Hydrology and
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Sciences
Discussions

Interactive comment on "Fractional snow-covered area parameterization over complex topography" by N. Helbig et al.

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General comment

The study investigates and proposes a new parameterization of snow cover sub-grid variability based on snow depth distributions from airborne photogrammetric and laser scanning data. The approach is based on simple sub-grid terrain parameters and mean snow depth indicator. The methodology is tested in three regions in eastern Switzerland and Spain. The results indicate that snow distribution is mostly unimodal in all selected domain areas. In all three regions, the difference between theoretical and observed snow depth distributions decreases with increasing domain size. The assessment of the relation between snow depth distribution moments and terrain pa-

rameters indicates that the standard deviation of peak snow depth can be parameterized by using terrain characteristics. Authors conclude that this relationship allow to develop a snow cover sub-grid parameterization, which is not dependent on a specific geographic region.

This is an interesting topic, within the scope of HESS. The airborne sensing techniques provide an unique information about snow depth distribution in a very high spatial resolution. Thus the results are interesting and novel. I have only a few critical comments, which might be considered before recommending for publication:

1) The main question I had when reading the manuscript is to what extent it is possible to make robust and strong interpretations from using only 2 images (in different regions and years). In my opinion, many statements are too strong and need to be validated (supported) by using much larger number of images, describing different snow conditions (snow poor/rich winters, different periods of snow season). For many practical applications, the temporal variability/uncertainty of snow cover sub-grid parameterization might be more important than one fixed relation found in one winter maximum. I would thus suggest to very carefully and critically consider the limitations of using just selected examples of snow distributions and to revise some interpretations made. Some statements seem to be too general and are likely not fully supported by presented results.

2) Some of the expressions (terminology) might be misleading. In the motivation, there is expressed a clear need for proper sub-grid parameterization of snow cover for climate and regional modeling, (and I agree with it), however the typical grid sizes of regional models (5-50km, even more for large-scale climate models) goes beyond the upper limit of grid sizes tested in the manuscript (3km). So I wonder if are the results directly comparable and applicable for studies using coarser grids? Please consider this point when introducing the objectives and discussing the findings with existing applications of regional (and climate large-scale) models. Is the relative role of sub-grid variability so important also in e.g. 100m grid snow modeling?

3) Having said that, I would suggest to strenghten the story of the paper. Some more detailed outline in the introduction would be helpful to better understand, why is the scaling analyzed first, snow cover sub-grid parameterization later. Please consider also, to more clearly demonstrate the advantage and reasoning for using airborne data for deriving sub-grid snow cover parameterization, in comparison to other methods. The benefits of using airborne data are not clearly formulated and discussed.

Specific comments

1) The selection of three different regions is not clear. Why Spain in a different winter? The size around 30km2 seems to be rather small for making robust interpretations for coarser grid sizes.

2) The Summary and Discussion section reads really as a summary only. Linking the findings with existing literature (in a separate section) will help to more clearly indicate the benefits and challenges of using airborne data for sub-grid snow cover parameterization.

3) Figure 1. It would be interesting to present also snow depth distributions used for the analyses.

4) Figure 3,6,8: Please consider to use a discrete color legend instead of continuous.

5) Figure 9: It would be interesting to see also some validation of derived snow cover depletion curves.

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