REVIEWER #1

COMMENT # 1.1

> *The authors clearly put a good amount of effort into addressing comments from the previous round of reviews and the manuscript is greatly improved. I do not have any major concerns about the manuscript but I have a number of minor comments that should be addressed by the authors prior to consideration for publication.*

Reply:

We appreciate the Reviewer's positive acknowledgment of our changes to the manuscript and thank them for their contribution and careful reviews. The suggested textual changes are helpful and greatly appreciated. The following provides a point-by-point response to the Reviewer's minor comments.

Minor comments:

COMMENT # 1.2

> *Line 3: Remove ", despite being of great societal interest" because it makes the sentence somewhat awkward with where it is located in the sentence.*

Reply:

We thank the reviewer for spotting this. We removed the sentence.

COMMENT # 1.3

> *Line 15: You currently end the abstract with stating that the skill score is improved by 19%. I generally recommend that abstracts include quantitative results if possible but this is hard to interpret without the reader knowing what the skill score means. You can keep this if you'd like but I recommend adding another sentence afterwards that summarizes the performance of the joint simulation so that the reader can easily understand the broader importance of the work. Are spatial variations in snow depth reproduced more accurately? Are temporal patterns in catchment-wide averages reproduced more accurately during the accumulation and/or ablation season? Providing this sort of information in the abstract will let the reader interpret the promise of the method even if they aren't sure how to interpret the skill score.*

Reply:

We appreciate the Reviewer's help in more carefully conveying the key messages in the abstract. We extended the abstract to detail better the joint assimilation results and the contribution from ICESat-2.

Changes:

Another encouraging finding is that adding the snow depth profiles to fractional snow-covered area observations leads to an accurate reconstruction of the snow depth spatial distribution~~, improving the skill score~~. Evaluating the simulations with a set of independent drone-based snow depth maps using a probabilistic skill score, we find that for the accumulation season the joint assimilation's score improves by 19% ~~in the accumulation season~~the established approach of only assimilating fractional snow-covered area. The direct but incomplete snow depth information from ICESat-2 is a key constraint on simulated basin-average snow depth.

This study makes use of globally-available datasets and shows the promise of adopting ICESat-2's snow depth retrievals in seasonal snow modelling, especially when also assimilating complementary observations. In light of our encouraging results, more research with different experimental designs in varying snow conditions combined with continued methodological development is desirable to further catalyse the use of these retrievals in cryospheric and hydrologic applications.

COMMENT # 1.4

lines 21-23: Instead of ending this sentence with a focus on measuring variability in snow "from space", I would say something like "at a watershed-scale using remote sensing methods" because you mention satellites, aircraft, and drones earlier in the sentence and the issue is really that we have problems getting accurate estimates across full watersheds.

Reply:

We recognise the confusion in the sentence, and modify accordingly:

Changes:

Despite the many approaches involving satellite, airborne and drone sensors of various different types currently being used, accurately measuring the temporal and spatial variability in snow amount (i.e., mass or depth) ~~from space~~ at watershed-scale with remote sensing is still a major scientific challenge (Gascoin et al., 2024).

COMMENT # 1.5

line 42-43: I found this sentence difficult to read. Consider revising this sentence and the one afterwards to be more straightforward with the fact that the data-assimilating intermediate snow model helps overcome issues with the use of coarse-resolution and potentially inaccurate large-scale atmospheric reanalyses for hydrologic forecasting.

Reply:

We simplified the structure of the thought as follows:

Changes:

~~Observations such as snow depth or fSCA can mitigate the main limitation of all snow hydrology models which has been shown to be~~ The main limitation for large-scale snow hydrology modelling is the accuracy of atmospheric forcing data (Raleigh et al., 2015). This applies especially ~~in a potentially globally-applicable~~ to spatially-distributed ~~setup when~~ setups where the forcing needs to be extracted from large scale atmospheric model outputs such as coarse-resolution (30 km) global atmospheric reanalyses (e.g. ERA5; Hersbach et al., 2020).

COMMENT # 1.6

lines 47-61: Consider revising some of the sentences in this paragraph so that they do not focus as much on the authors of the referenced papers. When you start a sentence with the authors of a paper, you automatically focus on who did the work and not what the work tells you. For example, "Girotto et al. (2020) noted that most snow DA research – with a few exceptions – has focused on purely temporal DA…" could be revised to "Most snow DA research – with a few exceptions – has focused on…" without changing the message but taking the focus off of the citation.

Reply:

Thanks for this useful advice. We agree that the attention is on the authors of that paper, but in this case, we chose to do that as the cited paper is a review paper (Girotto et al., 2020) where an important limitation of current snow DA is highlighted. Changing the order would put this review (which we want to keep in the main sentence) on the same level as the few exceptions. We hence keep this particular phrase as is. However, we have changed another sentence in the paragraph which fits under the point made by the reviewer:

Changes:

... ~~De Lannoy et al. (2022) recommend a~~ A greater adoption of spatio-temporal multivariate DA is recommended (De Lannoy et al., 2022). ...

COMMENT # 1.7

line 61: Remove the end of this sentence "‚as recommended by comment 6 in Anonymous (2023) and by Gascoin et al. (2024)" because it really isn't needed

Reply:

We acknowledge that the phrase could be fine without the ending as the Reviewer suggests, but we would like to keep this to show that the community has previously expressed the need for such experiments as those that we are carrying out in this work.

COMMENT # 1.8

lines 82-87: The first sentence in this paragraph indicates that the paragraph is going to describe a hypothesis but that is not true. Please revise at least the first sentence.

Reply:

Right, we change to:

Changes:

~~The underlying hypothesis~~ An important assumption of this work, shared with other high-resolution DA assimilation studies, can be outlined as follows...

COMMENT # 1.9

line 154: Typo "method"

Reply:

Thanks for spotting, corrected.

COMMENT # 1.10

line 156: Typo "(Fiddes et al. 2019)"

Reply:

Corrected.

COMMENT # 1.11

line 168: Change "a 3 m height" to "a height of 3 m"

Reply:

We changed the text to make it more readable.

COMMENT # 1.12

lines 173-178: For coregistration, you are working with an ICESat-2 track with snow across at least a portion of the domain. Did you only use photon from snow-free areas in your coregistration process? If so, how did you identify them as snow-free? If you used all the photons, that needs to be stated. Either way, it needs to be clear what photons were used because ultimately that impacts the accuracy of your snow depth estimates and therefore the model performance

Reply:

We thank the reviewer for helping make this part of the methods clearer. The co-registration process does not use only the snow-free area, as there are not enough of these areas in the used DEM, but we make use of all the "selected" photons (according to the method defined in the lines 165-173). We modified as follows to explain that each beam is coregistered with the snow-off DEM.

Changes:

Before comparing the ATL03 photon events to the snow-free reference surface elevation it is necessary to co-register this dataset with the snow-off DEM. ~~Every beam is independently co-registered with a horizontal displacement. A vertical offset common to all the acquisitions and beams is obtained by computing the median of all the snow-off acquisitions vertical offsets, as other studies have done (Enderlin et al., 2022; Besso et al., 2024).~~ We employ the Nuth-Kääb algorithm to obtain the horizontal shifts using as input all the selected ICESat-2 photon events and the snow-off DEM elevations (Nuth and Kääb, 2011), implemented in the xdem python library (Dehecq et al., 2021). Every beam is independently co-registered to account for different horizontal displacement. Individual vertical offsets (expected due to snow cover) are not removed; instead, we correct with a common offset

between the DEM all snow-free ICESat-2 data following established practice (Enderlin et al., 2022; Besso et al., 2024).

COMMENT # 1.13

Table 1: Typo "hyperparameters" in the caption. Also, I do not understand how the mean of the precipitation hyperparameter can be a negative number and outside the bounds that are provided. If that is not a typo, then these values should be explained more in the text because their interpretation is not straight-forward.

Reply:

Thanks for the comment, we corrected the typo. The effect of a negative mean parameter of the associated Gaussian distribution for the logit-normal prior on the precipitation is to have a right-skewed (left leaning) density distribution for the precipitation perturbation. It is important to note that this is not the mean of the logit-normal distribution itself, but rather the mean of the associated (transformed) normal distribution. The logit-normal is usually specified this way since given its analytical form it makes more sense to define it in terms of the parameters of the associated normal distribution. In fact, there is no analytical expression for the mean or variance of the logit-normal distribution. This is described in detail in (e.g.) (Aitchison and Shen, 1980). It is nonetheless possible to analytically compute the median of the logit-normal by taking the scaled expit (inverse logit) transform of μ so we report this in the text. To better, albeit briefly, explain these details, we modified the paragraph where the prior perturbation extraction is explained.

Changes:

The perturbation parameters are time-invariant throughout the water year, and are extracted from a logit-normal distribution whose prior hyperparameters ~~μ and σ~~ can be seen in Table 1. ~~We choose this~~ The prior perturbations are obtained by extracting samples from an associated normal distribution with mean μ and standard deviation σ. Subsequently, the inverse logit transform (sometimes called expit) with scaling is applied, resulting in a logit-normally distributed sample ranging from the lower to the upper bound (see section 3.3.1 in Aalstad et al., 2018). We choose the logit-normal distribution over a log-normal or a ~~Gaussian~~ normal distribution as the logit-normal restricts the perturbation within defined upper and lower bounds (shown in Table 1), in contrast to other distributions which would have respectively only one or no bounds (Aitchison and Shen, 1980). The nature of the perturbation is multiplicative for the precipitation (in part to prevent non-physical negative values) and additive for the other variables. The negative-mean parameter of the

associated normal distribution in the multiplicative perturbation parameter for the precipitation results in a right-skewed (left leaning) prior distribution with a median of 1.5.

COMMENT # 1.14

line 435: It took me a few reads to understand this sentence. My interpretation is that fSCA in the accumulation season doesn't tell you much about snow depth because nearly the entire domain can be covered in snow and the depth can vary quite a bit but once you have melting snow and ground is exposed, the depths of the remaining snowpack are more consistent. If my interpretation is correct, try rephrasing so that this point is made more clearly.

Reply:

We thank the reviewer for helping clarifying this. We inverted the order of the sentence and added a motivation to explain this.

Changes:

In experiment **(C)**, we simulate the snowpack and assimilated fSCA to create a baseline. ~~It has been shown that, despite fSCA exhibiting a lower instantaneous correlation with early season snow depth in a deep snowpack~~ The fSCA generally shows lower correlation with snow depth earlier in the snow season (when the ground is fully covered by snow) compared to the ~~end of the season with a melting snowpack (Girotto et al., 2020), the assimilation of fSCA allows~~ melting season (Girotto et al., 2020), but previous works show that assimilating fSCA can allow for an accurate reconstruction of peak SWE ~~(Girotto et al., 2014)~~(e.g. Girotto et al., 2014). Indeed, experiment **(C)** shows high accuracy and precision especially towards the end of the season. As the experiments adopt a ~~smoother approach, such information~~ smoothing approach, the melt-out pattern information contained in the fSCA observations is also propagated backward in time, and the posterior simulation offers a relatively accurate reconstruction for the peak-SWE: the validation is clearly close to the median ensemble spread, but the reconstruction for this part of the season is less precise compared to experiment **(D)**, and both less precise and accurate compared to experiment **(J)**.

COMMENT # 1.15

lines 464-466: After this sentence you should point out that ideally you would have incorpo-

rated another ICESat-2 profile based on the known improvement in simulations with ablation season observations, but you didn't have a good track from that time period

Reply:

We thank the reviewer for suggesting to add this consideration in the discussion.

Changes:

As a recommendation, we note that it would be ideal to add more ICESat-2 observations later in the water year so as to better constrain the melt season. Moreover, further experimenting with different spatio-temporal configurations for the assimilated observations in different snow environments could help shed more light on the utility of ICESat-2 in the context of seasonal snow modelling.

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REVIEWER #2

COMMENT # 1.1

The authors have clearly put considerable effort into revising the manuscript based on the reviewers' feedback. The revisions have resulted in a more focused presentation, and the clarification of several key steps has significantly improved the manuscript clarity. While the assimilation of ICESat-2 data represents a significant strength of this study, I believe further in-depth investigation could optimize its impact. Specifically, exploring the sensitivity of the assimilation results to factors such as the number and temporal distribution of ICESat-2 acquisitions, as well as the specific snow conditions (e.g., accumulation vs melting, or shallow vs. deep snowpack) at the time of acquisition, could yield even more robust results. Dedicating more research time to systematically analyzing these aspects might allow for a deeper exploitation of the unique capabilities of the ICESat-2 dataset for snow assimilation, potentially revealing optimal acquisition strategies for different scenarios. Furthermore, while the authors present a promising methodological approach, the current validation, limited to a single, small catchment in the Pyrenees, raises concerns regarding the generalizability and global applicability of the findings. Claiming global applicability would necessitate a significantly more geographically diverse set of experiments, encompassing a wider range of snow regimes, terrain complexity, and climate conditions (at least in my view). While the current results provide a valuable initial foundation, the study is still in the early stages of demonstrating broad applicability. Given that the exploration of ICESat-2 data assimilation in snow models is still in its nascent stages, clearly outlining these limitations and future research directions will be crucial for the manuscript impact and for guiding future research in the field.

Reply:

We thank the Reviewer for contributing to the improvement of the manuscript through the previous review, and for acknowledging the effort the authors have put into improving the clarity and focus of the manuscript.

We agree that further in-depth investigation and methodology development is desirable in the future. In particular, wider experiments with different dates of acquisition, snow conditions will shed light on when ICESat-2 observations are the most valuable. Moreover, we agree that further methodological development is needed to use ICESat-2 observations on large-scale snow reanalysis.

We are grateful that the reviewer evaluates our methodological approach as promising. We see this as the first step towards a wider use of ICESat-2 for cryospheric applications, and think it would be presumptuous from our manuscript (the first using ICESat-2 observations in a DA setting) to give a final answer or to define one method that works for all the possible applications. We also want to push back a bit by saying

that this first step we took could have been much smaller. For example, the experiments could have been carried out at the point scale or along a single ICESat-2 profile. Instead, they were conducted in a fully spatio-temporal joint data assimilation setup at very high resolution for an entire catchment (not directly observed by ICESat-2). In the manuscript, we underline that the datasets used are available globally, and this was a deliberate choice because it was developed so that it could be applied in different regions where larger uncertainties in snow accumulations are present. We do not claim that this method is the way to improve snow modelling in all the world's regions, that would require further experiments. We do, however, both claim and demonstrate that assimilating ICESat-2 snow depth holds considerable promise and is worthy of continued research.

Following the Reviewer's suggestion, we added some considerations on the limitations of this work in the discussion.

Changes:

(line 476, in section 5.1)
As a recommendation, we add that it would be ideal to add more ICESat-2 observations later in the water year, to force the melting season better if that is available. Moreover, experimenting with different temporal and spatial distribution of the assimilated observations as well as with conditions of shallow or deep snow could clarify even more the utility of ICESat-2 in the context of seasonal snow modelling.
... (line 513, in section 5.2)
Moreover, the proposed method relies on a spatially correlated prior to propagate the sparse observations. This step requires the inversion of a squared matrix as large as the number of the simulated cells. Being this is a computationally expensive process, further research is recommended as approximations might be needed to extend the proposed methods to large basins. In addition, larger domains could be split to diminish the size of the matrix to invert and make this process feasible..

Moreover, we added that further experiments and methodological developement is needed in a last paragraph in the abstract.

Changes:

This study makes use of globally-available datasets and shows the promise of adopting ICESat-2's snow depth retrievals in seasonal snow modelling, especially when also assimilating complementary observations. In light of our encouraging results, more research with different experimental designs in varying snow conditions combined with continued methodological development is desirable to further catalyse the use of these retrievals in cryospheric and hydrologic applications.