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The Cryosphere Discussions

Interactive comment on "Snow water equivalent in the Alps as seen by gridded datasets, CMIP5 and CORDEX climate models" *by* Silvia Terzago et al.

Anonymous Referee #2

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Review of "Snow water equivalent in the Alps as seen by gridded datasets, CMIP5 and CORDEX climate models" by S. Terzago, J. von Hardenberg, E. Palazzi and A. Provenzale.

Recommendation: Major revision

In this paper, the authors assess the snow water content in the Alps as represented in several atmospheric reanalyses, ERAinterim-driven (and to a lower extent CMIP5-driven) regional atmosphere models (EURO-CORDEX), and numerous CMIP5 models. I appreciate the large amount of datasets analysed in this study, however I have several concerns with the paper in its current form, and I think that a major overhaul is required before publication.

First of all, the aim of the present study is not clearly stated. Does the paper aim to

provide projections of snow water equivalent for end users (ecologists, road managers, ski resort), or does it aim to assess the models fidelity in order to point out limitations in our ability to project future snow water equivalent or for any other purpose? The aim should be better explained, and this should also be used to choose and justify which diagnostics are shown in this paper (e.g. why evaluating ERAinterin-driven RCMs in section 4.1 and 4.2 and additionally evaluating CMIP5-driven RCMs only in section 4.3?).

I also have a concern with the first diagnostics shown in this paper, i.e. the anomalies/biases represented on maps (Figs. 2-4). First, how are the datasets re-gridded prior to compute the difference? Furthermore, as the models (including reanalyses) miss the tail of the elevation distribution (as indicated in Fig.1), it is expected that they cannot account for high snowfalls observed in high-elevation areas. It seems to me that an alternative/complementary diagnostic would be to plot the snow water equivalent distribution per elevation bin. It would indicate whether the models behave well given their grid topography. I guess that the remapping used to build the Taylor diagram in Fig.5c partly addresses this, but it is not sufficient. In my opinion, this could replace sections 4.2 and 4.3 which I don't find very informative.

In addition, despite the limitations mentioned for snow water equivalent derived from passive microwave satellite observations, no attempt is made to discuss the validity of such products. The minimum would be to compare the two datasets described in section 2.1 over their common period. Of course, comparing to more datasets or to in-situ measurements would be even better. Is there any evidence that these satellite datasets are more reliable than the other datasets?

I have a possible concern with the choice of the simulations presented in the manuscript. ERAinterin-driven RCMs are more similar to AMIP models (atmospheric-only GCMs driven by observed SSTs) than to CMIP models, so in section 4.1 and 4.2, I think that comparing AMIP GCMS to ERAinterin-driven RCMs would make more sense. Then, in section 4.3 and 4.4 where the CMIP5-driven RCMs are evaluated, it

makes more sense to compare to CMIP5 models.

I have several other specific comments:

- Abstract, l.11: replace “latest” with “fifth” (in a couple of years, latest won’t be clear).

- 2nd and 3rd paragraph of the Introduction: there are also concerns related to snow itself (road & airport safety, ski resorts, . . .).

- Intro, l.25-26: “at relatively high spatial resolution” -> subjective, indicate a typical range.

- It would be interesting to discuss the rliability of satellite datasets in the Introduction. Note that the GlobSnow dataset is derived from satellite measurements but uses ground-based weather station data in the SWE retrieval.

- Section 2.3: Sabin et al. (2013) use LMDz as an atmosphere-only model (i.e. not coupled to an ocean), I don’t know how relevant this is to the CMIP models.

- Section 2.3, last paragraph, remove “at ISAC-CNR”.

- Tab. 1: there should probably be a line between satellite products and reanalyses.

- Given that LMD is mentioned, I’m surprised not to see the IPSL models in the long CMIP5 list, but anyway, there are clearly enough models in this paper.

- Sections 2.3 and 2.4: mention what kind of outputs are used (daily means or monthly means?).

- Section 2.4: what is a “non-reliable snow accumulation trends”? (and what is a reliable trend?).

- Section 2.5, about “The ability of climate models to properly reproduce snow water equivalent depends both on the accuracy of their snow schemes and on the reliability of the atmospheric forcings”: it actually depends on many kinds on biases in the regional model (e.g. radiation scheme, boundary-layer scheme, etc, all being able to eventually

impact snow).

- Section 2.5: what is the interpolation method for HISTALP and EOBS?

- Section 3 could probably be merged with section 2 into a “datasets and methods” section.

- Fig.2: why showing the relative precipitation bias (in %) while the temperature and snow biases are shown as absolute errors?

- Fig.2: the caption “snow water equivalent in the EOBS observational dataset and the NSIDC Global Monthly EASE-Grid Snow Water Equivalent Climatology respectively” is misleading, it would be clearer at a first read to write that EOBS relates to (a) and (b) while NSIDC relates to (c).

- Section 4.1.1, about “In order to facilitate the comparison we present the differences with respect to a given dataset: the NSIDC Global SNW Climatology for SNW, since it is available for a longer period (1980-2005) than the other satellite product AMSR-E (2003-2011)”. Ok, but it is a pity not to compare these two products aver the common period, especially given that you have claimed that “we expand the study by Mudryk et al. (2015) by including additional global SNW gridded datasets obtained from remote sensing” in the Introduction.

- Section 4.1.2: I would not say that REMO2009 is much better than the other RCMs, there is a substantial warm bias all over the domain (except maybe just over the mountain range) that could explain the relatively lower bias in SNW compared to other RCMs. Also, I would replace “CCLM4-8-17 and REMO2009 models which present no issues” with “CCLM4-8-17 and REMO2009 models which present weaker biases than other RCMs”.

- Section 4.1.3: what period is used for the CMIP5 models, 1980-2005 or 1850-2005?

- Section 4.2 and its Taylor diagrams. I don’t find the spatial correlation very relevant here, because it mostly relies on correlations between the topographies. Similar com-

ment for RMSE and NSD.

- Why removing the worst RCMs in section 4.2?

- I am a bit lost, why using CMIP5-driven RCMs to analyse the seasonal cycle in section 4.3 and not to evaluate the mean spatial patterns in sections 4.1 and 4.2?

- Section 4.3: I would not call 20CR a “reference dataset”, it is a coarse atmospheric GCM only constrained by surface pressure and SSTs, probably more comparable to a coarse AMIP model. . .

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