The Cryosphere Discussions

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Comment on tc-2022-124

Anonymous Referee #1

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-RC1, 2022

Summary

In this paper, the authors analyze the scaling properties of lidar-derived snow depth and possible dependencies with topographic and vegetation descriptors in two agro-forested and one coniferous site in eastern Canada. They conduct variogram analyses on snow depth fields to find possible scale break lengths that define regions with self-similar behavior, and develop random forest models to characterize predictor importance. The results show scale breaks spanning 4-7 m in forested sites, and relatively longer values in field areas – up to 18 m in wind exposed fields – in agreement with previous studies. The results also show that wind-related forest edge descriptors mostly explain snow depth variability in agro-forested sites, while canopy characteristics (i.e., forest structure) are more important in the coniferous site.

Overall, the topic, research questions and experimental setup are interesting for the snow hydrology community. The literature review and discussions are quite extensive (maybe more than needed), and the graphics included in the manuscript are very nice. I have three major comments that I think the authors should address before this paper is considered for publication. Additionally, the authors will find a set of minor comments and editorial suggestions that may be helpful to improve the quality of this manuscript.

Major comments

1. Fractal analysis:
i. It is not clear from Figure 4 that scale breaks actually exist and, therefore, all the remaining analyses and interpretations remain unsupported, unless the authors demonstrate quantitatively that the snow depth scaling behavior changes. I recommend the authors to revise Mendoza et al. (2020a) as a reference on how to detect scale breaks in variogram analysis.

ii. The authors need to show quantitatively that variograms hold a power law (which is required to indicate that a spatial pattern is actually fractal). I think that, at the very least, the authors should demonstrate that a linear model in the log-log space holds before and after the scale break, with a high coefficient of determination (e.g., R2 ï□³ 0.9). I also recommend the authors to test whether other geostatistical models are more suitable for this data (e.g., spherical, Gaussian).
iii. The authors might consider comparing omnidirectional variograms of snow depth (and potential scale breaks) with those obtained from bare earth topography and topography+trees (e.g., Deems et al. 2006; Trujillo et al. 2007). Additionally, the analyses could be enriched by computing directional variograms and associated scaling parameters, in order to establish possible connections with dominant wind directions (e.g., Deems et al. 2006; Schirmer and Lehning 2011; Clemenzi et al. 2018; Mendoza et al. 2020a).

2. Partial relationships: I think the authors should be more quantitative, since the reasoning provided in section 3.3.3 is subjective and difficult to follow. You can easily improve this section by computing the Spearman rank correlation coefficient, and reporting the p-values. I suggest avoiding statements like 'strong relationships', 'stronger that', 'slight decrease with increasing', etc.; instead, you can show the numbers and let the readers judge.

3. The authors may consider using the following sequence to display RF results:
i. Exploratory analyses with scatter plots of snow depth vs. predictors (current Figure 6). In any case, I recommend the authors verifying these results, since it's very odd that there is practically no scatter along the y axis. Are you displaying all points within your domains in each panel?
ii. RF model performance (i.e., modeled vs. observed snow depth, current Figure 7) for training and prediction periods (a 2x3 panels plot, with the top row for training, and the bottom row for OOB).
iii. Results for predictor importance (current Figure 5). How different are these compared to those obtained with the training dataset?

Minor comments

4. L25: Do the authors mean "physically-based models"? Note that all hydrological models (even simple bucket-style models) are, to some extent, process-based (see discussions in Hrachowitz and Clark 2017).

5. L37: Do you mean vegetation density?

6. L39-40: You might want to read and cite the work of Deems et al. (2013).

7. L43: Please clarify what you mean with high-resolution. In L43 you say <100 m, but in the following line you say <10 m. Also, I suggest providing references for micro and meso-scales, and reviewing the study of Tedesche et al. (2017).

8. L48-49: The authors should include other studies that also reported multiscale behavior in snow depth (Helfricht et al. 2014; Clemenzi et al. 2018; Mendoza et al. 2020b). The latter is particularly relevant for this study (and the discussion in L432-433, L445-447), since the authors found 4-m scale break lengths (similar to what is reported here) at the only Andean vegetated site they examined.

9. L50: I think the authors want to say "different combinations of processes". Also, I would precise that you refer to the importance of horizontal resolution, not only for the measurement scale, but also to inform model scales.

10. L62: Please note that process-based models are assemblages of hypotheses about the functioning of hydrological systems. Accordingly, models might be missing processes (e.g., avalanches, blowing snow) that are relevant in particular locations, and hence not all of them are applicable to all conditions (see discussions in Clark et al. 2011).

11. Table 1: Was the snow-on flight conducted right after a storm? I think this information is relevant to establish possible connections between your snow depth results and dominant winds.

12. L182-183: If your aim is to analyze wind effects on snow redistribution, you should filter your data considering (i) wind speeds above a threshold (e.g., 4 m s−1) and (ii) air temperature below 0°C when snow transport by wind is most likely to occur (e.g., Trujillo et al. 2007). I also recommend the authors to revise Li and Pomeroy (1997).

13. L197-198: I recommend the authors to explain with words what the canopy cover and the gap fraction are, before providing details on how you compute their values.

14. L205-209: I think this explanation would greatly benefit from a diagram showing what a forest edge is, a hypothetical dominant wind direction, windward, leeward, and the maximum search distance.

15. L241: How did you define the maximum lag distance for variogram calculations? Note that Sun et al. (2006) recommended setting it to half of the maximum point pairs distance for variogram calculations.

16. Figure 3: It would be helpful having the site names here, hopefully between the top and the bottom panels. You could also include the maximum snow depth, the coefficient of variation (CV) and the skewness to compare field vs. forest. Even more, the authors might consider merging Figures 1 and 3 into a unique Figure, to make it easier to see the snow accumulation patterns they describe with the various land cover types.

17. L279-280: This is really hard to visualize. Do we really need this level of detail?

18. L286-287: Where are those snow depth intervals coming from? They don't seem to reflect the actual ranges.

19. L295-296: Where are you showing this? I don't see it in Figure 3.

20. L297: I think what you actually mean is "multi-scaling". Multifractal implies a continuous spectrum of fractal dimensions (Mandelbrot 1988). Also, you should define what a fractal is (perhaps in the methods section).

21. Section 3.3: there are too many acronyms in this manuscript, making it difficult to follow the reasoning. I suggest deleting some or replacing them for more intuitive ones.

22. Figure 5: Perhaps it would be easier to understand these results if you linked the different symbols with straight lines.

23. L357-358: this is not clear from Figure 6. Can you please provide a better explanation?

24. L383: I think it’s the other way. Figure 7 shows RF model estimates vs. observed snow depth.

25. L424: ‘more spatially continuous’. What do you mean with this? It seems to contradict the previous sentence.

26. L444: I think here you should cite Mendoza et al. (2020b) and NOT Mendoza et al. (2020a).

27. I think section 4.3 could be largely condensed. You may also consider shortening the introduction.

28. L479: ‘At the combined scale’. I think it is more appropriate to write ‘At the full domain’.

29. L489: I think that you mean Hydrologic Response Units (HRUs).

30. L490: ‘successfully modeled’. Can you please provide some numbers demonstrating that the hydrologic modeling was indeed successful?

31. L518: These results seem quite poor. Did you compare RF performance with multiple linear regression models?

32. L532-533: I would not even mention those references, since NSE is not a good metric to assess the spatial accuracy of model simulations. There are other performance measures for such purpose (Koch et al. 2018; Demirel et al. 2018; Dembélé et al. 2020).

33. L558: I would avoid referring to ‘improved accuracy’, since RF model results are quite poor.

34. L567: ‘forest structure variability’. Do you mean spatial or temporal variability?

Suggested edits

I have provided some editorial suggestions. However, I think that the manuscript would tremendously benefit from a language revision.

35. L13: ‘for the accurate prediction’ -> ‘for accurate predictions’.
36. L28: delete ‘problematic’.
37. L34: ‘on the downstream hydrograph’ -> ‘on downstream hydrographs’.
38. L56: ‘The knowledge’ -> ‘the estimation’.
39. L60: delete ‘modeling approaches like’.
40. L77: ‘that used RF algorithm to express’ -> ‘quantifying’.
41. Table 1: ‘mm/y’ -> ‘mm/yr’.
42. L150: ‘were quantified’ -> ‘were obtained’.

43. L153-155: I suggest writing the sentence between parentheses in a separate sentence.
44. L162: 'When taking into account' -> 'Considering'. Delete 'that was typically'.
45. L163: add a comma after 'environment'.
46. L165: 'to represent' -> 'represent'.
47. L165: 'As well': I find this term quite odd. I would replace by 'Additionally', 'Further', 'Moreover', etc. (this comment applies for the entire manuscript).
48. L180: 'which gives' -> ', providing'.
49. L182: 'from the hourly' -> 'from hourly'.
50. L250: 'two thirds… is' -> 'two thirds… are'.
51. L266: delete 'of a variable'.
52. L314: delete 'to allow'.
53. L321-322: rewrite as '…LAI and WFE have the highest (64 %) and least (3 %) impacts, respectively, on snow depth…'
54. L323: 'acting in forests and fields' -> 'in such environments'.
55. L407: 'In our results' -> 'our results show that'.
56. L415: '… suggests microtopographic… ' -> '…suggests that microtopographic…'
57. L458-459: Awkward sentence. Please re-write.
58. L462: 'by the preferential' -> 'by preferential'.
59. L470: Delete 'Whereas'.
60. L475: 'and dominates' -> 'dominating'.
61. L487: 'in the melting' -> 'during the melting'.
62. L488: 'challenge the'. Delete 'the'.
63. L512: 'could be due to' -> 'could be explained by'.

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