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The Cryosphere Discussions

Interactive comment on "How much snow falls in the world's mountains? A first look at mountain snowfall estimates in A-train observations and reanalyses" *by* Anne Sophie Daloz et al.

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Dear anonymous reviewer,

Thank you very much for the time you took to read and review our article. I will answer your comments with more details later but I would like to answer some of them now, specifically the ones about the novelty and the scope of the paper. Although I agree with some parts of your critics, I also strongly disagree with some of them and would like to explain why.

Based on my experience presenting these results, I think the scope of the paper should

not change but could certainly be better explained. We had very limited funding to work on this article and we tried to do a paper that would both be useful for the people developing the different datasets and the ones using them. Some of the users are part of the climate community and might be aware of the differences and biases in these datasets them but some of them are not (e.g. impact modelers) and they are most of the time very surprised by these differences. In an impact model, these differences can make a huge difference. Even in the climate community, researchers who know these datasets were usually very surprised by our results (e.g. MERRA2 South America). I have presented this work in a couple of conferences and it clearly appeared to us that these results were useful to a lot of people working with snow datasets from the climate community or other communities. So I agree with you, we know all these datasets but we need more papers giving a simple comparison of them to be aware of their limitations and abilities in different regions. Furthermore, I agree with you that more work could be done for understanding the differences between the differences between the datasets but I also think we already gave some interesting insights to the people developing the datasets. And again, with the limited time and funding we had, we could not go much further.

About the novelty, I think this paper is novel in different ways: 1) because all these reanalyses and CloudSat data had not been compared before in terms of snowfall on all these regions at the same time, 2) because they had not been compared before in terms of mountainous and non-mountainous snowfall.

So in general, I think we could clarify the aim of the paper in the abstract and the introduction as well as improving the section discussing the results, including some of the limitations you mentioned for example (height acquisition Maahn et al. 2014), however, I really think we should keep the scope of the paper as it is one because it is very useful for a number of researchers.

I am of course open to more discussion about this.

Anne Sophie.

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