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Interactive comment on "Snowfall in the Alps: Evaluation and projections based on the EURO-CORDEX regional climate models" *by* Prisco Frei et al.

Anonymous Referee #3

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Title: Snowfall in the Alps: Evaluation and projections based on the EURO-CORDEX regional climate models

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Recommendation: [Revision]

GENERAL COMMENTS:

This article assesses the performance of (an ensemble of) simulations with regional climate models conducted in the framework of EURO-CORDEX in representing snowfall

characteristics in the Alps and the surrounding regions. It also discusses the changes in snowfall projected by the model ensemble under the RCP8.5 and 4.5 emission scenarios. Rather than taking snowfall from the raw (or native) model output (which is not available for all RCMs) is derives snowfall amount from total precipitation and near surface temperature. The newly develop method also accounts for the effect of subgrid-scale orographic variance. The authors show subsequently that a form of bias adjustment in precipitation and temperature for each GCM-RCM member separately is needed to allow a meaningful comparison with an 'observed' reference state.

Overall the paper is coherently written, and the methodology applied to bring model output from the various GCM-RCM simulations in a common framework regarding snowfall characteristics is adequately presented and properly motivated. The work is not entirely novel, many of the findings confirm conclusions from previous studies. The strength of the paper is that the authors developed a well-structured approach to assess the performance of a variety of models, each with its own characteristics, in representing a relevant climate parameter which is difficult to access through direct observations.

Having said that, I think the authors have still a bit of work to do. Throughout the paper I came across a number of issues which require some further attention from the authors before the paper can be accepted. Yet, I am sure that after the authors have adequately addressed my points of concern a revised manuscript will be suitable for publication.

MAJOR COMMENTS:

1. Half-way the introduction (lines 75-80) the authors write " Within the last few years . . ." followed by "Most of these analyses are based on GCM output or older generations of RCM ensembles at comparatively low spatial resolution . . .". This may indeed be the case for most of the studies cited in the sentence before, but not for all of them, and the authors should specify explicitly which of them is the exception, and how that study

compares with their work. E.g. Piazza et al. use, amongst other models, a number of RCMs operated at 12 km resolution, the study by de Vries et al. (2014) is based on a 8-member ensemble of EC-EARTH-RACMO simulations at 12km resolution (historical and rcp8.5) configured on a smaller domain, but in principle quite comparable to the simulations used in this paper.

2. Instead of using “bias correction” I would strongly recommend to use the phrase “bias adjustment” as was also the adopted terminology by the EURO-CORDEX community. The word “correction” suggests there is a well-established methodology including a ground truth observed state which we all agree on. This is obviously not the case. It is therefore much better to use the word “adjustment” which automatically triggers the questions “how” and “to what” or “in which context” as it should be.

3. In section 2.2 it is mentioned that all GCM-driven EUR-11 simulations for which control, RCP4.5 and RCP8.5 runs are currently available have been included in the study. This can obviously not be a correct state of affairs. Currently means “at the moment” and since the number of simulations published in the ESGF-archive is still growing, there will be a moment that the statement is no longer true. In fact, already in October 2016 there were 16 simulations that met the criteria set by the authors. In addition to the their selection there were results published from HadGEM2-RACMO and MPI-ESM (r2i1p1)-REMO. In April 2016, none of the two MPI-ESM-REMO simulations were available, but the HadGEM2-RACMO simulation was, albeit based on version v1 which was replaced by version v2 in August 2016. So, a) you need to specify currently, and b), either include the simulations that were in the archive but not in your selection, or convincingly explain why some simulations were not selected.

4. Following the previous point, it is important mentioning that three different realizations of ICHEC-EC-EARTH are used to force four different RCMs: r12i1p1 is used to force CCLM and RCA, r3i1p1 is used for HIRHAM, and r1i1p1 for RACMO. Different realizations of a GCM can show distinct behavior owing to long-term large-scale natural variability implying that differences between the EC-EARTH forced RCM simulations

are not only due to differences in the RCMs. This would only hold for the CCLM end RCA simulations forced with r12i1p1. Please, mention this aspect when introducing the 14 GCM-RCM chains.

5. In section 2.5 the methodology to separate snowfall from total precipitation is discussed (Richards method). In the final paragraph a parametric formulation fs,Ri is introduced (Eqs 4-7) to express the snowfall fraction in terms of coarse-grid temperature Tk, the topographic standard deviation σh of the designated grid cell, and a number of constants (E,F,G,H) which are determined through an empirical fitting procedure. The function fs,Ri is meant to be used to separate snowfall in the RCMs as well (line 282-283). Since the subgrid-scale orographic variance parameter of the model orography is not known (at least this parameter is not in the ESGF-archive) I presume the authors have used σh from the observational dataset. Somehow the observational σh and model height should match. This is not guaranteed a priori. The authors already mention that orography fields from different RCMs can be quite different from each other, and from the observations (line 157-159). The authors should explain how they deal with such mismatches.

6. In section 2.6 the bias adjustment approach is discussed. I was surprised to see that after the detailed treatment of snowfall separation, the adjustment of temperature has been dealt with so crudely. While according to Fig S4 the temperature bias considerably depends on elevation the authors have chosen the shifted fractionation temperature to be independent of elevation. According to Fig 4 the adjusted fractionation temperature and the temperature bias (one point per GCM-RCM realization) show considerable scatter around a linear relation, but it is not at all clear and also not explained what causes this scatter. It might be due to bias depending to elevation, but also to month of the year and/or region. Or is there something else? The authors should discuss their treatment of shifting the fractionation temperature in more detail, it is particularly relevant because the snow fractionation temperature appears to be a crucial, and probably also a sensitive, parameter in the analysis.

7. Line 565-566. The sentence “Previous studies . . . with this theory (e.g. Allen and Ingram, 2002; Ban et al. 2015)” is completely out of context. I strongly suggest to omit this sentence and the corresponding references. The focus of the Ban et al. paper is on summertime convectively driven sub-daily (hourly) precipitation extremes and its relation with temperature. This is miles away from the Sq99 and S1d snowfall parameters used in this paper to indicate heavy (but not extreme) snowfall at the daily scale outside the summer season.

MINOR COMMENTS:

1. In the introduction line 46-47: “First, total snowfall sums are expected to decrease by a decreasing probability for precipitation to fall as snow and a decreasing snowfall fraction (ratio between solid and total precipitation).” I do not see the difference between the two reasons, I would say that the first reason implies the second. Suggest to retain only the first part.

2. Line 107: rephrase “On centennial time scales” as “On decadal to centennial time scales”

3. Line 157 reads “It is important to note that each of the six RCMS considered . . .” I presume six must be seven in accordance with Table 1 and line 152.

4. Throughout the text subgrid should be rephrased as subgrid-scale. (that is apart from the few places where scale was already appended)

5. Line 174: “present day” → “ present-day”

6. Line 196: “were initially set”. What is the meaning of “initially”. I suggest to write just “are set”

7. Line 203. “possible” → “possibly”

8. Line 245-246: According to the text “the Binary method underestimates Smean and overestimates Sq99 for all elevation intervals (Fig.2.)”. Looking at Fig 2 this is not true

for Smean at the highest elevations. Adapt text accordingly.

9. Connected to the previous point, in caption Fig 2 the text reads “. . . and the full subgrid snow representation” I presume this is the same as the reference snowfall SSG which is used in the main text. If correct, use same terminology in caption as in main text, if not correct, explain the meaning of full subgrid-scale snow representation ?

10. Line 247: “. . . even for coarse grid temperature . . .”→“. . . even for a coarse grid temperature . . .” or “. . . even for coarse grid temperatures . . .”

11. Line 258: “with C the point of inflexion, and the growth rate D.” →” with C the point of inflexion, and D the growth rate.”

12. In line 339 the text mentions “250 m-elevation intervals in the range 950-1650 m”. I don’t see how 250-m intervals match in the range 950-1650 m” Please explain.

13. Line 362: “. . . elevations (Fig. S1).” Presume this must refer to Fig. S2.

14. Line 364:”. . .observations approximately linearly depends on elevation” rephrase as “. . . observations depends approximately linearly on elevation”

15. Line 376-377: “However, a potential undercatch can only partly explain the overestimation of precipitation found in the present work.” Please, discuss why it can only be a partly explanation, and not the full explanation

16. Line 430: “This again highlights the fact . . .”. highlights is too strong a word. Suggest to use illustrates.

17. Line 468 (connects to main point 4). “(e.g. the four simulations driven by EC-EARTH)” As pointed out before, the EC-EARTH forcings were inferred from three different realizations.

18. Line 473: “.. in Fig.8 that . . .” → “ .. in Fig. 8 which . . .”

19. Line 483: rephrase "... which forces the multimodel mean change to be slightly positive in January ..." as " ... which leads to a slightly positive change in multi-model mean for January ..."

20. Line 485: "... change sin ..." → " ... changes in..."

21. Line 506: "... mutlimodel ..." → " ... multi-model ..." (multimodel requires a hyphen everywhere)

22. Line 534: " ... and > 2000," → "... and > 2000 m a.s.l."

23. Line 595: rephrase "This is approximately given ..." as "This behavior is approximately found ..."

24. Line 639-640: "(which is for many GCM-RCM chains not available)" For most model chains it is available so many is a too strong qualification. Suggest instead "(which is not available for all GCM-RCM chains)"

25. Line 690: "We here confirm this conclusion." Too strong phrasing, please write "We here confirm this finding."

26. Line 690-691: rephrase " ... the optimal temperature for heavy snowfall will still occur in a warmer climate and, hence, ..." as " ...heavy snowfall in a warmer climate will still occur in the optimal temperature range, hence, ..."

27. Line 700: "... snowfall-friendly temperature range ..." Funny wording!

28. It would help if e.g. Fig 3a contains an ancillary line indicating the relative occurrence of the 250-m elevation ranges for Swiss and/or the Alpine region (based on the topography map in Fig 1) with labels on the upper horizontal axis.

29. The colour scheme used in Figs 7 and S5,S6,S7 is lacking contrast. Would it be possible to use a wider and somewhat lighter colour scheme?

30. Caption Fig 2: "factionation"→"fractionation"

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