

The Cryosphere Discuss., referee comment RC2  
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## Comment on tc-2022-96

Anonymous Referee #2

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Referee comment on "Assessing the seasonal evolution of snow depth spatial variability and scaling in complex mountain terrain" by Zachary S. Miller et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2022-96-RC2>, 2022

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Review of the manuscript "Assessing the Seasonal Evolution of Snow Depth Spatial Variability and Scaling in Complex Mountain Terrain"

The research described by Zachary Miller et al., exploits a UAS dataset of snow distribution observations obtained along the 2019-2020 snow season to analyze both the snow depth spatial variability and the scaling behavior in two close domains (a steep couloir and a meadow area). Manuscript authors show a consistent UAS validation with manual sampling and then analyze the scale breaks using variogram analysis at a variety of resolutions to determine scale breaks in snow distribution behavior. The work is valuable for the snow community working in mountain areas as it compares the snow spatial variability on two (close) sites with distinct topographic conditions. However there are several major points that, from my point of view, must be addressed before publication. After

Major points:

Despite these points are later detailed in the minor points list, I give here an overview of them:

- It is not clear why spherical models are used. There are other models which can fit omnidirectional variograms. Please check references, test other models (log linear models, exponential...) and justify why you use one particular model. Similarly I think the experimental omnidirectional variograms must be shown in the manuscript to

justify the fit with different models.

- Methods section needs further details on how variograms are computed (references, mathematical expressions). Moreover, there are some methodological issues that must be corrected in the analysis. For instance, the maximum distance considered on variogram computation cannot be larger than half the maximum points pair distance (Sun et al., 2006 <https://doi.org/10.1080/01431160600676695>). Methods section needs a full description of the variables later analyzed (range, nugget, sill) in view to the vocabulary of previous works. Finally why scale breaks are not computed?).
- I encourage manuscript authors to show the results for all resolutions and not only for 0.5 m. Please include values of table 2 for all resolutions. Also include results of figure 7 for all resolutions.
- From the results shown in the manuscript, I consider it is not justified this conclusion: "We found that spatial resolutions greater than 0.5 m do not capture the complete patterns of snow depth spatial variability within complex mountain terrain". For some dates this is true but in most of them 1 m resolution results (figure 5) is quite close to 0.5, 0.025, 0.1m..., and in some cases also 2.5m. This point is no convincing from my understanding. Additionally, a steep couloir is a very characteristic "complex mountain terrain", so I think it is not possible to extend this conclusion to "all complex mountain terrain". Please change conclusions conveniently.

Minor points:

Line 15: "We produced 12 snow depth maps..." I think here and all long the manuscript there is an error. Figures show 11 snow depth maps. Maybe authors refer here to 12 UAS flights (including the snow-free flight), from which 11 snow depth maps were derived. Correct conveniently in the manuscript (introduction, discussion, conclusion...

Line 50-55: I consider that, here, it is needed to state slope limitations of satellite-derived snow maps, which cannot be obtained when the slope is above a threshold value. Please verify this threshold in the references and include it.

Line 81: Change "sparse temporal observations" to "more sparse temporal observations" and include other works analyzing the snow depth spatial variability from UAS/LiDAR systems. Here the links to some examples: <https://doi.org/10.1029/2020WR027343>, <https://doi.org/10.1016/j.jhydrol.2019.124046>

Line 113-119: Three AWS are described here, however in Figure 2 only data from Brackett meadow AWS are included. Why you describe the other two? Remove the description of AWS not used in your research please.

Line 132: The grid pattern at 50 m above ground was respected for the entire flight or this elevation was programmed in the UAS control software in view to the elevation data of this later software? Please specify.

Line 133: change units of ground sampling distance to the cm/pixel as this is the most extended unit used in UAS works.

Line 139: How many points of the 25 ground control points were included in the partial selection? Were used, 1 to 3, 5 to 25, at least 7? This information is relevant for further error assessment.

Line 140: Similarly to previous point, how many surveyed snow depth points were deployed each date? Please include this information.

Line 153: Include the horizontal and vertical RMSE of image geolocation after PPK processing. Later, include details on the RMSE values obtained for the GCPs and the number of GCPs available for validation each date.

Line 163: Also Include references to other works evaluating RTK and GCP accuracies in snow dominated areas: Eberhard et al., 2021 (<https://doi.org/10.5194/tc-15-69-2021>) and Revuelto et al., 2021 (<https://doi.org/10.1029/2020WR028980>)

Line 164: Did you obtain the snow depth distribution directly subtracting regular grids from snow free and snow covered DSMs?, Were these differences computed with 3D point clouds and then rasterized to the distinct spatial resolutions? Please detail this. If 3D point clouds were directly subtracted detail the software used.

Line 166: Please give an idea of what unrealistic snow depths are for you. I guess these are negative values, too high values...

Line 174-177: I think that the manual avalanche crown profile data are not used in this research so I do not see any need to include this information. I would remove it.

Line 195: I think manuscript authors are working with regular grids, but this is not clearly stated in the manuscript. I consider it is needed to clearly state this in the first sentence of the DSM detrending section.

Section 3.5 Variogram calculation must be detailed as this is one the main analysis of this work. Include suitable references here and the mathematical expressions to compute them. I think it is unbalanced the details given in section 3.6 for computing the coefficient of variation whereas no details are given for variogram calculation.

Line 213: Why spherical models? there are other models that can be tested and fitted to the variogram. See Mendoza et al., 2020 (<https://doi.org/10.1029/2020WR027343>). Indeed there are other models which fit better the snow behavior depending the spatial scale (see Noriaki et al., 2019 <https://doi.org/10.1002/hyp.13415>, Mendoza et al., 2020 <https://doi.org/10.1029/2020WR028480>) . This point (also referred in major points section) must be fully addressed and justified.

Line 218: It is not possible to compute variograms till the maximum distance of the study area, it must be half of the maximum point pairs distance (Sun et al., 2006 <https://doi.org/10.1080/01431160600676695>). This is an important point that must be also addressed in addressed in results and methods section.

Line 223: Include details on how the "average" variogram is computed. Later, in results section (figure 4). The seasonally averaged variogram is shown. Nonetheless no details on how is computed are included (each point is the average of semivariances for all resolutions?)

Line 235: The  $n=70$  snow depth measurements include all observations? this is, following Lopez-Moreno (et al., 2011),  $70/(4 \text{ corners} + \text{center obs}) = 14$  locations? Or the 70 snow depth observations repeated this procedure in 70 distinct locations? Please clarify. Where these locations constant all acquisition dates?, always the same number?

Also an overview of the locations of these validation locations would help to potential readers where did you validate your UAV observations. I encourage manuscript authors to include one snow depth distribution map (of one selected date, maybe the seasonal maximum) to allow the reader to see the snow distribution characteristics. Include in this new figure the point snow depth measurement locations.

Line 268: I think the experimental variogram must be included in this comparison. This is, show both, the spherical fit variogram and the variogram derived from true snow depth observations. This might demonstrate that spherical model fits the experimental variogram. This is related with the major point of models tested.

Line 269: Please define in methods section what is for you range (I guess scale breaks but not sure), nugget and sill. These details can be included in section 3.5. Why scale breaks are not computed?

Line 272-273: If 0.5 m resolution is representative of most resolutions, you must somehow show it. Table 2 admits more information, so please include range, nugget and semivariance (average semivariance??) for all resolutions in both sites. Otherwise you cannot say that 0.5 m resolution is representative. If manuscript authors consider that this information is not needed in table 2, this information must be included in the

supplementary material.

Line 275: I think all the information of Figure 3 is not fully described in just one sentence. For example the variogram models differences in HG, MD for the 15/01/2020 deserve some comments. Similarly why this marked difference in 20 m resolution the 17-03-2020? Provide more detailed comments on this figure.

Line 289: "the greatest variability" in what? Between the scale breaks, between the resolutions?

Line 308: In HG, I would say that for 10 out of the 11 snow depth observations, 1m resolution has a range close to that of 0.5, 0.25 m. In the worst case, only for one date (the third acquisition on February) the range value is close to that of 1m, 2.5 m...This is not a difference of 25-75 % of the observation dates. This result does not fully support one of the major outcomes of this research: 1 m is not enough to capture snow depth spatial distribution. This happens in some cases from my understanding of your results.

Line 321: Show in a new figure or table CV coefficients for all resolutions. This might justify that the "coefficients of variations were similar across a variety of snow depth DSM resolutions" and also will justify why you choose 0.5 m resolution choice.

Line 346-350: You cannot argue that your results suggest that elevation is the most significant variable and justify that the natural elevation gradient runs parallel to the dominant wind direction. I encourage manuscript authors to compute directional variograms for 8 distinct angular windows. In the contrary I consider this point cannot be fully discussed here.

Line 355: In view to results of figure 3, 4 and 5, I do not agree with this statement. Some days 0.5 captures well the spatial variability, yes, but for many days also 1m and even 2.5 m.

Line 359: I think that conclusions about range values between the different resolution cannot be based on the average calculations (figure 4 and 6). These are important but, it must be underlined that for many dates (see figure 5) range and sill or resolutions from 0.02 to 1 and sometimes 2.5m are quite close.

Line 362: In view to previous comments, I do not agree with the statement about 0.5 m resolution. This resolution is needed to capture small-scale spatial variability for certain dates, but in most of them, 1 m (and for some of them 2.5 m) is enough to capture the spatial variability in complex terrain (HG).

Also I think you cannot extend your results to “complex terrain” you are covering a domain with very particular characteristics, a steep couloir.

Line 372: Here you talk about scale breaks, but it is not clear if you compute these or not. Please see my comment on line 269.

Line 374: Which is the value of the similar consistency in less than 20 m? Can you please give the scale breaks you obtain?

Line 403 to 414: I consider this paragraph is not really needed. This is computation, not snow since. Please reduce this paragraph and give a (very) short overview of this point.

Conclusions: Change conveniently in view to previous comments. These conclusions must be changed accordingly to the spatial resolution needed for accurately capturing snow spatial variability, which, regarding your results are in most cases 1 m and fro some dates 0.5m. Moreover the conclusions must highlight that this work was conducted in a steep couloir, which is a frequent landform in complex mountain terrain but not the only landform.

Figure 1: Include a photograph of the Meadow area to allow readers having a clear idea of differences between Hourglass couloir and the meadow area. I also encourage manuscript authors to include an intermediate map between the lower right corner USA frontiers and the topographic map. Please change figure composition and include a colored DEM of the mountain range where the location of the study site can be better observed.

Figure 3: This figure is difficult to understand. Please split it in two more rows and increase subplots size for an easier understanding.

Figure 6: Include a legend for the colors of the points.