REVIEWER #2

General Comments:

COMMENT # 1.1

This study investigates using ICESat-2 satellite data to improve FSM2 simulations. The authors employ the DA proposed by Alonso-Gonzales et al. (2023) with a spatial propagation of the sparse data points from ICESat-2 that tries to compensate the fact that ICESat-2 data are acquired in profiles with many temporal and spatial gaps. They perform three experiments to assess the effectiveness of assimilating different data types snow cover area, from Sentinel-2, snow depth, from ICESat-2 or both. The reported findings indicate that by incorporating snow cover area data alongside snow depth from ICESat-2 led to the most accurate snowpack simulations.

Reply:

We appreciate the Reviewer's interested approach to the paper and are grateful for the constructive suggestions that have helped us to improve the study. We kindly point out that the aim of the study is not primarily to improve the FSM2 simulations in the study catchment per se, but to present a newly developed method that allows the joint assimilation of snow cover data and novel ICESat-2 snow depth profiles that are sparse in time and space and thus currently of very limited use for snow modelling. The method presented here is designed to be globally applicable. ICESat-2's satellite-derived profiles have a very different nature and greater uncertainty than the experiments assimilating subsampled, within-catchment drone-based snow depth data presented by Alonso-González et al. (2023). Our method builds on this earlier work and the spatio-temporal data assimilation method therein to show how actual satellite-derived snow depth data from ICESat-2 can be propagated/transfered in space and time from outside the catchment using to yield spatio-temporally complete reconstructions of the full snowpack state in the catchment. Moreover, we also show (to the best of our knowledge) for the first time how to perform joint spatio-temporal assimilation of both ICESat-2 snow depth and Sentinel-2 fSCA so as to exploit the highly complementary nature of these two types of observations (Gascoin et al., 2024). In addition, we provide a thorough uncertainty quantification of the respective simulations, which is not trivial in the case of spatially propagated information from observations located near but outside the study area. The following provides a point-by-point response to the Reviewer's Comments.

COMMENT # 1.2

The authors leverage a data assimilation system (MuSA) from a previous work (Alonso-González et al., 2023). Moreover, the methodology for spatializing the ICESat-2 data (a point of significant interest) builds upon concepts presented in the previous work, but without substantial further development. While the initial findings are interesting, their persuasiveness could be strengthened through further analysis. This deeper exploration would allow the research to culminate in a more robust and impactful paper.

Reply:

We agree that the core simulations of this paper relies on the MuSA assimilation system, which was developed in previous work (Alonso-González et al., 2022, 2023). However, we disagree with the rest of the Reviewer's statement while acknowledging that the novelty of this study could have been better emphasized in the paper. As such, we would like to highlight the differences and the method development we have accomplished to make these new experiments possible:

- Despite our short description in Section 3.1, a substantial effort was necessary to treat the ICESat-2 elevation observations in order to retrieve snow depth observations, as there is not an established method to do that. Careful data curation was required to eliminate outliers so that the observations could be ingested into a data assimilation system.

- To the best of our knowledge, this is the first work that goes beyond validating ICESat-2 data with snow depth measurements from another source, but instead assimilates them to constrain a snow model.

- The MuSA assimilation system was updated in order to spatially propagate information coming from a subset of the observations while doing a so-called joint data assimilation (assimilating multiple different types of observations). Because of the way we define a neighbourhood, a large number (many hundreds) of cells with fSCA-observed cells would be included in the neighbourhood. The computational burden would then increased drastically due to the resulting large input/output operations to access all neighboring fSCA predictions and observations for every Kalman update. The previous MuSA system, without the upgrade done in this study, would have required the resources of a larger supercomputer for such a joint assimilation. The updated assimilation method was implemented through modifications in the spatial_MuSA module of the system, changing the subset of observations that are considered to create a neighbourhood of pixels for domain localization that are considered for the state update. See the pull requests 14-17 in the MuSA repository.
(https://github.com/ealonsogzl/MuSA/pull/14)

- For this work, we used the MuSA system to perform joint assimilation in a spatio-temporal setting. While the MuSA system has already been used to perform joint assimilation (Alonso-González et al., 2022), there the observations were land surface temperature and fractional snow-covered area but both were spatially complete products so this was done for a single pixel in a purely temporal ("1D", or "embarrassingly parallel") setting without any spatial propagation of information. Here we instead jointly assimilated snow depth and fSCA in a spatio-temporal setting whereby in the previous works snow depth was the only observation assimilated in experiments with spatial propagation. To the best of our knowledge, this is a completely novel snow data assimilation approach. In the described experiments, the two observation types (fSCA and snow depth) that we either jointly or individually assimilate have a different spatial coverage. This made calibrating the relative accuracy of the observations and the selection of the spatial propagation parameters such as the length scale a relatively tough exercise since these hyperparameters both have a considerable effect on the relative weight of the two sets of observations influencing the state of the simulations as well as on the spatial distribution of the simulation.

We realise that we may have undercommunicated the methodological development and MuSA updates required to achieve the results and framework presented in this study. In the revised manuscript, we better emphasize in the Introduction, in the Methods and in the Conclusions the novelty of the approach and how it differs from previous work.

COMMENT # 1.3

While the current findings are interesting, further analysis could significantly enhance their persuasiveness. Consider incorporating other inputs data that only ERA-5 (also derived from in-situ) and also move to other (larger) catchment where distributed HS are available (e.g., ASO data if ICESat-2 data are available or Dischma in Switzerland) to solidify the results.

Reply:

We are grateful for your acknowledgment of the interest of our findings and approach. To a limited extent we agree that further analysis would enhance their persuasiveness. We would like to address your suggestions on adding different forcing data and other study sites separately.

Study site: we appreciate your suggestion to experiment in larger catchments with distributed snow depth available. However, we propose to keep this as the sole study site because of the acknowledged interest of the results shared by you and the other Reviewer, and expand the analysis on the utility of ICESat-2 observations to infer snow spatial-distribution point of view in the future (as suggested in Section 5). To our best knowledge, the locations you suggest are much larger and have not been surveyed by airborne instruments with the same temporal frequency available in the current study area (around 12 acquisitions in the studied season). The lower temporal resolution of distributed snow depth dataset could limit the solidity of the analysis on accumulation and ablation season. Moreover, high resolution and large scale DA exercises require substantial further model development or changes to the output resolution in order to be computationally affordable (e.g., not to simulate in a fully distributed manner or at a much coarser resolution). We therefore consider this possibility worthy of a complete new and separate study. Considerations about this are added in the Discussion.

Forcing data: the second suggestion is about incorporating in the analysis other input data than only ERA5, such as data derived from in-situ observations. We acknowledge that, if the scope would be to achieve the best possible snow simulation of this specific basin, it would certainly be the better option to use a continental or even national reanalysis together with in-situ data from a local meteorological station. However, we underline that this study was designed to showcase a globally applicable workflow where high-resolution forcing data is usually lacking – and the assimilation of observations is used to achieve distributed result maps. Note that one of the main motivation for using satellite DA in snow modeling is precisely to fix errors in the forcing data, so it is particularly worth demonstrating that these methods are able to work with coarse globally available meteorological forcing in line with many previous snow DA studies (e.g. Fiddes et al., 2019; Alonso-González et al., 2022, 2023). Moreover, several other DA studies are based solely on one global reanalysis as input forcing (in these cases MERRA or MERRA-2) such as Cortés et al. (2016) or Liu et al. (2021). Hence, we used the current state-of-the-art (in terms of resolution and accuracy) *global* atmospheric reanalysis ERA5. Its original spatial resolution clearly misses the hillslope scale heterogeneity, this we obtain partly through a preliminary topographic downscaling routine (TopoSCALE; Filhol et al., 2023) but mainly via the information contained in the observations that we assimilate. We considered this a sufficiently general prior knowledge of the seasonal snow evolution to be able to claim global applicability of this workflow, as it will arguably be most useful for other (less studied) areas where large knowledge gaps on snow amounts are existing and no regional forcing data (let alone in-situ observations) exist. In our opinion, adding another reanalysis to the experiments would confuse the reader and not add much value to the study for the following reasons:

- Starting from a higher quality prior knowledge of the seasonal snow evolution would leave the reader with the question: is this approach of any value for remote places (e.g. Central Asia) where such improved prior information is not available?

- The hillslope processes (100 m scales and smaller) we aim at simulating would not be represented in any higher resolution atmospheric reanalysis even using costly state-of-the art convection permitting atmospheric models which are at best at the kilometer rather than the hillslope scale.

We realise that we may have not emphasized enough that the suggested workflow was designed to be globally applicable. In the revised manuscript, we better underline this aspect when motivating the methodological choices both in the Introduction as well as in the Methods.

COMMENT # 1.4

Revisit the scientific questions the paper aims to address. Sharpening these questions will guide the research and ensure the experiments directly address them.

Reply:

Good point, we previously only stated a hypothesis at the end of Section 1. We now replace it with three specific research questions as we agree this can guide the reader to understand the idea behind our experiments.

Changes:

The two datasets have complementary features: ICESat-2 retrieves snow depth directly, but only along profiles; while fSCA has an indirect ~~correlation~~ relationship with snow depth, but ~~this dataset~~ is spatially distributed. ~~Our hypothesis is that the joint assimilation will be able to exploit these to better infer the seasonal snow evolution.~~ The novel scientific questions we aim to answer are:

- Can information from sparse snow depth retrievals from ICESat-2 along profiles be used to provide information about average catchment-scale snow depth and its complete spatial distribution?

- Is assimilating sparse ICESat-2 snow depth retrievals better than more commonly used fSCA observations derived from optical satellites?

- Is ensemble-based DA able to able to leverage information from both observation types when jointly assimilating both fSCA and sparse snow depth observations?

Comment # 1.5

Section 3.5 appears to hold the core of the paper contribution. Dedicating more space and development to this section would allow for a more thorough exploration and potentially lead to more impactful conclusions.

Reply:

We fully agree with both the Reviewers' Comments stating that a deeper explanation of how information from observations is propagated in space is needed. The same suggestion came from Reviewer 1 in Comment 1.7. We propose to expand of Section 3.5 in the revised version of the manuscript to guide the reader better concerning the spatial propagation/transfer of information. See Comment 1.7 in this document for more details.

Detailed comments:

Comment # 1.6

The introduction of the paper could benefit from being condensed and sharpened. Focus on presenting the key scientific questions, the research aims to answer, and clearly outlining the paper main novelty (difference with previous works). This will ensure the experiments directly target those questions and guide the research direction. The core innovation of the paper lies in applying the DA method (from Alonso-Gonzales et al. 2023) to ICESat-2 data. The unique approach for propagating spatial and climatological information holds significant promise. However, further development of this methodology is necessary, particularly regarding the justification for using data outside the area of interest for analyzing snow accumulation and redistribution (see next points).

Reply:

Agreed, we have revised and shortened the introduction according to the structure proposed by the Reviewer as well as Comments from Reviewer 1, and also added research questions that the paper aims at answering. We take care to explain why we use snow depth observations outside the study area, which we find is one of the strengths of the proposed approach as it shows the utility of the sparse observations measured by the satellite ICESat-2 profiles. Our study catchment has only nearby ICESat-2 observation available, and the situation will be the same for many other catchments of interest around the world, but the utility of ICESat-2 in snow modelling is still of interest.

COMMENT # 1.7

Figure 3, potentially the paper core novelty, requires a more detailed explanation. From the scatterplot, it appears there might be a weak correlation between snow depth and CSMD (and TPI24). However, the relationship between snow depth and Sx200 seems less clear, potentially indicating no significant correlation. At least this is my understanding with the provided text. If this is not correct, I suggest a clearer description to enhance reader comprehension explicitly guide them through the correct explanation.

Reply:

We originally included the Winstral index (Sx) because both Revuelto et al. (2014) and Mendoza et al. (2020), who carried out studies about the snow spatial distribution in the Izas experimental catchment, recommended the adoption of the Winstral index in addition to TPI when predicting snow depth. When exploring what dimensions to choose, we analysed their interplay in an explorative way and found that although Sx does not exhibit a linear correlation with SD, in combination with CSMD this dimension is a relevant predictor of SD (e.g. no low SD observations with a low Sx). However, due to the considerable cost of running the DA experiments we did not perform a complete factorial exploration of all possible feature dimensions. Nonetheless, following this particularly astute Reviewer Comment about the weak correlation between Sx and snow depth, we have now repeated the ensemble simulations for all DA experiment runs excluding Sx from the dimensions of the feature space. We were positively surprised to see a slight improvement in the results. We believe that in our case, the inclusion of that index is not beneficial because of the large amount of ICESat-2 observations that were located in an area with negative Winstral index, a characteristic shared with only few cells in the drone domain. It is possible to see this in the first submission's Figure 3. It seems that limited representativity of ICESat-2 data for the catchment topography in terms of Sx was leading to smaller correlation values and, consequently, a small influence of the observations for the cells of the simulated domain.

Hence, we propose a large change of Section 3.5 as you suggest in Comment 1.5, to guide the reader in understanding the spatial propagation of information. We removed Sx from the predictors and also add Figure 1 (see below) to the manuscript. The Figure exemplifies a situation where a cell in the experimental catchment with drone data – depicted in panel b) with a cross – has to be updated. The solid points in the scatterplot are selected to be part of the neighbourhood, and all of them have

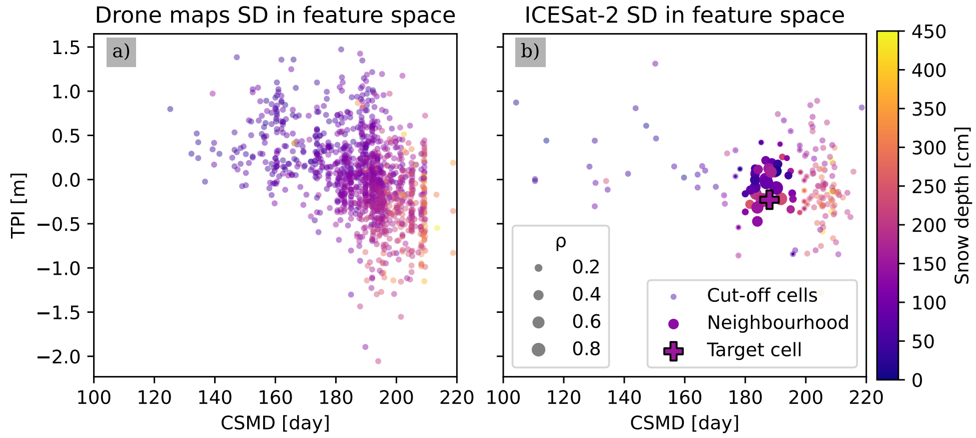

Figure 1: *Panel a): scatterplot depicting the position of the cells from the drone maps in the feature space. This space – created with TPI and CSMD – is adopted to define the similarity between cells. The points are colored according to the snow depth observed with the drone. Panel b): ICESat-2 snow depth observations in the extended catchment, displayed in feature space, with snow depth-based coloring. The cross represents one cell from the drone domain where a snow depth of 150 cm was measured. The solid points are ICESat-2 data points included in the neighbourhood for this cell, with their size proportional to the correlation* ρ.

influence on the Kalman update (see step 11 in Algorithm 1 of Alonso-González et al., 2023), used to update the local ensemble of the target grid cell. As cells closer in feature space to the target cell should have a larger influence, their ρ is larger, which can be appreciated by looking at the size of the scatter points.

COMMENT # 1.8

Please revise the text from L276-282 to make more clear (and less compressed).

Reply:

This part of the text has been extended as part of the answer to Comment 1.5. See above the answer to Comment 1.7 for more details.

COMMENT # 1.9

The paper relies solely on ERA-5 data for atmospheric forcing. While ERA-5 is a valuable product, acknowledging the existence of other models with potentially significant output variability (up to 100%) would strengthen the main message of the paper that ICESat-2 data can be useful and in which situation. A discussion on why ERA-5 was chosen over other op-

tions would be beneficial. However, for a more robust understanding of the proposed method I suggest incorporating data from at least a couple of additional models is recommended. This comparative analysis would highlight the method sensitivity to different forcing data. In particular, given the small target catchment area (and the high target resolution of 20m), exploring the use of spatially distributed data from nearby in-situ stations could be highly valuable. This would provide a more realistic scenario: not sure the first choice to simulate 5ha at a resolution of 20m in an experimental catchment in a European mountain range is starting from a 30km ERA-5 data.

Reply:

We thank the Reviewer for the suggestion of a better argumentation for choosing ERA5 as the (only) source for forcing data. We point to the answer to Comment 1.3 and will not repeat the arguments here.

COMMENT # 1.10

The results of experiment D are puzzling (at least to me in the present form). While Figure 3 (and related text) suggests that ICESat-2 data captures the relationship between SD distribution, topography, and climatology using this data alone in experiment D appears to yield inaccurate snow patterns, whereas it helps in experiment J when used together with Sentinel-2, what is the main mechanism behind this behavior?

Reply:

We also find this result intriguing and did not find a simple answer. Below, we outline what factors likely contribute to the relatively poor result for experiment **(D)** but improved performance of experiment **(J)**. The spatial patterns we see in the distributed maps in the Results are governed by the features that design the spatially correlated prior. We agree that in experiment **(D)**, the snow patterns are inaccurate – but the basin average snow depth is greatly improved. The spatial patterns are a result of the features and their relative weights, which are the hyperparameters of the prior that were chosen from various tested combinations/variations, but not inferred or optimized. Adding fSCA observations – and hence moving to experiment **(J)** – adds to the local updates for the cells in the drone catchment a cumulative information about the accumulation and melt processes. Melt-out patterns are reproduced into snow patterns in the peak-SWE maps we show, while the snow depth profile provides information to adjust and improve absolute snow depth values.

COMMENT # 1.11

A crucial evaluation metric for DA methods is computational time. The paper should explicitly report and analyze this metric, ideally providing a detailed profile for each operational step.

Reply:

The simulations were run on a local server from the Department of Geosciences of the University of Oslo. It is equipped with a 1TB RAM and 40 processors were used for this task. However, there was high variance in computational time depending on the varying load on the server. For the three experiments the computational time was similar and at best it took 7 hours, or at worst three days. The computational cost depends on the GC parameter and on the density of observations, so it is hardly comparable to simulations in other sites. Moreover, we note that the current implemetation of MuSA is a wrapper around the Fortran implementation of FSM2. Simply improving FSM2, for example by translating it to the Python programming language, or other software-side improvements related to the observations use might greatly improve the computational time. Further development of MuSA with regard to computational efficiency is planned.

We acknowledge that computational time is a crucial factor for readers to know about the applicability of the method, and the order of magnitude will be mentioned in the revised manuscript in Section 3.6. However, technical differences in the implementation can make the computational time vary dramatically (see above). In this publication, the focus is on the scientific questions related to the utility of ICESat-2, and we prefer to avoid a lengthy technical sidetrack.

COMMENT # 1.12

Fig 7 can you add the drone maps? Beside demonstrating a significant improvement in an accuracy score, the scientific community is starting to become interested in understanding how realistic snow distributions become when observations at high resolution are assimilated into models. The paper could benefit from a stronger emphasis on this aspect.

Reply:

Two drone maps will be added to Fig 7, depicting the average snow depth for both the accumulation and melting seasons, so that the reader can compare the results with the absolute amounts.

COMMENT # 1.13

While Figure 7 presents the CRPS for various scenarios, a deeper analysis could help isolate the contribution of ICESat-2 data. The similarity between the CRPS with ICESat-2 and the prior suggests limited influence of ICESat-2. However, the simultaneous improvement in J (potentially reflecting the contribution of ICESat-2) is intriguing and puzzling at the same time. Can you better comment on this?

Reply:

We thank the Reviewer for the comment. For our experiments, one should note from the time series of Figure 4 that the validation maps average (black points) lie very close to the median of the prior (gray lines) during the accumulation period, hence the prior simulation already provides an accurate (albeit not precise) simulation in terms of average snow depth for this period. As a consequence, a gain on the CRPS score in the accumulation season is in our experiments harder to achieve than in the melting period, as there the prior average snow depth is quite far from the validation points and an improvement is thus easier to improve. In experiment **(D)**, there is a substantial improvement given by the diminishing of the spread of the ensembles. This is shown in the panel a) of the first submission's Figure 5 in terms of basin-average snow depth, which corresponds to a CRPS improvement of 14%. We agree that it's puzzling that this improvement is not better than what we achieve with fSCA-only assimilation. In the fSCA case, the basin average snow depth does not improve (see Figure 4, panel a)), where the blue trajectories overestimate the validation black points overall), but the reduction in CRPS is caused by a better relative spatial distribution of snow depth. The latter is visible from the similarity of panels b) and c) of Figure 4 as well as from the mostly uniform CRPS values in the spatially distributed map for experiment **(C)** in Figure 7. This is point is crucial: the improvements obtained by assimilating spatially complete fSCA and sparse snow depth are complementary, hence the large improvement we see in experiment **(J)**. We acknowledge it is important to underline this crucial point and we add this arguments in the discussions of the revised manuscript.

COMMENT # 1.14

L430: the fact that SCA assimilation doesn't improve the simulation during the accumulation period could be attributed to the fact that the area of interest is having 100% snow cover?

Reply:

The assimilation of fSCA using ensemble-based smoothers is a topic which has received many studies (e.g. Girotto et al., 2014; Margulis et al., 2015; Aalstad et al., 2018; Fiddes et al., 2019; Alonso-González et al., 2021). If the assimilation method acts as a filter, it acts sequentially: the observations modifies the state of the simulation at the current time, and the past (relative to the current observation) is not affected. Otherwise, for a batch smoother, (as in the presented experiments herein and references above), the information from an observation can also propagate backwards in time, as all the observations in the current water year are assimilated at once. It has previously been convincingly shown that fSCA assimilation can improve the seasonal snow simulations also in the accumulation season if such a smoother is employed because fSCA observations contain cumulative information about both accumulation and melting processes, indeed this is the key behind state-of-the-art probabilistic snow reanalyses (e.g. Margulis et al., 2016) and the earlier deterministic snow reconstruction techniques Girotto et al. (2014). However, since the information is integrated over both accumulation and melting-related parameters, equifinality problems can arise. That means there is not enough information in the fSCA observations to infer all the perturbation parameters, and very heterogeneous sets of parameter can be used to reconstruct the states as they're observed. This is the likely cause of the missing improvement in the fSCA experiment in terms of catchment-average snow depth, but note that there is an improvement in the CRPS score.

COMMENT # 1.15

L439: this does not seem true to me (at least in the present form).

Reply:

We agree, the sentence was a leftover from an earlier version of the results section. The paragraph will be changed:

Changes:

...As Figure 5 shows, this information leads to a more precise reconstruction of the catchment-average peak-SWE compared to experiment **(C)**. This demonstrates that the ~~spatio-temporal DA is successful, as the information propagated from observations outside the Izas catchment carries more or at least a similar amount of information compared to the temporal-only information propagation that happens in experiment **(C)**.~~ spatial transfer of information method succesfully relates snow depth and the features, but only when averaging over the whole basin, as the ~~Compared to experiment **(C)**, this simulation has a better agreement with the observed snow depth histogram distribution, as the range of the snow depth histograms has a better match (panels~~

~~d) and e), Figure 5). However, the~~ relative spatial patterns of the simulation only partially match those of the validation maps (panels b) and c), Figure 5).

COMMENT # 1.16

L442: this is in contradiction with L253 where it is stated that the snow depth is strongly governed by topography, which is also the main hypothesis why the proposed approach has been applied. This ping pong effects makes it challenging to understand the overall benefit of ICESat-2 data (and the validity of the presented results).

Reply:

We agree that this is a contradiction, and we propose to modify both the sentences to remove the challenges for the reader. The consideration at line L442 will be modified to include that part of the feature space occupied by the drone domain is not covered by ICESat-2 observations.

Changes:

L253: . . . As the relative snow depth's spatial distribution ~~of snow depth~~ is strongly governed by topography, . . .
L442: . . . ~~Since the~~ The observations we use in this experiment are not direct measurements in the ~~catchment,~~ experimental catchment, so this result is in the end not surprising~~: the similarity measure we define is only partially able to propagate snow depth information properly. Nevertheless, single pixels~~. While the entire area experiences the same general snow conditions there are local differences which can be only partially captured with a low dimensional space, as TPI and CSMD do not fully characterize the snow depth distribution. For example, single cells with extreme values located in the ~~basin~~ experimental catchment might not be similar ~~(~~in terms of ~~topography and meltout date) to the ones which are~~ TPI and CSMD to only cells observed by ICESat-2 with as extreme snow depth values, but also to medium snow depth observations, not getting the ideal update.

COMMENT # 1.17

L452: speculative, please revise it.

Reply:

We rephrased.

Changes:

~~If the previous suggested improvements would improve the results of the experiments for this setting, this could be used, in principle, in a forecasting system, as snow depth observations have instantaneous value; while fSCA are more useful in a reanalysis setting. There would be the need to speed-up the ICESat-2 processing for the low-level product, as it is now usually three~~ As the ICESat-2 satellite will potentially collect data until the mid-2030s, and the snow science community is eager to keep on testing its potential to evaluate mountain water resources, this dataset has the potential become a functional tool for water managers to estimate the maximum seasonal snow accumulation. However, especially within an operational snow hydrological forecasting context (Mott et al., 2023), there is a clear need to reduce the processing time of the geolocated photon low level data which currently takes months.

COMMENT # 1.18

L484: 20 m is not hyper resolution.

Reply:

We acknowledge that the level of spatial resolution is a term relative to the context. For example, in climate modelling hyper-resolution is kilometric (Wood et al., 2011), while in non-ensemble forest snow modelling hyper-resolution is submetric. In the context of snow and hydrology ensemble-based DA where a cell is simulated numerous times, recent studies have defined their resolution as hyper for cell sizes well below 100 m (Fiddes et al., 2019; Alonso-González et al., 2023). In contrast, snow DA simulations with cell sizes of about 100 m typically define their spatial resolution as high (i.e., one level coarser than 'hyper') (Margulis et al., 2016; Girotto et al., 2020). Moreover, high resolution is also applied in recent literature to snow DA reconstructions at kilometric grid cells (Oaida et al., 2019; Brangers et al., 2023). At least within the context of snow DA, calling ensemble simulations at 20 m hyper resolution is warranted. In conclusion, we believe that this paper is not the location where an arguably ill-posed unified interdisciplinary definition of spatial resolution should be discussed. For such reason, we keep the term hyper in line with previous snow DA work.

COMMENT # 1.19

References are generally ordered alphabetically.

Reply:

The references were already ordered alphabetically by last name of the first author of each paper as is the norm, but we appreciate that the reference list is somewhat confusing given that it also included the full first names of all authors rather than just initials. The first names in the reference list will be replaced by initials in the final typeset version of the manuscript.

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