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Interactive comment on "Snow depth mapping from stereo satellite imagery in mountainous terrain: evaluation using airborne lidar data" *by* César Deschamps-Berger et al.

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The paper entitled "Snow depth mapping from stereo satellite imagery in mountainous terrain: evaluation using airborne lidar data" by C. Deschamps-Berger et al. presents the second sound evaluation of VHR satellite data for snow depth mapping in alpine terrain after Marti et al. 2016. In their study, they apply high-quality, spatial continuous LiDAR as reference dataset, allowing for a meaningful validation over a large region. This is an important and valuable contribution to further advance snow depth mapping based on remote sensing data. However, there are four main points I would like to see clarified and complemented before I can recommend the paper for publication:

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Discussion paper

1. Error combination of snow-on and snow-off DSMs:

In their study they compare HS maps form Pléiadessnow-on minus Pléiadessnow-off with HS maps generated from LiDARsnow-on minus LiDARsnow-off. Certainly, this comparison makes sense and provides interesting insights but only as a second step. First, we need to see how much error is coming from individual surface models. If you take the snow-off and the snow-on DSM from the same platform and processing the errors of the individual DSMs could a) be added to each other or b) erase each other. So, if the snow-on DSM has a high error on one side (e.g. being too high) and the snow-off DSM has the error to the same side, the resulting snow depth is accurate even though both DSMs are bad. In the comparison as it is done now in the paper, there is now chance to see the quality of the individual DSMs. Therefore, I request the authors to do a HS comparison applying also the LiDARsnow-off DSM for the Pléiadessnow-on DSM. This will reveal how the snow-on DSM performs excluding random error addition from the snow-off DSM. Also, a DSM comparison between the Pléiadessnow-off and the LiDARsnow-off DSM should be performed and discussed.

2. Justification of selected photogrammetric processing method:

The authors select three different processing methods in ASP (SGM-binary, SGM-ternary and Local search). But they do not justify why they selected these algorithms nor do they describe the algorithms and their differences. Are there other options? An overview on the available algorithms including their strength and weaknesses would be very helpful for the readers and would allow for the justification of the selection. Why does the SGM binary perform best? This section has to be expanded.

3. Comparison to results from other photogrammetric platforms (UAS and airplanes) and discussion of photogrammetry specific issues:

The embedding of the work into the current literature is weak (section 6.1). It is true that they are among the first mapping snow depth with optical satellite data. But there has been a lot of additional work concerning photogrammetric snow depth mapping from UAS platforms (e.g. Van der Jagt et al. 2015, Bühler et al. 2016, De Michele et al. 2016, Harder et al. 2016, Adams et al. 2018). The authors should compare their results also to these publications and discuss more about the specific problems of photogrammetry on homogenous snow-covered areas (e.g. Bühler et al. 2017) and the effect of vegetation. I assume that there is distinct problem with noisy surfaces. This topic is not sufficiently discussed in the paper.

4. Discussion on which applications can benefit from this approach and which not:

With the elaborated accuracies in mind, there should be a discussion on which applications could benefit from this new method and which not. For example, regions with shallow snow cover (0 – 50 cm) over the vast regions on the higher latitudes of the northern hemisphere would be within the error range. So, I assume the method would be not accurate enough. But for what applications could it be of big value? I think this would be a very interesting part for the readers.

Detailed comments:

P3L65: Why are DSMs less accurate in steep terrain? Is it mainly because a small x,y shift results in a large z-shift? Please explain a bit more

P3L76: the term airborne lidar altimetry sound a bit strange to me, I would use airborne laser scanning ALS.

P3L87: Please give some references for this statement

P4L103: Why did you select exactly this zone? Can you give some justification?

P4L118: How was the image acquisition performed? What prices did you have to pay? What would be the conditions for people who want to do the same as you did?

P5L135: Why is the resolution 3 m is there a justification for that? What was the point density per m2 of the LiDAR dataset? Double period at the end of the sentence.

P7L173: In my experience it can be very hard to find snow free, stable terrain in high alpine regions and this with a good distribution over the entire investigation area. Please discuss this issue and give some recommendations what to do if not enough stable terrain can be found. Please also discuss why an absolute orientation via GCPs is not suitable.

P7L176: Why did you choose the thresholds -1 to 30 m? Please justify

P7L182: In my experience a big problem are alpine bushes (0.5 to 2 m high). How do you treat these? It is not mentioned

P7L190: Why do you use nearest neighbor and not cubic resampling? Please justify.

P10L244: Artifacts in the equations, please check all equations

P11L266: It would be very helpful for the readers to have a figure showing the matching success and the hillshade of the DSMs generated with the different pairs This would also clarify the artifacts you mention.

P12L296: Why do you use the threshold 3 x NAMD? Please justify.

P13L325: Do you have a hypothesis where this error is coming from?

P13Table2: There is a 10 cm difference in RMSE and NMAD between front-nadir-back and nadir-front-back. Why is this showing, please explain as you take basically the same input data just in reversed order.

P14L346: Please extend this section substantially by discussing results from previous UAS and airborne investigations including the reached accuracies and performance on snow covered surfaces (see my main point 3)

P14L362: But please mention the benefit of additional coverage in particular within steep slopes.

P14L366: Can you make the statement “B/H around 0.2 seems to be beneficial” stronger by providing more justification

P15L400: what was calculated ASO minus Pléiades or the other way round? ASO should be higher as it uses a DTM as snow-free DSM

P17L449: Please expand this section taking into account published UAS and airborne photogrammetric investigations

P18L473: The conclusions are a bit thin. Please also provide the RMES values. Could you provide more important information? The term "good accuracy" is vague.

P20Fig4: The scale bar is too large hindering a sound interpretation of the values. I would propose a range of 0 to >5 m. Here a difference image (HS_ASO minus HS_PLéiades would be essential. This would allow for a detailed interpretation of the results, also for the subsets. What you name an avalanche deposit in d) does not look like that to me at all. I saw many avalanche deposits in HS maps and I am pretty sure that this one is something else. Please check that. Why did you locate the transect there? Can you give a justification?

P21Fig5: Here it would help if you could draw some more transects at further locations to see the differences

P21Fig6: The ASO HS median is mostly lower than the Pléiade HS even though the ASO uses a DTM as snow-free surface (penetrating low vegetation). Therefore, we would expect the LiDAR HS to be slightly higher. Can you comment on that? âĂČ

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