

## ***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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This paper quantifies the fraction of total snowfall that falls in the world mountains as well as the absolute amount of snowfall in the mountains, based on the CloudSat radar and different reanalyses. It analyzes the different datasets and gives possible explanations for the differences seen in the datasets, especially as it comes to the absolute amount of snowfall. A significant effort was made to compare the different datasets on the same grid, rigorously. The paper is well written and informative, and I think it deserves to be published.

However, some points need to be analyzed in greater depth. I have one major comment

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and many smaller changes I would like to see in the final version of the paper. This won't require new work on the data though (I believe).

My major comment is the following : the maps (Figure 2) are great, but not analyzed at all, and it is a shame, because they DO contain a lot of information. The authors say "the geographical distribution of mountain snowfall is similar between CloudSat and all the reanalyses", but I disagree. There are many interesting differences. I think the authors must work more on the maps, by considering for example maps of the differences between the different datasets, or by computing mean RMS errors between each reanalysis and CloudSat (even though I understand CloudSat has its own uncertainties). For example, in the case of MERRA-2 (which clearly stands out), there is a lot of snowfall over the mountains of eastern Russia and Kamtchatka, more than for MERRA-1. Why ? JRA55 seems to miss a lot of the patterns too. Please elaborate more on these interesting maps !

Otherwise, here are some more minor comments :

I.34 : "the fraction of mountain snowfall" is ambiguous; the authors might want to change it to something like "the proportion of snow that falls in the mountains compared to the continent as a whole".

I.37 : I agree with the authors point regarding the large-scale forcings, and it is an interesting conclusion of the paper; all the models predict precipitation when air masses are converging. but I disagree on the point that the differences in the snowfall amounts result from differences "at smaller scales". As said line 327 in the conclusion, it is more likely due to differences in the physical parameterizations of the models, as well as subgrid-scale parameterizations of orographical effects.

I.84 : what do the authors mean by "is more realistic" ? and what does it have to do with the previous sentence ?

I.93 I.97 I.117 I.120 : you might be interested in the papers of my colleague, F. Lemon-

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nier, on that subject :

CloudSat-inferred vertical structure of snowfall over the Antarctic continent F. Lemonnier, J.-B. Madeleine, C. Claud, C. Palerme, C. Genthon, T. L'Ecuyer, N. Wood JGR Atmospheres, doi:10.1029/2019JD031399, December 2019

Evaluation of CloudSat snowfall rate profiles by a comparison with in-situ micro rain radars observations in East Antarctica F. Lemonnier, J.-B. Madeleine, C. Claud, C. Genthon, C. Durán-Alarcón, C. Palerme, A. Berne, N. Souverijns, N. van Lipzig, I. V. Gorodetskaya, T. L'Ecuyer, N. Wood The Cryosphere Discuss., doi: 10.5194/tc-2018-236, March 2019

I.154 : I believe the Snow Retrieval Status (SRS) in release 5 was improved, and this might help select the profiles the authors use, especially in mountainous regions where the ground clutter might affect the retrievals. I am not saying that the authors should use release 5 and redo everything from scratch (please don't !), but that they might want to check if release 5 gives different results or not, just in case !

I.167 : "somewhat compensated by the competing effects of evaporation and undetected shallow snowfall" ; I have not read Maahn et al. (2014), but this sounds quite speculative to me. A lot can happen between the 1200m level and the surface, especially in mountains (slope winds, complex boundary layer). I think the authors should remain cautious about this point, and not say there is some kind of compensation of errors.

I.168 : this should be said earlier, when describing the CloudSat dataset.

I.174 : "less than about 15% at the surface"; what is "the surface" here ? the 1200m level ?

I.189 : "assimilates" > uses, is based on

I.199 : "while CloudSat started in 2007" this should be said earlier, when describing the CloudSat dataset.

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I.206 : "based on the Kapos et al. (2000) definition" : could the authors summarize the criteria that define a mountainous terrain ?

I.233 : "In spite of these differences, the geographical distribution of mountain snowfall is similar between CloudSat and all the reanalyses" : as mentioned above in my major comment, I disagree, we see large differences between the different datasets, and these spatial differences might be part of the reason why the absolute amount of snowfall differs between them.

I.258 to 261 : does this mean that the CloudSat estimate, which is already high, is probably a lower bound, because it might miss some large events ? if so, this should be said in the text.

I.268 : "To ease the comparison between the different datasets" I don't understand why the amounts are normalized; to me it makes things more difficult to understand, with very different y axes. Are the authors sure it is the best way to represent this ?

Table 1 : I don't understand the row entitled "Global" : for example, 1763/43403 means that when the four continents are put together, 1763 cubic km per year of snow falls in the mountains (i.e. the sum of the rates for the four continents, which is not always exactly the case by the way...), but I don't understand the number "43403"; does it include Greenland and Antarctica ? it is much bigger than the sum of all the snowfall amounts. Please clarify.

Figure 4 : How is this frequency computed exactly and how comes this is so different between the different continents ? Please clarify.

Typos

I.57 : "the response of" can be removed

I.288 : "for MERRA-2", remove "for"

I.312 : "for researchers for"

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I.317 : that THEY have difficulties ?

Figure 4, y axis, upper left panel : occurrence > occurrence

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Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2019-302>, 2020.