The Cryosphere

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Discussions

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Interactive comment on "Evaluation of long term Northern Hemisphere snow water equivalent products" *by* Colleen Mortimer et al.

Anonymous Referee #2

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Summary This manuscript performs an intercomparisons and evaluation of seven different northern hemisphere representations of daily Snow Water Equivalent: four reanalyses (CROCUS, ERA/Land, MERRA, and GLDAS, two products based on AMSR-E passive microwave data, and the GlobSnow product, which is based on a combination of passive microwave and in situ snow data. The authors compare the products to one another and find broad similarities among all products except the passive microwave only products, which are quite different. Evaluation against in situ snow course data also suggest that all products other than the passive microwave only datasets provide similar levels of accuracy. The study has implications for any hemisphere-scale analysis that relies on understanding of snowpack.

Overall Review

I found this paper to quite well written, and I very much like the approach the authors took to their analysis. I kept finding myself wanting a particular type of analysis to be done and then, a few paragraphs later, the authors had done just what I'd hoped for (e.g. the ensemble analysis). However, there are a few points that I think would substantially improve the paper.

The first one, and most major, is that the selection of reanalysis products is somewhat outdated. In particular, both MERRA and ERA-Interim/Land have been, at least to some degree, superseded by MERRA-2 and both ERA5 and ERA5-Land. In the latter case, the resolution of the data products is higher (30 km) and much higher (9 km). I anticipate that most users in the future will probably use these more recent datasets rather than the older ones listed here. So the current paper is useful, but it would be so much more useful if these additional datasets were included. I recognize that it would probably be a fair amount of work to add them in, but I really think it would probably be worth it. That said, this is a decision that should be made by the authors in consultation with the editorâĂŤI do think the paper is publishable as is, just not as useful as it could be.

Second, I would like to see just a bit more discussion of snow in high-topography regions. I recognize that this is not the primary focus of the paper, but mountain snowpack is pretty important. There's been some really good work published on this recentlyâĂŤI'm thinking of the paper by Jessica Lundquist that talks about the utility of models vs. observations in understanding mountain snow and precipitation (https://doi.org/10.1175/BAMS-D-19-0001.1) and some of the work by Melissa Wrzesien that intercompares different global products in a way similar to what's done here (but explicitly for mountains), such as https://doi.org/10.1029/2019WR025350. I don't think this needs to be a very heavy lift, but I would like to see some mention in the abstract of the fact that mountains are (mostly) excluded in the analysis, along with a paragraph in the discussion addressing this point and related work.

Specific comments

Line 34: "There is a growing number"

Line 39: I think it would be good to cite the relevant paper by Meromy et al. (2013) here: https://doi.org/10.1002/hyp.9355

Line 60: I just want to say that I really like this sentence about gridded/in situ datset comparisons

Line 76: Somewhere in here it would probably be good to mention the new Nature Communications paper by Lievens et al. (https://doi.org/10.1038/s41467-019-12566-y). Also would be good to mention it in the section on mountains that I suggest above.

Line 160: what fraction of the grid cells have at least one data point? How do these data represent (vs. not represent) different environments?

Line 164 (Section 3.1): It would be great if you could get a little bit more quantitative in this section. Right now it seems like you're doing a visual comparison of the climatologies from the different datasets, but it wouldn't be difficult to also compare them quantitatively.

Line 181: "The source of inability of the standalone passive microwave products" sounds a bit awkward. What about "The reason the standalone passive microwave products. . . " or something similar?

Line 185: There should be a hyphen between observation and sparse.

Line 203: I think it might make sense to include a metric such as relative RMSE or normalized RMSE to assess whether the performance in Canada is, in fact worse because there's more snow. I also wonder if it might not have something to do with the less systematic nature of the in situ measurements in Canada. You even make reference to relative RMSE later in the paper (Line 239), though no values are provided.

Line 279: I had to read this sentence a bunch of times before I understood what you meant. Could you rewrite to try to be a bit clearer about what you did? I think you

basically took the spatial correlations for all days and then averaged them. Also, could you clarify what difference, if any, there is between spatial correlations and pattern correlations? I think you're using them interchangeably, but it's not totally clear.

Line 283: If you look at the AMSR-E datasets in Figure 6, it sure looks like the mean pattern correlation is higher. Can you clarify?

Line 333: No need for the comma between Canada and show.