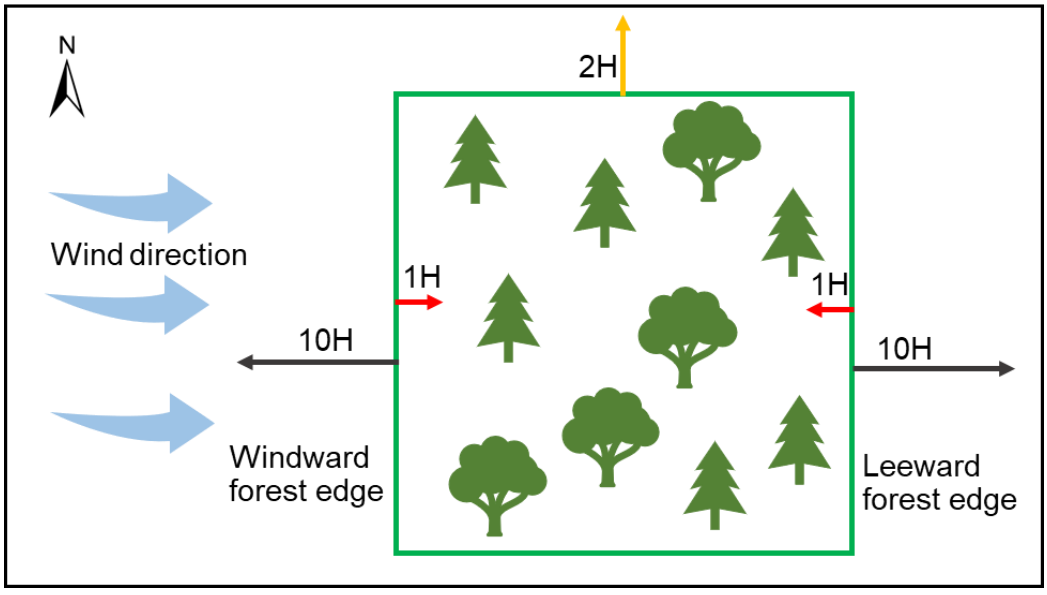
We thank the anonymous referee #3 for his/her thorough review with constructive comments and suggestions that certainly will improve the manuscript. In the following, we will address the referees' comments point by point. We mark "black" the comments given by the referee, and our responses in "blue".

Comment on tc-2022-124 Anonymous Referee #3 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-RC3, 2022

Major comments:

1. This manuscript is using UAV-lidar data to understand the snow-depth heterogeneity in agro-forested environments and boreal forests. Since the author also used the slope and aspect as topographical variables for studying the snow-depth distribution. However, at site Saint-Marthe and Saint-Maurice, the elevation difference is actually very small comparing to the spatial scale of the study area, it would be kind of surprising to observe any meaning effects from topographical variables in these 2 study areas. And although Montmorency has a elevation difference of 20 meters in the study area, it seems the main area is facing towards south east direction. It would also be helpful to visualize the distribution of the slope and aspect of the 3 study areas to make sure there is significant variability in these predictors of snow-depth before feeding them into a model like random forest.
   Thanks. Please kindly note that we included the plots of the independent variables in supplementary material.

2. It is not very clear on how the forest edge descriptors are derived, it would be helpful to have a visualized illustration to demonstrate how variables are derived based on canopy cover data from lidar.
   Thank you for the suggestion. We will include the following visualized illustration to demonstrate how forest edge descriptors were derived in the revised manuscript.



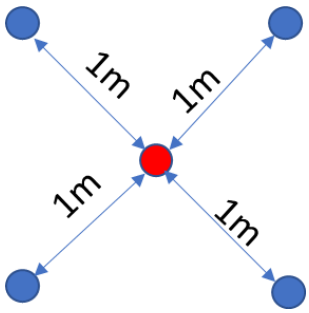
10H indicates the maximum search distance (d_{max}) in the open field from the forest edge in windward and leeward direction, 1H indicates the maximum search distance in the forest from the forest edge in the windward and leeward direction, and 2H indicates the maximum search distance northward of the forest edge. Forest edge boundary was extracted from the site variable.

3. The 2nd objective raised in the Introduction part for this manuscript seems to be extremely open ended. Is there a particular hypothesis the authors would like to test with the used dataset and validate the hypothesis throughout the manuscript? The current objective of "exploring the relationship between snow depth, topography, and forest structure" seems too vague and not specific enough.
   Thank you for point this out. We will make the second objective more specific in the revised manuscript. We will add the following sentences at the end of this paragraph. "Forest edge effects are expected to exert a greater control on snow depth variability in agro-forested sites and the forest structure is expected to be dominating the snow depth variability in coniferous forested environment."

Minor comments;

Figure 1 – it looks like the first 2 study areas are very flat with low elevation and the 3rd one has elevation difference and the elevation is much higher. It the precipitation in this area affected by orographic effect as well?
At the scale we considered for our study, elevation difference at any of the sites were too small to produce any meaningful orographic effects. Even at larger scales, the first two sites are generally flat, hence will not have any orographic effects on precipitation. But in the third site, Montmorency, when the scale of the study area becomes larger (beyond that analyzed here), orographic effects would be expected to have an impact on precipitation.

Line 154-155: given it is 1 m diagonal cross shape, why it is 1.4x1.4 m grid cell? Isn't it going to be 0.7m x 0.7m grid cell instead?
The manual measurement strategy with 5 measurements at each sampling location we adapted was as follows:



Hence, the length in one side of the grid cell is $(1^2+1^2)^{1/2}$. Which is 1.4m.

Line 162-165: it is not clear why UAV-lidar is more robust and the technology represents an improvement to previous studies.
By considering your comment and a similar comment from referee #1, we will rephrase and remove some parts from this section in the revised manuscript.

Line 184-186: The closest weather station is 19 km away from Saint-Maurice. Is the wind data going to be trustable for this site given it is very far and the wind speed and direction can be quite different comparing to the actual on the site, right?
This is the closet wind station available for this site. Given that the flat topography of this area, wind speed is expected to be not driven by topography and spatially coherent over large scales.

Line 193: how is LAI, CC, and GF calculated? By using Lidar360 software?
Yes. They were calculated in Lidar360 software. This will be better explained in the revised version.

Line 202: why the grid size for vegetation is so much larger than the resolution of the snowdepth (1.4x1.4 m). It seems the vegetation grid resoluation is so much coarser and are we able to capture all the forest variable based on such a low resolution?
The initial goal was to analyse snow depth variability at a scale of single-tree level. We selected the resolutions in a way that at least one tree will be inside the grid cell. However, we agree that the variability averages out inside a coarse grid cell. Based on your comment and that of referee #2 we redid all analyses at the 1.4m resolution. Random forest analysis results and discussion sections will be replaced accordingly in the revised manuscript.

Section 2.2.4, please see major comment #2, it is not very clear how d and d_max are derived based on the forest-covered lidar data.
We will add an illustration to better explain the derivation of d and d_max.

Line 250-253, are hyperparameters in Random Forest tuned or selected before training each model?
Hyperparameters in random forest at each site were tuned before training each RF model. This will be mentioned in the revised version.

Line 257: It is not very clear how forested vs. fields are defined. It would be helpful to have a map of these site showing this binary variable. And why don't we use this binary variable directly in the RF model directly? Is it to show at different area how other variables affecting snow-depth differently?
The binary site variable was derived according to the field and forest area boundaries manually mapped at each study area. If there was a forest patch in the field, we considered that patch as forest and vice versa. For instance, in Sainte-Marthe we considered forest patch located to the southwest in the field as forest in addition to the large forested area. In Saint-Maurice and Montmorency the derivation of the site variable was more straightforward as field and forest patches were well separated and so more distinguishable than in Sainte-Marthe (i.e., there were no forest patches in field as in Sainte-Marthe). Once we delineate the forest and field boundary, we assign 0 to field and 1 to forest. We will add more details to describe the derivation of the site variable in the revised version.

Please note that the plot of site variable at each site was included as supplement figures.

Figure 3: it is a bit surprised to me at Montmorency there is not many data points for under canopy. Then we might not be able to observe a lot of under canopy snow-depth signals.
Since Montmorency has a thick evergreen canopy cover, we did not get ground returns under the canopy at some locations. Hence, we lacked the snow depths under canopy on such occasions. This which is a limitation of the UAV-LIDAR system in thick coniferous covers is acknowledged in section 4.4 Limitations of the study.

Figure 4: how is the scale break selected? Please describe that in the Method section.
We will include this in the revised version of the manuscript. This comment resonates with other referee's comments regarding the scale break identification. For the time being, we provide a detailed description of this in response (response 1) to referee #1's first extensive comment on this point.

Figure 5: it might be better to use bar chart with different colors. It is a bit difficult to differentiate color the marker styles on this scatter plot.
Thanks. We will update figure 5 to a horizontal bar chart in the revised manuscript.

Section 3.3.3: there is only one line in the Method section (line 267) discussed about the partial relationships of preditor variables with snow depth. It is still not very how that is calculated. Please add details in the Method section.

Thanks. We will integrate this in the method section in the revised version.

Figure 7, it looks like the model is not performing very well at Saint-Maurice and Montmorency. The slope of the predictioned vs. observed is not close to 1. What would be the reason that the trained RF model is underfitting and has this systematic bias?

The performance improved with the higher (1.4m) resolution model, but the poorer performance at Saint-Maurice and Montmorency could be due to underlying processes/variables that were not considered in our model, as well as unexplained snow depth variability that is within the system (UAV-LIDAR) detection limit. We will add this issue to the updated discussion.