

Snow water equivalent in the Alps as seen by gridded datasets, CMIP5 and CORDEX climate models

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Second review by reviewer 1, Yves CORNET.

The last version of the document has been strongly improved. I congratulate the authors. I'm globally very satisfied of the complete and pertinent answers given to my question and comments in the paper and by interesting supplementary material provided.

Regarding your comment *"We noted that for few comments of Revision 1, the page and lines indicated by the Reviewer do not match exactly the page/lines in the manuscript published on TCD."* Indeed I recognize I had some difficulties to manage the web access to the documents!!! I read the first version of your paper dated 6 December 2016 and not the version dated 23 January 2017. This had some consequences (see below) because figure 2 and 3 of the version dated 23 January is much more explicit than the homologous figures of the version dated 6 December. Sorry!!!

Answers to the sections

"Methodological issues"

"Research paradigms and hypothesis to be demonstrated"

I still remain skeptical about the SWE's prediction. But the new version of the paper is presented with more information and precautions so that the reader correctly evaluates these predictions in terms of magnitude of variation and inconsistencies.

I recognize my misunderstanding of the weighting procedure used to aggregate spatially over the GAR. In the last version of your paper the explanation is now clear. I think you had to clarify it.

Regarding remapping the conservative procedure is now specified, so my doubts are blurred.

As suggested, you have now canceled of the 20CR reanalysis in the mean reference computation as suggested. I'm convinced it was justified.

I remain a little bit disappointed about the answer given regarding the effect of the interdependency between Global SWE Climatology and CFSR snow outputs. Their weight remains high in the computation of aggregated value. It is not a question of similar range of variation!! But In the last explanation you have provided it is clear that you have a very good knowledge of those products (much better than mine). So I trust you ...

Thank you for the supplementary material and the precision given to get ensemble mean of HiRes models (MMM-HiRes) using only one of the 3 CESM models that show similar spatial patterns. This should reduce the non-independency effect. Regarding GCMs It is obvious that no further analysis is needed.

You provide an interesting investigation of two approaches that provide more consistent results. Fig. R3 and 5a are explicit.

My question about the usefulness of spatial analysis of differences between two gridded products to compare them in complement of only using Pearson-r association coefficient was founded by the fact that similar values of r values can hide very different spatial patterns of consistency/inconsistency. In the case of random spatial distribution of difference values the absence of geographical and climatological reasons can be assumed. In the case of high spatial autocorrelation of differences, a geographical/climatological must obviously be investigated. As I read the 6 December version of your paper (see above), my question about spatial analysis was justified. The new version of figures 2 and 3 (already present in the 23 January version) provide new insight that answer in a totally satisfying way to my question. Air temperature and precipitation are indeed the output of models that can explain spatial variation of SWE biases. So sorry again for my review of the oldest version of your paper!

Reactions to the sections

“Some specific comments”

“Comments on the document’s form (text, units, figures …)”

I agree with all the reactions to the specific comments.

All the requested modification of from have been applied.

Conclusion

I suggest the editor to purplish this version of the document.

Concittadini (per meta`) e collegiali complimenti !

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