

Response to Anonymous Referee #1

We thank the anonymous referee #1 for reviewing the manuscript for the second time, his/her comments, and suggestions. 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". We also included the line numbers in revised manuscript in our responses.

Comment on tc-2022-124

Anonymous Referee #1

I want to thank the authors for the detailed replies to all my comments, and I commend them for re-doing a lot of analyses and including new results. I think that the main concerns raised in the previous revision have been thoroughly addressed, and the manuscript reads much better. I have a final set of observations and editorial suggestions that will help the authors to put this paper in publishable format.

Minor comments

1. Section 2.2.2: can you please clarify how does the TWSI parameter relate to the TPI or the Sx parameter in Revuelto et al. (2014)?

We added a sentence addressing this in section 2.2.2 now (L173–L174).

2. Section 2.3.1: please clarify what you mean with 'topography+vegetation'. Bare earth topography + forest trees?

Thanks. We added this information in L247 now.

3. I think that subsection 3.3.1 should be in the Methods section, and NOT in the results section.

Thanks. We now moved this section to section 2.3.2 Random forest model in methods (L288–L307).

4. L495 (section 4.2.1): are bare earth topography variograms actually comparable to snow depth field variograms? Did you use exactly the same domains to compute these? Please clarify.

Yes. We used bare earth topography in the field to compute the variogram. We now mentioned this in L246–L248.

Suggested edits

5. Abstract: I suggest adding a connector between the first two sentences.

Thanks. We modified the sentence as suggested now.

6. L19: large -> larger.

Thanks. Changed as suggested.

7. L22: add a comma after 'Hence'.

Thanks. Changed as suggested.

8. The last sentence of the abstract seems redundant. I suggest deleting it.

Thanks. Changed as suggested.

9. L31: ‘melt dynamics of the snowpack’ -> ‘snowmelt dynamics’.

Thanks. Changed as suggested.

10. L60: ‘aspect in understanding’ -> ‘aspect for understanding’.

Thanks. Changed as suggested.

11. L72-74: ‘the random forest (RF) model... started gaining popularity’ -> ‘random forests (RF) models ... have gained popularity’.

Thanks. Changed as suggested.

12. I personally dislike the use of the word ‘successfully’ or ‘successful’ in scientific writing, without providing. If you want to use them, I think that, at the very least, you should show the numbers that make a method ‘successful’.

Thanks. Changed as suggested.

13. L97: postulate -> hypothesize.

Thanks. Changed as suggested.

14. L102: delete ‘of the three sites’.

Thanks. Changed as suggested.

15. L113: ‘for the interpretation purpose’ -> ‘for interpretation purposes’.

Thanks. Changed as suggested.

16. L405: there is a typo here (Aspest_SN).

Thanks. Corrected.

17. L585: create -> creating.

Thanks. Changed as suggested.

18. L652: ‘...and land cover’ -> ‘..., land cover’ (i.e., replace ‘and’ by comma).

Thanks. Changed as suggested.

19. L672: ‘process-based models’ -> ‘physically-based models’.

Thanks. Changed as suggested.

20. L679: delete ‘most of the process-based’.

Thanks. Changed as suggested.

References

Revuelto, J., J. I. López-Moreno, C. Azorin-Molina, and S. M. Vicente-Serrano, 2014: Topographic control of snowpack distribution in a small catchment in the central Spanish Pyrenees: intra- and inter-annual persistence. *Cryosph.*, 8, 1989–2006, doi:10.5194/tc-8-1989-2014.

Response to Anonymous Referee #2

We thank the anonymous referee #2 for reviewing the manuscript for the second time, his/her comments, and suggestions. 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". We also included the line numbers in revised manuscript in our responses.

Comment on tc-2022-124

Anonymous Referee #2

I am reviewing this manuscript for the second time, and I recognize and appreciate that substantial effort went in this revision to address the reviewer comments on the first version of the manuscript. This has clearly improved the manuscript; in particular, I find the analysis at higher spatial resolution more convincing than the original one.

There are a few remaining issues that need to be fixed before publication; except for one (see 'major comments'), they mainly relate to readability, clarity, and logic – I felt that some statements were contradictory. In general, I think manuscript readability would benefit from some shortening, which could be achieved by more precise and concise language and by removing some repetition in the discussion.

Please note that line numbers in my comments refer to the revised manuscript without tracked changes.

Major comments:

1. In L20ff, the authors state that 'Results show that [...] increased the model prediction accuracy by more than 90 %.', and the same statement reappears at the beginning of the conclusion. I find this statement a bit problematic for two reasons: Firstly, without context, it is not clear what this 90% improvement is compared to, nor where the number comes from. Moreover, it is presented as a major result of the study in the abstract and the conclusion, while there are multiple instances where the authors state that predictive accuracy was not the primary concern of the study. This is contradictory and should be handled more consistently, I believe highlighting the other main findings would be more appropriate

You are correct that some clarifications were needed. Indeed, the main focus of the analysis was on identifying the drivers of variability and less on the prediction accuracy itself, although relevant and also discussed in the paper. This is why, for example, we carefully screened predictors for collinearity. Here, we do not compare the model accuracies, but instead the *increase in model accuracy* (decrease of prediction error) when these key predictors are considered. The relative importance of a variable shows how much the inclusion of the variable of interest decrease the mean squared error (MSE) of the RF model prediction *compared to a model that discards the variable of interest*. As such the large relative importance (given as %) of WFE in agro-forested sites and LAI in coniferous site imply that including these predictors improve the model prediction accuracy greatly compared to a model that discards these effects. We acknowledge that this was not clearly presented in the paper. We now modified the abstract, methods, discussion, and conclusion to better explain this.

2. L271ff: This paragraph is a bit confusing. The authors first state that data was not split, but then argue that the general procedure IS to split the data and explain the in-bag / out-of-bag concept. It remained unclear to me what exactly was done for this study (i.e. whether data was splitted or not).

We modified the text to clarify the procedure (L270–L280). We wanted to highlight that we did not keep a set-aside test set from our sample data. Instead, we used all the available data in RF model and used the OOB statistics for model evaluation. The RF model itself randomly splits the input data into in-bag and out-of-bag sets, train the model using the in-bag set and validate the model using the out-of-bag set.

Minor comments:

L44: ‘high-resolution micro variability’ -> I suggest dropping high-resolution to avoid repetition in the same sentence

Thanks. Changed as suggested.

L 54: ‘while a shorter (6 m) and longer (20 m) distance in non-vegetated areas are explained’ -> Should be changed to shorter distances (plural)

Thanks. Changed as suggested.

Section 2.2.4 I appreciate that the site variable is now better explained, but it took me a while to understand that the classification relied on the land cover map only; maybe it would be a good idea to refer back to Fig 1 here.

Thanks. Added as suggested (L199).

L234: The ‘DCE’ descriptor from Mazzotti et al. (2019) is also continuous, hence this statement is not strictly correct. I think the novelty is mainly that the edge metric used in this study takes wind direction into account. I suggest reframing this statement.

Thanks. We reframed this now (L231–L233).

L290: ‘It is a risk-adjusted alternative to variable dependence.’. this statement (especially ‘risk-adjusted’) is unclear, consider rewording

Thanks. Changed as “better alternative” (L311).

L305ff: I am not convinced that listing the max. snow depth values at each site is very meaningful since the values all seem outliers at the upper end of the histogram. I would omit this for the sake of making this section more concise.

Thanks. We now removed the maximum values reported in the Figure 4 and the section.

L420: ‘probably due to the influence of instrumentation’: It is unclear what instrumentation this refers to.

Thanks. Changed the sentence as “influence of instrumentation at NEIGE-FM site” (L414).

L430 ‘than their forest models’: consider rephrasing to ‘than the forest model at the same site’ (or ‘than the corresponding forest model’), I believe this would be more precise.

Thanks. Changed as suggested (L424).

L445: ‘The overall amount of snow in the forest compared to field in the boreal forest of Montmorency could thus be underestimated due to poor lidar coverage under dense canopies’: It is unclear why a lower coverage implies an underestimation of snow depth

Snow depth actually has to be “overestimated”. We apologize for this mistake. We now explain this better (L442–L445).

465: ‘or larger-scale effects’: It is unclear what this refers to

We corrected this as larger-scale topographic effects now (L455).

472: ‘Shorter scale break distances in forested areas compared to open field areas (Fig. 5) is analogous to previous studies that studied the fractal distribution of snow depths with lidar data.’ : This statement doesn’t seem supported by the references listed in the following sentences, please revisit.

Thanks. We rephrased this section now (L471–L478).

L514ff: For modelling, this is only true if the goal of the modelling is to fully resolve the variability, but not strictly necessary – a sub-grid parametrization is also an option

Thanks. Considering your comment, we now changed the wording “represent” to “fully resolve” to better explain this (L513).

530ff: I suggest removing this introductory sentence to avoid redundancy

Thanks. We now removed this introductory part.

L581: ‘microtopography has a more restricted influence on deeper snowpack at this site compared to the shallower snowpack at the agro-forested sites’; vs L591: ‘Our findings however show that the microtopography, even under wind-sheltered conditions in the forest, still plays an important part of the spatial variability.’ – Note that while not contradictory, contrasting statements like these confound the key message of the paragraph. I would strongly advise to streamline such discussion sections.

Thanks. We rephrased the latter part now (L588–L589).

Chapter 4.3.3 somewhat disrupts the story line and refers to results that are presented in the supplementary material only. I would recommend moving the entire section to the supplementary material.

Thanks for this suggestion. We decided to remove this section altogether from the paper now not to disrupt the flow.

Chapter 4.4.3: the table comparing RF and MLR performance seems better suited to the results section, I suggest reconsidering this

Thanks for this suggestion. We moved the table 3 to results section “3.3.4. Performance of RF models at each site”.

616: ‘grid-scale mean snow depth’: It is unclear what this means

Thanks. We just use “grid-scale snow depth” now (L600).

Chapter 4.6: It would be nice to see statements on two additional aspects: 1. Amount and coverage of your data – how transferrable are insights obtained from so little data only? 2. The fact that statistical methods provide only limited understanding of the processes underlying the observed patterns.

Thanks. This has been addressed in section 4.6 now.

679: ‘More often’: Than what? Consider omitting.

Thanks. Changed as suggested.

691: 'would benefit from incorporating the meteorological conditions': It is unclear what 'meteorological conditions' refers to and which result allows drawing this conclusion, please specify.

That confusing statement has been removed altogether.