

The Cryosphere Discuss., referee comment RC2
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Comment on tc-2022-124

Anonymous Referee #2

Referee comment on "Topographic and vegetation controls of the spatial distribution of snow depth in agro-forested environments by UAV lidar" by Vasana Dharmadasa et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2022-124-RC2>, 2022

The study by Dharmadasa et al. explores snow depth distribution at three Canadian sites (agro-forested and boreal forest) based on snow depth maps derived from UAV-LiDAR. Scaling behavior is investigated, and random forest models are used to assess the importance of various topographic and vegetation controls on these snow depth distributions. The topic of the study is of interest to the community, and in general, the manuscript is neatly organized, and the figures are well made. However, I have some methodological concerns that need to be addressed before this manuscript can be considered for publication. A major issue is certainly that the data at hand may not be sufficient to reach the conclusions drawn in the study – in the current state, the key findings and novelties of the study as well as the potential impacts of these findings are not highlighted well enough. Please find my major and minor comments detailed below, as well as some suggestions that I believe would make the study more novel and convincing.

Major comments:

- In the methods, the authors should state more explicitly that the datasets are published already. Some redundancy with their article published earlier this year is unavoidable but should be kept to a limit. There are instances where the text could be shortened and simply refer to Dharmadasa et al. 2022, I have pointed these out in the minor comments. Watch out for self-plagiarism - Figure 1 and Table 1 are almost identical to Dharmadasa et al. 2022, and need to include a proper reference (e.g. 'adapted from').
- I am not convinced by the choice of aggregating the variables to different resolutions (10-20m) for the RF modelling and related analysis, in my opinion this raises some problematic questions:

- Firstly, I don't understand why the aggregation of the vegetation parameters is needed at all (L200ff). 'Canopy height' and 'Tree height' is not necessarily the same thing, and I see no problem with computing canopy height if the pixel size is smaller than the tree crown. Doing so would actually allow extracting the small canopy gaps that have been shown to be the main source of forest snow variability in some other studies (cited in this manuscript), while these gaps are averaged out if the pixels are aggregated to 10-20m resolution. This averaging is likely masking some dependency of snow depth distribution on forest structure.
 - Likewise, averaging topographic variables will average out most of the micro-topographic variability that I understood to be the focus of the study. Again, some dependency between snow depth and these variables may be masked by this aggregation. It should be clarified whether the authors are trying to quantify micro-topography or topography.
 - I find it particularly problematic to use an aggregation that is larger than the scale break identified in Section 3.2. Doesn't this mean that the variability that you are trying to explain is averaged out?
 - Finally, aggregating leaves you with a rather small sample size, as the surveyed areas are quite small. Especially in the case of Montmorency, the field landcover type covers a very limited area only.
 - I would find the analysis more convincing if it was conducted at the 1.4 m resolution, I suggest doing that in view of a resubmission.
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- Some parts of the results chapters require more detail / explanation, and the discussion needs to be more convincing. Some examples:
 - Section 3.1: The large overlap of forest and field histogram makes me wonder whether the difference between the two distributions is statistically significant – testing this would be appropriate (see e.g. Currier et al. 2018 for examples of such tests). The inter-site comparison is problematic because data acquisition did occur in different years.
 - Section 3.2: It is unclear how the scale breaks were identified.
 - Section 3.3.1: You need to better justify why you computed so many topographic and vegetation variables if most of these remain unused in the analysis / RF model. You also need to explain why you used the same predictors at all sites – for instance, NFE and WFE are homogeneous across the forested area in Montmorency forest so it is not surprising that they have no predictive power.
 - Section 3.3.3: This section is a bit lengthy, and the key messages don't quite come through. Maybe it would be better to show fewer subpanels in Figure 6 and focus on the variables that actually exhibit interesting relationships to snow depth.
 - In the discussion, you need to comment on the actual benefit of such an RF approach. The model performance shown in 3.3.4 is pretty poor, and it's not straightforward to extract the interesting information from the plots in Figure 6. In fact, I felt like the most insightful Figure to grasp the snow depth variability was Fig 3, where increased accumulation in sheltered locations is visible by eye.
 - Given that the datasets have already been presented and used in an earlier study by the same work, the added value of the analysis presented in this manuscript seems a bit limited and the novelty of the study is not emphasized enough. What are the key

findings, and where will they be useful? I realize that it's not easy to get more out of such a limited amount of data, but I tried to make some suggestions that the authors could consider in view of a re-submission.

- If the authors have the opportunity to acquire more data in the upcoming winter, the authors should consider postponing the final analysis to after the upcoming season. Repeated flights over the same site would allow for a much more insightful analysis. It is very difficult to draw conclusions on individual processes based on snow distribution data from one acquisition only, which I think is part of the reason why the discussion does not seem very conclusive. For instance, it could be interesting to see if the snow depth maxima at the forest edges are a recurring feature, and if their effect persists throughout ablation (that's just an idea, but one could do much more). It would also be good to survey all sites in the same year to allow a more convincing comparison between sites.
- Since the authors analyzed many more terrain and vegetation variables than they ended up using in the RF model, it could be interesting to dedicate a section on the physiographic variables themselves, attempting to identify a set of variables useful to characterize this sort of landscape. Maybe in comparison to variables that have been related to snow distribution in other studies. For example: Elevation has been found to exert a main control on snow depth in complex terrain in other studies, but it is not a really 'useful' predictor at the sites used in this study – is this a consequence of the site choice, or is this site representative of the terrain found in the whole ecoregion?
- Exploring the relationships between snow and physiographic variables at different spatial aggregation levels could be interesting.
- Applying additional modelling approaches (statistical, or even physically based) to compare with the RF model could be insightful, especially to draw conclusion on the utility of these findings for later work or practical applications.
- Adding an application of the model results would be a nice addition – e.g. suggest tiling approaches, extend to larger area or entire watershed, etc.

Minor comments (including wording/language suggestions)

L37 'topography and vegetation type, and density' -> you mean vegetation density?
Sentence doesn't read very well, consider rephrasing

L46: a very nice and comprehensive paper on the topic:
<https://doi.org/10.1029/2011WR010745> - I suggest including this reference

L51 'a short scale break is reflected by interception' -> I would say it's the other way round?

L62: You should refer to much more recent developments of process-based models, as some of these models now actually do resolve small-scale variability due to heterogeneous

canopy structure. See Broxton et al. 2015 (already cited elsewhere) and Mazzotti et al. (<https://doi.org/10.1029/2019WR026129> and <https://doi.org/10.1029/2020WR027572>).

L93: 'one of the earliest results [...]'. I think this is not a very fair 'selling argument' for this study, since the data used for the analysis has already been presented in another paper.

L108: incomplete sentence (WMO's station network?)

Table 1: Winter season -> snow cover period?

L136 is this vertical or horizontal accuracy, or both?

L164: what do you mean by 'multipath effect'?

L165: you just said the accuracy is comparable to previous studies, so what is the improvement? I would omit the entire end of the paragraph from 162 onward and just refer to Dharmadasa 2022.

Section 2.2.2-2.2.4: Please specify that maps of the variables are found in the supplementary material, I was missing those maps here and found them only much later. Note that the figures in the supplement should include the units for all variables.

L197-199 Is $GC = 1 - CC$? and at what resolution are these metrics calculated, also 1.4m?

L201: How did you estimate crown diameter?

L224: This approach seems a combination of Currier & Lundquist and the DCE presented by Mazzotti et al 2019 (which however has no notion of search distance contrary to your method). Maybe worth noting?

L256: It is not very clear how you define the variable 'Site', or at least it wasn't to me when looking at the descriptor maps in the supplementary material and comparing with the other vegetation metrics. I think this is quite crucial for understanding the edge

metrics, hence I more detail is needed here.

L264 Tenses are inconsistent

L266: Variable importance of a variable? Consider rephrasing

Figure 3: specify whether you used the binary variable or the land cover classification to create the histograms.

L308: how did you come to this conclusion?

L310 'collinearity analysis suggested discarding GF and CH in favor of LAI at the two agro-forested sites, while LAI was instead flagged as colinear instead of GF and CH in the coniferous site'. Please rephrase – 'LAI was flagged as colinear' is unclear (colinear to what?)

Figure 6: Y-axis label missing (snow depth)

L445-446: Unloading through branches should reduce spatial variability, no?

L511-513: The counterintuitive [...] stations at the site. -> I don't understand what you are trying to say here.

L565ff: this section needs to acknowledge hyper resolution process based (physically based) models (see earlier comment). There are ways to account for fine scale canopy structure, while I would say that the terrain roughness still represents a major difficulty.

L483: I think this should be 'Hydrologic response units'

Section 4.3: The work from Safa et al. (<https://doi.org/10.1029/2020WR027522>) needs to be included in this discussion – they applied RF models as well.

