfall sums)

(6) no analysis of a calibrated threshold within the "naive" STM method (which I assume could yield even better results than the two best identified methods, as even the performance with a fixed 2°C threshold is very close to the two best-performing methods), and

(7) no analysis of the importance of variations on the sub-daily scale which might be important for daily snowfall sums.

The main message of the manuscript is currently, that for this specific setup (this specific RCM, this specific bias-adjustment method, this specific reference snowfall), the two identified methods if applied to bias-adjusted IPSL-WRF temperature and precipitation output can yield a representation of snowfall that well reproduces the ERA5 reference snowfall. These results are in my opinion not per se transferable to different models or to a future climate scenario or to a different reference snowfall (especially not to a true observation-based snowfall estimate). As such, the value of the work is limited for the time being in my opinion and not too informative for a broader readership. I would hence recommend to return the manuscript to the authors for major revisions. During these revisions, the mentioned points should be picked up in order to increase the relevance of the work. A couple of further issues are mentioned below.

With kind regards.

FURTHER ISSUES:

Line 24: Very unclear what is meant.

Lines 35-36. Also rather unclear.

Lines 38-40: This is actually not true, the entire set of so-called "perfect prognosis" downscaling methods is ignored here. These do not adjust the simulated variables towards observations but exploit calibrated relationships between observed (or reanalysis-simulated) large scales and observed local scales.

Lines 141-142: Very unclear.

Line 145: Above (line 132) you mention that only daily data are used, here you obviously employ hourly data. Please clarify.

Line 151: "grid step" unclear

Line 181: Rather unclear what is meant by "standardized temperature anomalies" and why these are used.

Chapter 3.1.2: This sub-chapter contains a large amount of rather technical information, which is appreciated, but which should be moved to some technical appendix I believe.

Line 517: Do you have any explanation for these rather low calibrated tresholds? Is there a relation to orographic height, for instance?

Line 526: Should be "Fig. 2" instead of "Fig. 1".

Lines 688-689: Very unclear.

Lines 707-708: Better representation of the tails is not really apparent from the figure I'd say.

Figure 1: Color scale is not very intuitive.

Figures 2 and 3: Bad color scale: White color means threshold temperatures around 0°C but also "not applicable". I'd suggest to modify the color scale.

Figure 4: Legend too large. Also, the methods are named differently compared to Table 1 and are sorted in a different order. Please harmonize. Also, it would be good to use

the same unit in the lower panel as in Table 1 (10ˆ-3)

Figure 5: Upper panel: Please use the same sorting of methods as in Table 1.

Figure 6: Very bad color scale, not at all intuitive. Also, the color scale should be identical for all panels to enable a comparison (same color should mean the same value in all panels). Is the unit actually m/27 years (1979-2005) or m/year? Please clarify.

Figure 7: Legend of lower panel too small.

Figure 8: What about the bad-performing grid cell in northern Italy in logit seq and cubic spline? What is happening here?

Figures 9 and 11, upper panels: Sorry, but even after reading the explanation several times it is not really clear to me what is displayed here. Also, I'd suggest to use a white background instead of a black background. Lower panels: Please specify the unit of the x-axis.

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