Reviewer 1
Recommendation Major Revisions:
The authors have put forth an interesting study that aims to quantify the impact of assimilating airborne gamma SWE measurements into the Noah-MP land surface model. This study focuses on forested regions in the Northeast United States, a region that is underrepresented in snow-hydrology research. Furthermore, the choice to assimilate airborne SWE measured from gamma radiation into Noah-MP and quantify the impacts is somewhat novel. The use the LIS, airborne gamma measurements and UA datasets is appropriate, and the paper is generally well structured. Finally, this topic of study is interesting, and the results are of clear value to both the research community and regional stakeholders. Accordingly, this study fits well within the journal scope and is worthy of publication. However, I see two major deficiencies within the work that need to be addressed prior to publication.
[Answer] Thank you for your constructive feedback and the valuable comments on our manuscript. We have carefully revised our manuscript based on each of your comments.

First, I do not see how any reasonable person would be able to replicate this work based on the information provided in the methods section and the data availability statement. I think this needs to be addressed, especially considering that many journals are moving towards an increased emphasis on replicability and open data practices. In my view, significant effort is required to modify the method section, and more data should be made available (if not large model output datasets, at least model config files and namelists) before this study could be considered open and transparent.
[Answer] We appreciate the reviewer for pointing the limited data-availability statement. Based on the Reviewer's comment, we will provide all lis.config files used to run the simulations in supplemental materials. And original and reformatted airborne gamma SWE data (as well as the R code used to reformat them) as well as time series outputs from the OL and DA runs will be available for download at [will add a link to data from Zenodo, currently being setup with an ODC Attribution (ODC-BY) license for access without restrictions].

The second major deficiency is that the open-loop (OL) Noah-MP simulation performs so poorly that I'm left wondering if the model was configured or forced properly. This feeling is exacerbated by the fact that the authors provide very little information regarding the model configuration. Further, the authors make several vague and dismissive statements pinning the poor performance of the model on "model physics" with no supporting evidence. Considering that there are now dozens of studies that show that Noah-MP has rather good performance with respect to snow (including over the Northeast), the exceedingly poor model performance in this study is an outlier, and should be addressed. I suspect that the poor model performance is related to the model configuration chosen for this specific study rather than issues intrinsic to the model. So, prior to publication, the authors should revisit their baseline OL model configuration and track down some of the causes for the poor model performance. My suspicion is that in doing so, they will be able to attain much better accuracy within the OL simulation, and accordingly, better and more robust results regarding the impact of the SWE DA. Once these two issues are addressed, I think the study will easily meet the criteria for publication and make a wonderful additional to the snow-hydrology literature.
[Answer] We very much appreciate the reviewer's comment. To address the Reviewer's comment, we conducted additional experiments using multiple different parameterization schemes, which were

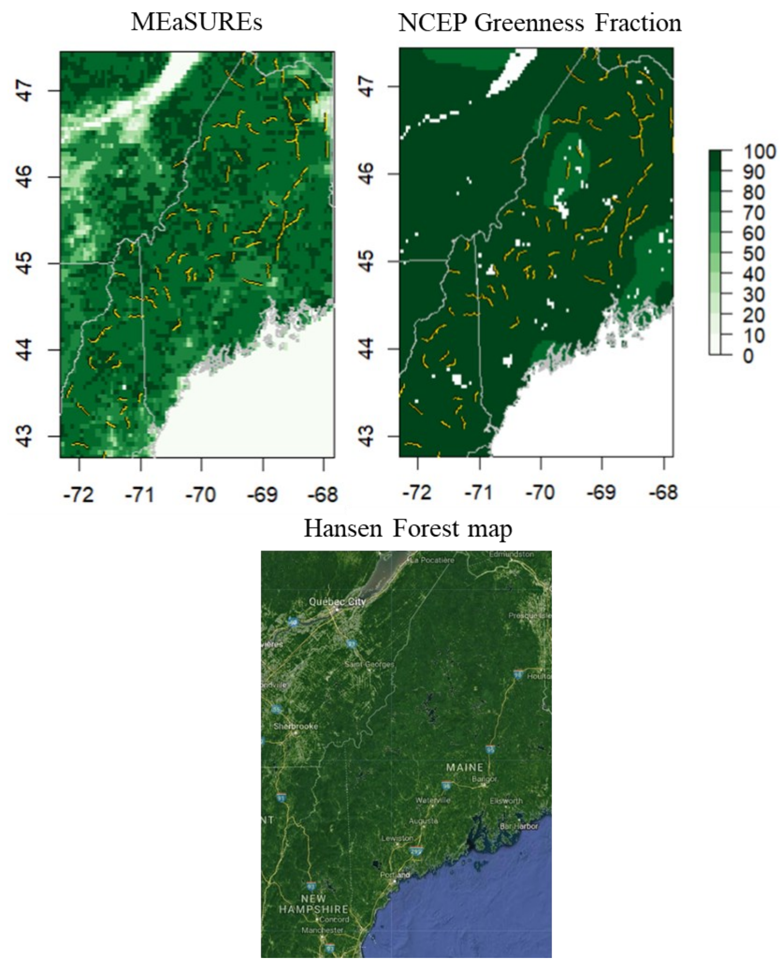
included in the revised manuscript. Please see our detailed answer to your comment for "Lines 227-229" below.

Major Specific Comments:

Line 175 – 190: This section could be improved with additional information regarding the Noah-MP configuration. There are several specific questions that I have regarding the model set up that may go towards explaining some of the results in the results section, particularly with regards to the melt season:

1) Was the TCF described in section 3.4 used to inform the Noah-MP vegetation fraction? And if not, was the Noah-MP vegetation fraction used in the model compared to the TCF for consistency?
Thank you for your comment. We cannot directly compare between the MEaSUREs TCF map used in this study and the vegetation fraction maps (NCEP greenness fraction), because the MEaSUREs data is annual-based historical VCF data while Noah-MP model in this study used NCEP monthly greenness fraction climatology. The main reason why we used MEaSUREs TCF maps in this work is that the maps provide "annual" vegetation fraction cover from 1982 to 2016, enabling us to obtain VCF values at the year when the historical gamma SWE was collected.

To address the Reviewer's question, the spatial maps between the MEaSUREs map in 2016 (left) and NCEP max greenness fraction map (right) are provided below. As an additional source, we also provided UMD Hansen forest map.



2) Was forcing data downscaled to the terrain at all?

Yes. The MERRA-2 atmospheric forcing was spatially and temporally interpolated using bilinear and linear interpolation algorithms, respectively, within LIS before being fed to the LSM.

3) Similarly, was Noah-MP run on a grid covering a region? Or ONLY for "grid-cells" that would have assimilated data, i.e., along the flight-path + gridcells located within the localization radius?

Noah-MP was run on the whole study domain (i.e., northeastern United States). However, the analysis (i.e., performance comparison between the OL and DA cases) was conducted only for grid cells within the flight lines and within the localization radius for the DA experiments without and with applying the localization approach, respectively.

4) What were the physics parameterizations? It's mentioned later on in the paper that the Jordan phase partitioning is used, but equally as relevant would be the snow albedo option (BATS vs. CLASS), the melt-factor used in determining the pixel snow-cover fraction, the radiation_scheme option (specifically

whether or not Fveg is used) and the temp_time_scheme option. (Cuntz et al, 2016 and You et al. 2022 illustrate the impact of some of these options on snow).

We appreciate the reviewer for pointing this out and providing great references. Physical parameterization scheme options of Noah-MP v3.6 used in the current study are listed below: (1) dynamic vegetation for the vegetation option; (2) Noah-type soil moisture factor for stomatal resistance (Chen and Dudhia, 2001); (3) Ball-Berry canopy stomatal resistance scheme (Ball et al., 1987); (4) TOPMODEL-based runoff scheme; (5) simple groundwater scheme (SIMGM; Niu et al., 2007); (6) general Monin-Obukhov similarity theory (M-O; Brutsaert, 1982) for surface layer drag coefficient; (7) NY06 scheme (Niu and Yang, 2006) for supercooled liquid water (or ice fraction) in frozen soil; (8) NY06 scheme (Niu and Yang, 2006) for frozen soil permeability; (9) modified two-stream radiation transfer scheme (Yang and Friedl, 2003, Niu and Yang, 2004); (10) Biosphere-Atmosphere Transfer Scheme (BATS) for ground surface albedo (Yang and Dickinson, 1996); (11) Jordan91 scheme (Jordan, 1991) for partitioning precipitation into rainfall and snowfall; (12) original Noah scheme for lower boundary condition of soil temperature; and (13) semi-implicit snow and soil temperature time scheme. We included these in the revised manuscript in Section 4.1. Regarding the effects of different parameterization schemes on the results are further discussed in the later part of this document.

"Physical parameterization scheme options used in the current study are listed below: (1) dynamic vegetation for the vegetation option; (2) Noah-type soil moisture factor for stomatal resistance (Chen and Dudhia, 2001); (3) Ball-Berry canopy stomatal resistance scheme (Ball et al., 1987); (4) TOPMODEL-based runoff scheme; (5) simple groundwater scheme (SIMGM; Niu et al., 2007); (6) general Monin-Obukhov similarity theory (M-O; Brutsaert, 1982) for surface layer drag coefficient; (7) NY06 scheme (Niu and Yang, 2006) for supercooled liquid water (or ice fraction) in frozen soil; (8) NY06 scheme (Niu and Yang, 2006) for frozen soil permeability; (9) modified two-stream radiation transfer scheme (Yang and Friedl, 2003, Niu and Yang, 2004); (10) Biosphere-Atmosphere Transfer Scheme (BATS) for ground surface albedo (Yang and Dickinson, 1996); (11) Jordan91 scheme (Jordan, 1991) for partitioning precipitation into rainfall and snowfall; (12) original Noah scheme for lower boundary condition of soil temperature; and (13) semi-implicit snow and soil temperature time scheme."

5) What went into the ensemble members? Presumably, this was an ensemble with perturbed initial / forcing conditions? Or was it a physics ensemble? Towards this end, the data-availability statement of "To replicate the land surface model simulation and data assimilation, users can freely access …" is entirely insufficient for replicating the model simulation. At a *bare minimum*, any/all lis.config files used to run the simulations should be included somewhere as supplementary material. Further, was this in off the-shelf version of LIS? Or one that was specifically modified to assimilate airborne gamma snow observations?

Model uncertainty was implicitly represented by the ensemble spread (ensemble size of 20 was used in this study), which was generated by perturbing atmospheric forcing fields and model prognostic state variables with the assumption of a Gaussian distribution. Perturbation parameters applied during the OL and DA runs are summarized in the following table, which was included in the revised manuscript.

Table 1. Perturbation parameters applied to model prognostic state variables and atmospheric forcing fields during the OL and DA runs.

Variable	Perturbation Types	Std dev	AR(1)	Cross correlations		
Model prognostic state variables				*SWE*	*SD*	
SWE	M	0.01	3 hr	–	0.9	
Snow depth (*SD*)	M	0.02	3 hr	0.9	–	
Atmospheric forcing fields				*SW*	*LW*	*P*
Shortwave radiation (*SW*)	M	0.3	1 day	–	-0.5	-0.8
Longwave radiation (*LW*)	A	50 W m^{-2}	1 day	-0.5	–	0.5
Precipitation (*P*)	M	0.5	1 day	-0.8	0.5	–

M: multiplicative; A: additive; AR(1): first-order autoregressive temporal correlation.

As we mentioned above, we will provide all lis.config files used to run the simulations as supplemental materials. And original and reformatted airborne gamma SWE data as well as time series outputs from the OL and DA runs are available used in this paper are available for download at [will add a link to data from Zenodo, currently being setup with an ODC Attribution (ODC-BY) license for access without restrictions].

We used the publicly released version of LIS. The file format of the gamma-SWE observations was converted to one that the original LIS code can read.

Lines 227 – 229: "*Figure 2 was a consequence of the fact that the overestimated SWE during the accumulation season and early in the melt season was offset by the underestimated SWE during the snowmelt season (i.e., April and May)*"
The statement appears generally true looking at the time-series data, but that raises more pressing questions about the OL config. To me it looks like the OL model config has serious issues that cancel each other out in the bulk sense and leads a reasonable overall bias, were an assimilation of SWE can correct one of those issues, leading to worse results. Regardless, the fact that the OL simulation often shows a peak SWE over-estimate greater than 100mm (50- 80% of observed) *and still* melts out 2-4 weeks before the observed melt-out date is a red flag. In particular, the time-series for WY 1989 and WY 1997 are concerning. I *strongly recommend* that the authors a) contextualize the poor performance of the OL simulation with other recent studies evaluating Noah-MP snow in this region (e.g., Letcher et al. 2022 or Sthapit et al. 2022), and/or b) attempt to track down and correct the source of the poor performance. My experience is that the Noah-MP performance is strongly tied to the quality of the forcing data, so I recommend at least doing some analysis comparing the MERRA forcing to in-situ data in the region. Really, any efforts to try and understand why the OL model is performing so poorly would improve the manuscript quality. My feeling is that, if Noah-MP is configured well and driven with good forcing data, the OL performance would be almost certainly be much better than this paper suggests. If the OL simulation is configured for good performance, the impact of the DA will be more accurate and robust, so it is well worth the effort to try and improve the baseline simulation.

As the reviewer commented, the performance of the Noah-MP open-loop (OL) run can be affected by model parameters, parameterization schemes, and atmospheric forcing. One of the references suggested by the reviewer in the comment #4 (i.e., Cuntz et al., 2016) provide sensitivity analysis of the Noah-MP parameters including both the adjustable and hard-coded parameters that affect simulations of hydrological processes. Based on their analysis, some snow-related Noah-MP hydrological simulations exhibit high sensitivity to hard-coded parameters rather than tunable parameters. For example, snowmelt-induced surface runoff is sensitive to hard-coded snow-related parameters for surface resistance, partitioning of incoming radiation into direct and diffuse radiation, and snow thermal conductivity. Furthermore, while model parameter calibration is an important procedure for regional applications, parameter values empirically determined for certain areas are in general not applicable to other areas, and recalibration requires extensive datasets and efforts. As the parameter calibration is outside the scope of our current study, we decided to keep using the default parameter values suggested by the Noah-MP developer. We acknowledged this limitation in the revised manuscript. Consequently, here we only conducted additionally experiments using different parameterization schemes and bias-corrected MERRA-2 forcing.

"While the model parameter calibration is not conducted here because it is outside the scope of the current study, we acknowledge that the parameter calibration procedure could further improve the model performance for regional applications. Cuntz et al. (2016) provided sensitivity analysis of the Noah-MP parameters including both the adjustable and hard-coded parameters that affect simulations of hydrological processes. Based on their analysis, some snow-related Noah-MP hydrological simulations exhibit high sensitivity to hard-coded parameters rather than tunable parameters. For example, snowmelt-induced surface runoff is sensitive to hard-coded snow-related parameters for surface resistance, partitioning of incoming radiation into direct and diffuse radiation, and snow thermal conductivity."

Based on You et al. (2020)'s analysis, we tested four parameterization schemes that possibly affect the performance of the Noah-MP OL run. That is, the BATS scheme for partitioning precipitation into rainfall and snowfall, CLASS scheme for ground surface albedo, Chen97 scheme for surface layer drag coefficient, and fully-implicit snow and soil temperature time scheme were additionally tested. As shown in the figure below, the use of BATS or CLASS schemes did not make a significant difference in SWE estimates as compared to our original OL results. Although the Chen97 or fully-implicit schemes were effective in delaying the snow removal date, they added considerably more snow during the snow accumulation period and did not help capture snowmelt start date. Furthermore, the effectiveness of each parameterization scheme varied with flight lines and time periods within the study domain as also emphasized by You et al. (2020).

The figure also shows that the use of bias-corrected MERRA-2 forcing was effective in improving the SWE estimates during the snow accumulation period, but it still has the issue of rapid snow melting. The combinational use of the bias-corrected MERRA-2 forcing and fully-implicit scheme led to improved snow removal timing, but largely overestimated SWE during the accumulation period. Here, it is worth to note that assimilation of the gamma SWE data provides similar SWE estimates to the case of using the bias-corrected forcing with semi-implicit scheme when the gamma SWE observations are available during the snow accumulation period.

We also tested DA experiments using the bias-corrected forcing and a parameterization scheme "semi- vs. fully-implicit" of snow and soil moisture temperature time scheme which was effective in delaying the melt out date. The DA figure below shows examples that DA performances. In 1989, the DA output using the bias-corrected forcing with fully-implicit (grapy line) captured better the peak SWE and melt out date than the original DA using original MERRA2 forcing with semi-implicit (red line). However, the new DA output overestimated SWE (peak value as well) and estimated later melt out date in 1991.

Based on these results, we tone down our previous discussion and included further analysis and related sentences in the revised manuscript. However, we decided not to reconduct all DA experiments using the bias-corrected forcing and different parameterization schemes due to the following reasons. First, the bias-corrected forcing is not readily available for operational use. Our fundamental purpose is to apply the gamma SWE DA framework in operational systems. Thus, this study focuses on demonstrating the efficacy of the gamma SWE DA in improving the SWE estimates of a model driven by any atmospheric forcing as is. Second, the effectiveness of using different parameterization schemes is not constant for different fight lines and time periods within the study domain.

However, we definitely agree with the reviewer's comment that the proper model configuration and accurate forcing may result in more robust conclusions. We acknowledge the limitation of our study and provided more comprehensive discussion on the limitation in the revised manuscript as below.

"These issues can be attributed to parameterization schemes and/or atmospheric forcing employed in Noah-MP. Parameterization options for the precipitation phase partitioning method, ground surface albedo, surface layer drag coefficient, and snow/soil temperature time scheme can affect the snow simulations (You et al., 2020). To further analyze the issues, we conducted additional experiments using different parameterization schemes and atmospheric forcing. That is, the BATS scheme for partitioning precipitation into rainfall and snowfall, CLASS scheme for ground surface albedo, Chen97 scheme for surface layer drag coefficient, fully-implicit snow and soil temperature time scheme, and the bias-corrected MERRA-2 forcing were additionally tested. As shown in ***Figure 8a****, the use of BATS or CLASS schemes do not make a significant difference in the SWE estimates as compared to the original OL results. Although the Chen97 or fully-implicit schemes are effective in delaying the snow removal date, they add considerably more snow during the snow accumulation period and do not help capture snowmelt start date (****Figure 8a****). Furthermore, the effectiveness of each parameterization scheme varies with flight lines and time periods within the study domain as also emphasized by You et al. (2020).* ***Figure 8b*** *shows that the use of the bias-corrected MERRA-2 forcing is effective in improving the SWE estimates during the snow accumulation period, but it still has the issue of rapid snow melting. The combinational use of the bias-corrected MERRA-2 forcing and fully-implicit scheme leads to improved snow removal timing, but largely overestimated SWE during the snow accumulation period. We originally used the uncorrected MERRA-2 forcing to demonstrate the feasibility of the gamma SWE DA for improving the model estimates of SWE, particularly in forested areas, using the atmospheric forcing as is (i.e., without bias-correction), which is a typical case of operational prediction or monitoring systems. Here, it is worth to note that assimilation of the gamma SWE data provides similar SWE estimates to the case of using the bias-corrected forcing with semi-implicit scheme when the gamma SWE observations are available during the snow accumulation period.*

Many studies (e.g., Aoki et al., 2011; Augas et al., 2020; Cheng et al., 2008; Jennings et al., 2018; Kwon et al., 2014; Lecomte et al., 2011; Livneh et al., 2010; Saha et al., 2017; Suzuki and Zupanski, 2018) have emphasized the importance of the number of snow layers for accurate estimates of snowmelt timing because of its impact on the vertical snow temperature gradient. Augas et al. (2020) demonstrated that the accuracy of the SWE estimates increases with more snow layers, and Lecomte et al. (2011) showed *that the agreement between the observed and modelled vertical snow temperature gradient are improved by adding more snow layers. The minimum threshold of the number of snow layers suggested by existing studies ranges from 3 to 20 depending on locations, time periods, and model setup. Different precipitation partitioning methods may lead to differences in the amount of snowfall and subsequent snowpack (Xia et al., 2017; Jennings et al., 2018; Suzuki and Zupanski, 2018; Letcher et al., 2021), even though there were no significant differences in SWE between the two schemes. We used the scheme of Jordan (1991), in which total precipitation is fractionally divided into rainfall and snowfall using two thresholds of air temperature (i.e., no snowfall when Tair > 2.5℃; all precipitation is snow when Tair ≤ 0℃; and fractional snowfall when 0℃ < Tair ≤ 2.5℃). However, Noah-MP uses a spatially uniform threshold of Tair. Jennings et al. (2018) found that rain-snow Tair thresholds exhibited significant spatial variability across the Northern Hemisphere with the warmest thresholds in continental and mountain areas while with the coolest thresholds in maritime areas and lowlands. This implies that the high Tair threshold (i.e., 2.5℃) used in Noah-MP may lead to the overestimated snowfall, and subsequently the overestimated snow depth and SWE as the study area is characterized by maritime. Letcher et al. (2021) demonstrated that the use of cooler Tair thresholds in Noah-MP can improve the estimates of peak SWE in the northeastern United States. Further improvement in the modeled SWE during the melting season can be achieved by employing more sophisticated snow models since the sophisticated snow models with multi-layer of snowpack takes into account meltwater infiltration and refreezing within the snowpack (Avanzi et al., 2016; Terzago et al., 2020).”*

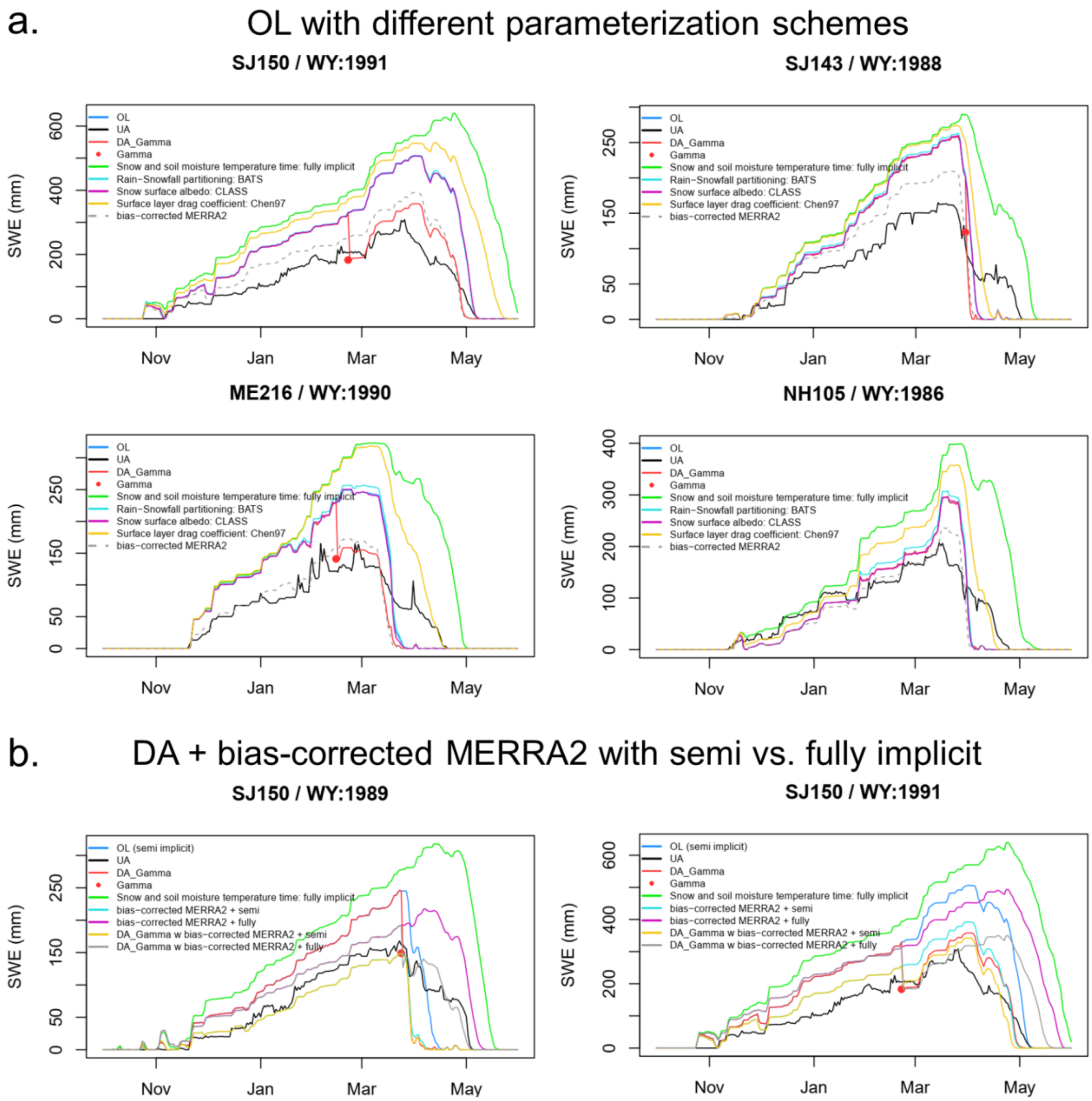


Figure 8. Examples of SWE time series including (a) the additional open-loop (OL) experiments using different parameterization schemes related to snow simulations such as the snow/soil temperature time scheme (semi-implicit vs. fully-implicit), partitioning precipitation phase (Jordan91 vs. BATS), ground surface albedo (BATS vs. CLASS), and surface layer drag coefficient (Monin-Obukhov [M-O] similarity theory vs. Chen97 [original Noah]), and (b) DA runs forced by original vs. bias-corrected MERRA2 forcings with each snow/soil temperature time scheme which is a parameterization option largely affecting snow simulations**.**

Line 243 – 248: This whole section needs additional analysis. The conclusions presented by the authors seem to make no effort in gathering evidence to support them. Rather, speculative comments like "probably due to limited model physics" or "may be attributed to model structure physics" are used to explain why the DA didn't help much during the spring. I don't think these types of statements really

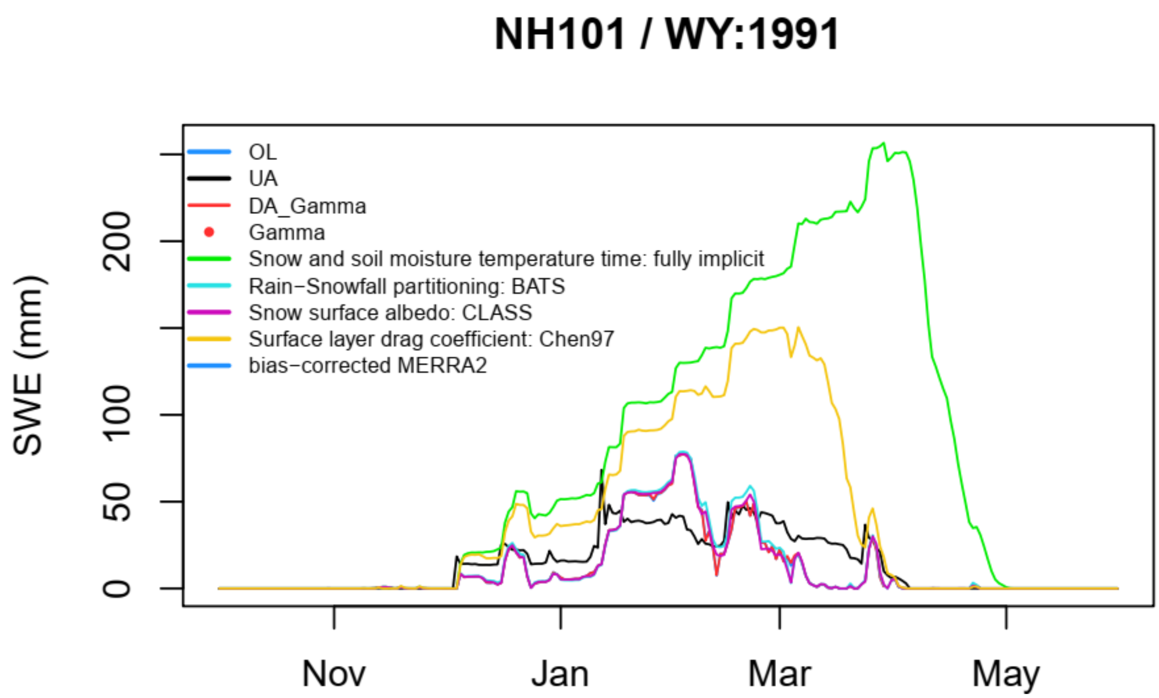
belong in a results section, especially when there are ample analyses that could have been done to support or refute them. Considering that there are several studies showing fairly decent Noah-MP performance, I don't think the poor performance found here is attributable to intrinsic issues with the model architecture. **"**However, the assimilation of the airborne gamma SWE measurements was not able to improve the snow ablation timing probably due to temporally sparse gamma data as well as limited model physics"
I disagree with this statement; I and I see very little evidence in the paper supporting it. For instance, WY2015 seems to have a gamma-observation during the ablation season, and this does not improve the simulation at all.

We agree with the reviewer's comment. As mentioned in the previous comment, we conducted additionally experiments using different parameterization schemes and bias-corrected MERRA-2 forcing. Based on the results, we revised the sentences as follows in the revised manuscript as follows:

"As shown in Figure 4, compared to the UA SWE, Noah-MP simulated earlier snow melt-out despite the overestimated snow accumulation, which may be attributed to the Noah-MP model structure and physics (e.g., simplified representations of snow layers), parameterization schemes, and/or atmospheric forcing. Discussions on this are provided in section 6. Also, the peak SWE cannot be corrected if a single gamma SWE exists only after the accumulation period. The availability of more frequent gamma observations during both the accumulation and snowmelt melt seasons could lead to further improvements in estimating SWE in the ablation period while the Noah-MP model snow layer representation and forcings needs to be enhanced."

General comment: Have the authors considered other metrics for quantifying snow improvement such as peak SWE amount and timing? Or melt out date? RMSE can be a tricky metric to quantify snow, since snow has a lower bound of zero (i.e., a lot of data points will simply be comparing zeros to each-other). See Rhodes et al. (2018) and Trujillo and Molotch (2014).

Thank you for the comment. We had considered the metrics such as the amount of peak SWE and timing and melt out date. However, we concluded that providing time series graph itself would be better to compare snow patterns between OL and DL and quantify snow improvement because unlike typical mountain snow regimes (based on the given references), ephemeral snow characteristics were frequently found in those regions, and those characteristics generated multiple peak SWE and melt on dates (an example time series for NH101 line is provided below). In addition to providing time series, we calculated RMSD, slope, and bias to quantify overall SWE performance as period-averaged quantifications (The time series' figures for all gamma lines will be provided in Supporting Information).



References:

Cuntz, M., Mai, J., Samaniego, L., Clark, M., Wulfmeyer, V., Branch, O., Attinger, S. and Thober, S., 2016. The impact of standard and hard-coded parameters on the hydrologic fluxes in the Noah-MP land surface model. Journal of Geophysical Research: Atmospheres, 121(18), pp.10-676.

Rhoades, A.M., Jones, A.D. and Ullrich, P.A., 2018. Assessing mountains as natural reservoirs with a multimetric framework. Earth's Future, 6(9), pp.1221-1241.

Sthapit, E., Lakhankar, T., Hughes, M., Khanbilvardi, R., Cifelli, R., Mahoney, K., Currier, W.R.,Viterbo, F. and Rafieeinasab, A., 2022. Evaluation of Snow and Streamflows Using Noah-MP and WRF-Hydro Models in Aroostook River Basin, Maine. Water, 14(14), p.2145.

Letcher, T.W., Minder, J.R. and Naple, P., 2022. Understanding and improving snow processes in Noah-MP over the Northeast United States via the New York State Mesonet.

Trujillo, E. and Molotch, N.P., 2014. Snowpack regimes of the western United States. Water Resources Research, 50(7), pp.5611-5623.

You, Y., Huang, C., Yang, Z., Zhang, Y., Bai, Y. and Gu, J., 2020. Assessing Noah-MP parameterization sensitivity and uncertainty interval across snow climates. Journal of Geophysical Research: Atmospheres, 125(4), p.e2019JD030417.

Minor comments:

There are numerous instances of RMSD and a couple of instances of RMSE throughout the paper, so my assumption is that RMSD is preferred. I'm assuming that RMSD is Root Mean Square Difference? Either one is fine, but please be consistent, and consider spelling out RMSD in the paper when it's first used.

We edited and consistently used RMSD throughout the manuscript.

Line 206: Was the model snow LWC content updated proportionally? Or was any SWE added/subtracted through assimilation considered all snow? E.g., if the snowpack was 5% liquid, was SWE added -> LWC= LWC + 0.05*NEW and FROZ = FROZ + 0.95*NEW?

The amount of the SWE update from the assimilation of the gamma SWE retrievals was added only to ice content of the bottom snow layer. Then, snow layer variables such as thickness, snow ice and liquid water content, and SWE of each snow layer were adjusted using the same methods as used in the Noah-MP's snow layer compaction, combination, and subdivision procedures. We added the explanations in the revised manuscript as follows:

"Note that assimilation of gamma SWE updates only modeled SWE and the amount of the SWE update is added to ice content of the bottom snow layer. Then, snow layer variables such as thickness, snow ice and liquid water content, and SWE of each snow layer are adjusted using the same methods as used in the Noah-MP's snow layer compaction, combination, and subdivision procedures."

Line 225 -226: Did the authors mean: "closer to" instead of "closed to?" Since the authors include the median slope of the linear regression for the OL, would it make sense to also include it from the DA run?

Yes. We edited this sentence as below.
"The values of 1:1 slope were closer to 1 (A median slope of OL and DL were 1.45 and 0.91, respectively) and RMSD values decreased, even though negative biases were found."

Lines 226-227: Was Figure 2 only for grid-cells that received DA updates? OR ALL gridcells (see previous comment on model config)?
As we mentioned above, Noah-MP was run on the whole study domain (i.e., northeastern United States). However, the performance comparison between the OL and DA cases was conducted only for grid cells within the flight lines and within the localization radius for the DA experiments without and with applying the localization approach, respectively. Figure 2 was based on grid-cells that received DA updates.

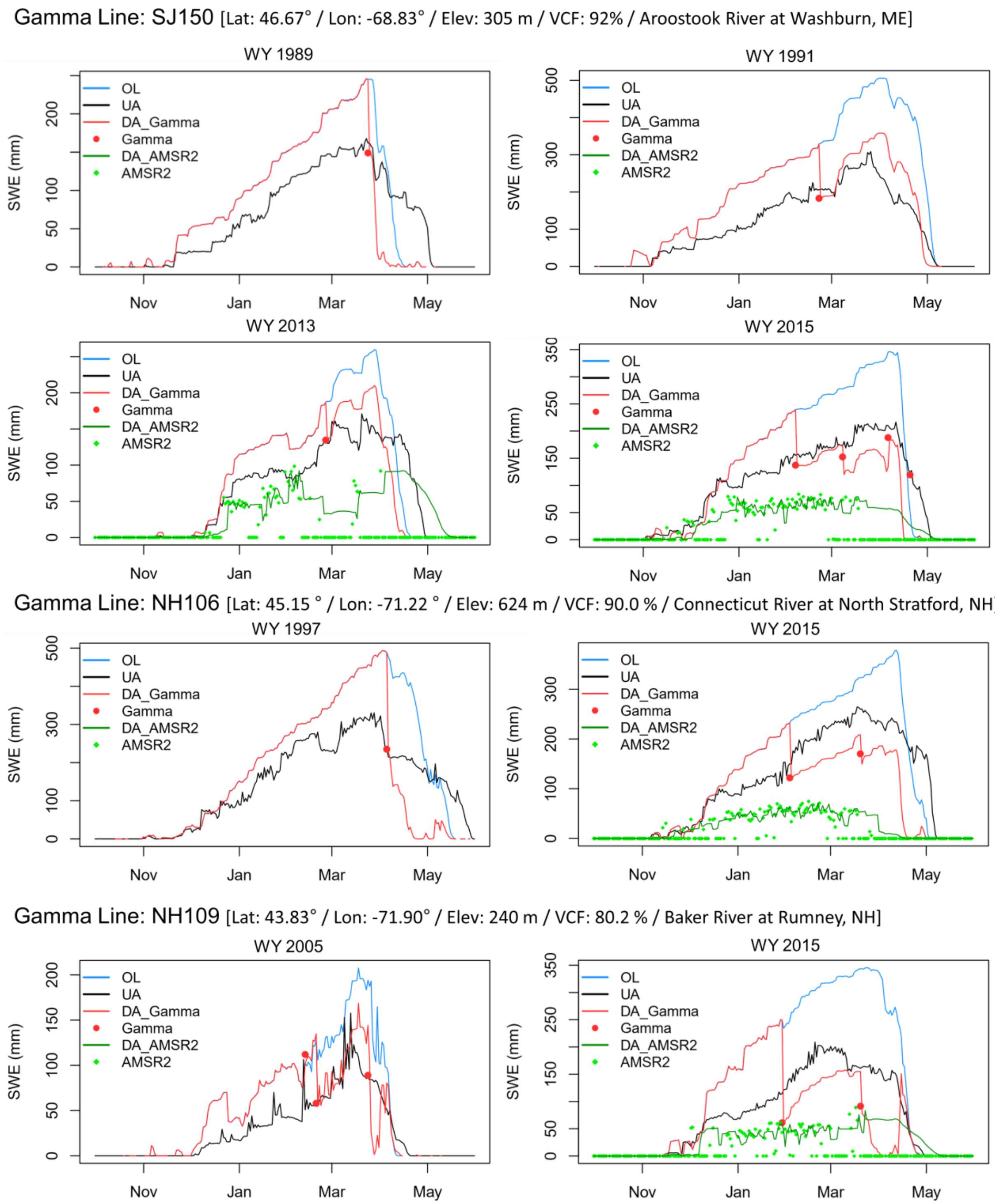
Lines 226-227: I recommend rewording: "The lower bias of the SWE estimates from the OL as compared to the DA in Figure 2 …" to be more specific: "The absolute SWE bias was higher in the DA simulation compared to the OL simulation (Figure 2b). However, this was a consequence of the fact …."
Thank you for the recommendation. Based on the Reviewer's suggestion, we reworded the statement as below.
*"The absolute SWE bias was higher in the DA as compared to the OL simulation (**Figure 2**). However, this was a consequence of the fact that the overestimated SWE during the accumulation season and early in the melt season was offset by the underestimated SWE during the snowmelt season (i.e., April and May)."*
Line 237: Please replace the word "enhanced" with "improved"
Edited.

Line 240: Recommend deleting "As shown in the figure"
Deleted.

Section 5.2 throughout:
I recommend replacing references to the “lower” and “higher” groups with “low” and “high” since this is how the groups were introduced.
Thank you for the recommendation. We edited these.

Line 252: What is “differences in the 1:1 slope?” is it differences in the linear-regression slope between the OL and DA simulations? Or is it differences between the linear-regression slope AND the 1-to-1 line for each simulation? Please clarify.
This means differences in linear regression slope for each simulation (OL and DA) with UA SWE.

Figure 4: I suggest increasing the font size on the legend to be more readable.
Thank you for the suggestion. We increased the font size. Please see the attached.



Line 253 – 255: Consider rewording: "In the figure, two groups of each land surface characteristics were determined by dividing the gamma flight lines into two (i.e.,low and high) groups of equal numbers of the flight lines."

As: "In this analysis, the land-characteristics sampled by the gamma flights were divided equally into two groups (i.e., low and high) such that the land characteristic value separating the two groups allowed for equal numbers of samples in each group."

Thank you so much for the suggested words, which are much clear to convey the meaning. We edited as suggested.

Line 260: what does "updated" mean here? Improved? Changed? Please clarify.
Improved.

Line 264: "added value of the gamma SWE data on the model SWE estimates via assimilation"
This is really awkward wording, please rephrase.
We rephrased this as below.
"For individual physical characteristics, the improvement of the model SWE estimates via the gamma SWE assimilation are greater for the low VCF range based on both bias and RMSD."

Figure 6: I'm a little confused how the mean slope for the 4km localized OL simulation is near 1-to-1 as compared to 1.8/1.9 shown in figure 3. I'm also confused as to why there is an increase in the error metrics in the OL simulation as the localization distance is increased? Does that imply that the model is better over the flight-lines, even without any DA? Am I just interpreting this analysis wrong? Is it just that the 4km localization provides fewer grid points, and therefore fewer opportunities for the model to accumulate large errors? Also, if the OL slope for 4km is close to one, how does that correspond to such high (50-60mm) bias? I don't think the y-intercept would explain that.
My understanding is that the model is running Noah-MP along some flight-path, and some number of adjacent gridcells corresponding to various different radii of influence. Shouldn't the OL metrics be independent of how many gridcells are in the simulation? Either way, the methods here are pretty vague, I recommend more clearly explaining the experiment to eliminate confusion.
Thank you for the valuable comment. When double-checking our calculation for Figure 6, we found a script error when calculating the error metrics. The error metrics were miscalculated using a "static" domain-averaged OL SWE time series with DA SWE time series at different localization distances. They should have been calculated with a domain-averaged OL SWE time series at a "corresponding" localization distance. We reviewed throughout the script, corrected the part, and recalculated the metrics which are shown in the new figure below.

The reason why the OL statistics were changed with increasing the localization distances in Figure 6 is that the effective distance used to calculate the domain-averaged OL SWE was changed according to the localization distance. For example, at a localization distance of 4 km, the domain-averaged OL SWE is calculated using gridded SWE values within an effective surrounding area (4 km distance from the gamma flight lines). At 32 km, the domain-averaged OL SWE was calculated based on larger number of the gridded SWE values within surrounding areas up to 32-km distance from the gamma flight line.

To clearly explain the localization, we edited and added descriptions in Section 4.3.

"To quantify if the spatially sparse gamma SWE observations can improve the SWE estimates in the surrounding areas, where the observations are not available, we apply a distance-based localization method into the assimilation procedure. The localization is applied in the assimilation by weighting

distances from the flight lines (up to a specified localization distance; r) using the Gaussian decay-based localization method as follows:

$$W = exp\left\{\frac{-d^2}{2\cdot\left(\frac{r}{2}\right)^2}\right\} \quad (7)$$

where d is the distance between the updated grid cells (i.e., flight lines) and grid cells without observations within a specified localization radius r. The degree of the SWE updates for a grid cell from the assimilation is calculated using the localization weight (W) which is calculated based on the distance (d) from the updated grid cells overlapped with the flight line."

The result parts of the manuscript related to the revised figure 6 were also revised as below.

"For the whole snow season that includes both accumulation and melting periods, the boxplot of the 1:1 slope shows that the localized DA SWE were improved as compared to OL. The slopes of the DA SWE are closer to 1 than the OL's slopes. The bias and RMSD boxplots also show that the DA SWE has lower errors than the OL SWE for all localization distances, expect for bias at 48 km which is too low (median: – 23 mm). The OL's RMSDs slightly increased at the distances up to 16 km (median: 72 mm) and decreased after that, while the DA's RMSD values continually decreased with increasing the distances up to 48 km (median: 53 mm). When the statistics were calculated for the accumulation and melting periods separately, the lower RMSDs and slopes closer to 1 of the localized DA SWE were found consistently. As previously discussed, the efficacy of assimilating the airborne gamma SWE is greater during the accumulation period, especially for bias and RMSD, than during the melting period. In the melting period, the improvements in the bias, RMSD and 1:1 slope are also achieved up to 32 km"

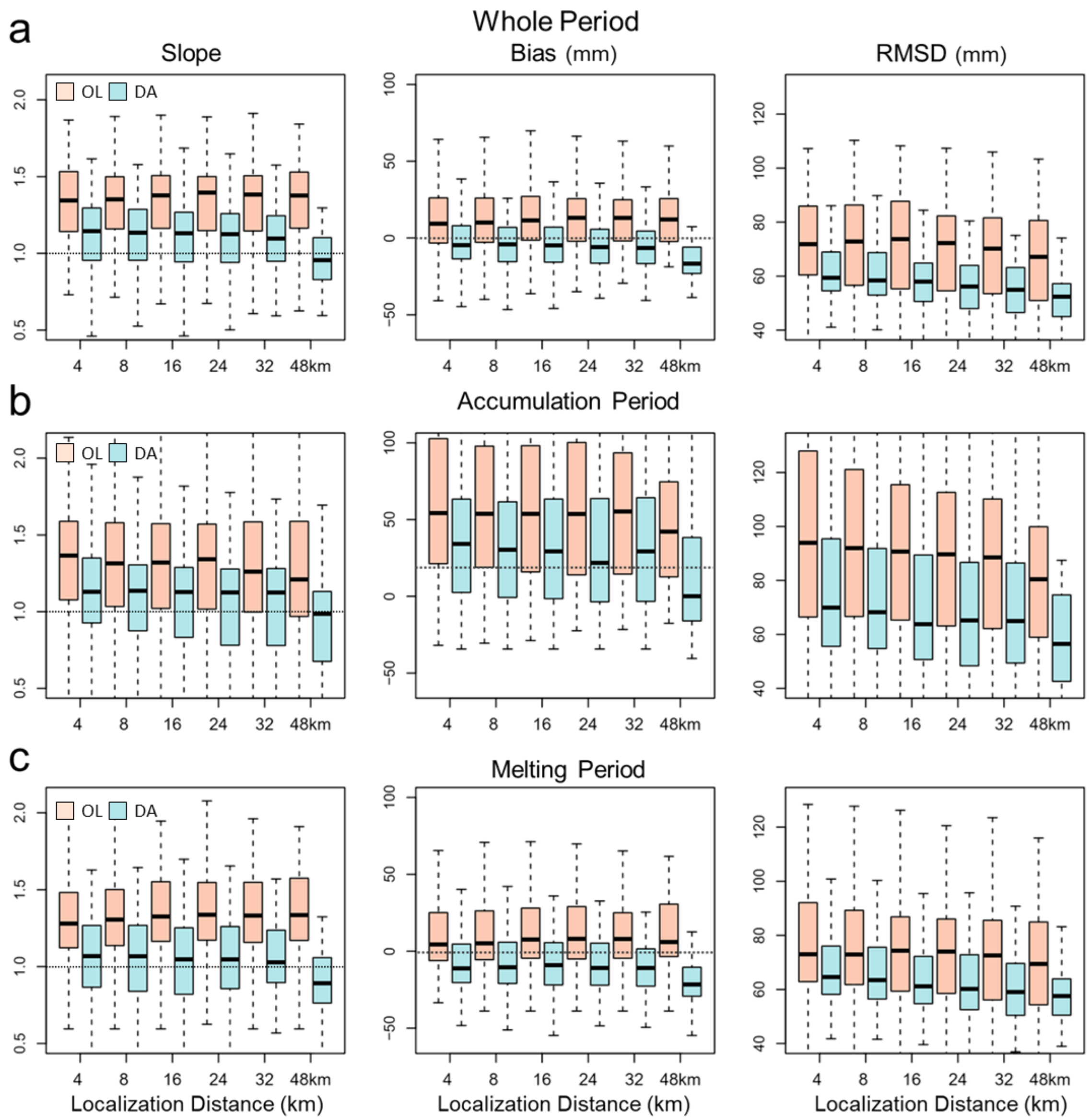


Figure 6. Localized data assimilation (DA) and open-loop (OL) Noah-MP SWE performances as compared to the UA SWE with different localization distances (e.g., 4, 8, 16, 24, 32, and 48 km) for the whole (accumulation and melting periods), accumulation, and melting periods, respectively.

Line 280: "closed" should be "close"

Done.

Line 307-308: "The overestimated SWE during the snow accumulation period was likely attributed to a precipitation phase partitioning method employed in Noah-MP." This could very well be true, but this is something that could be easily tested by running a couple of simulations with different temperature thresholding (e.g., PCP_PARTITION_OPTION = 3, instead of 1 in the namelist). That way the authors wouldn't have to speculate here.

We additionally tested the Noah-MP OL run using different parameterization scheme (i.e., BATS) for partitioning precipitation into rainfall and snowfall. However, the use of the BATS scheme could not improve the SWE estimates during the accumulation period. Our additional experiments using the bias-corrected MERRA-2 forcing show that atmospheric forcing was the main source of the largely overestimated SWE during the snow accumulation period rather than the parameterization scheme. We revised the manuscript based on these additional analysis as already presented in the previous comments.

Lines 322 – 324: "Although the snowpack in Noah-MP can have up to three snow layers, it may not be enough to accurately reproduce the energy budget within the snowpack in the study area."
Do you have a citation to support this statement? While there is probably at least some truth to it, I'm not sure that I agree 100%. In terms of simulating bulk SWE, I don't often see significant improvement with the addition of *more* snow-layers, just so long as the model has multiple discrete snow layers in it.

We do not have references specific to Noah-MP applications, but there are many studies (e.g., Aoki et al., 2011; Augas et al., 2020; Cheng et al., 2008; Jennings et al., 2018; Lecomte et al., 2011; Livneh et al., 2010; Saha et al., 2017) that emphasize the importance of the number of snow layers for accurate estimates of snowmelt timing because of its impact on the vertical snow temperature gradient. Augas et al. (2020) demonstrated that the accuracy of the SWE estimates increases with more snow layers, and Lecomte et al. (2011) showed that the agreement between the observed and modelled vertical snow temperature gradient are improved by adding more snow layers. However, we understand the reviewer's concerns and experiences, and the minimum threshold of the number of snow layers suggested by existing studies ranges from 3 to 20 depending on locations, time periods, and model setup. Therefore, we tone down the sentences and provided more citations in the revised manuscript. The related sentences included in the revised manuscript are presented in the previous comments)

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Saha, S.K., Sujith, K., Pokhrel, S., Chaudhari, H.S., & Hazra, A.: Effects of multilayer snow scheme on the simulation of snow: Offline Noah and coupled with NCEP CFSv2. Journal of Advances in Modeling Earth Systems, 9, 271–290, https://doi.org/10.1002/2016MS000845.

Line 344 – 345: "However, as proven in our findings, effective uses of the gamma SWE (e.g. localization function) will maximize the utility of the gamma SWE into the DA framework." I would scale back the certainty in this statement: e.g., replace "proven in" with "supported by" and "will maximize" with "can enhance."

Thank you for the suggestions. We edited them as suggested.

"However, as supported by our findings, effective uses of the gamma SWE (e.g. localization function) can enhance the utility of the gamma SWE into the DA framework."

Lines 347- 348: "promising alternative"; Alternative to what?

We changed this to "approach"

"DA has been used as a promising approach to improve SWE estimation at a large spatial scale"

Lines 356 – 358: "The added value of the gamma data on the model SWE estimates was greater for the relatively lower VCF range. For areas with higher topographic heterogeneity, the gamma-based DA SWE was still effective in reducing the errors."

This is very awkwardly worded, the first sentence is in reference to the vegetation fraction, and the second was for the topography heterogeneity. Specifically, the use of the phrase "still effective" is confusing since there is no "base-state" effectiveness of the DA for terrain heterogeneity described here in the conclusions. Please reword.

We agreed with the Reviewer's comment. The second sentence was reworded as below.

"While the gamma-based DA SWE had relatively lower improvement in areas with higher topographic heterogeneity, the DA SWE with reduced errors was found as compared to the OL.

Lines 361 – 362: "uncertainties in the Noah-MP physics (i.e., precipitation partitioning and simplified snow layers)." I really do not think that this statement belongs in the conclusions, since it's just speculation on the part of the authors.

We removed this part in the conclusion.

Data Availability:

Definitely a lot to be desired here, by only pointing readers to publicly available datasets, and choosing not to include model configuration data, analysis code, or processed (example) data, there is simply no way that a reasonable person would be able to repeat the experiment and analysis described in the paper. At a bare-minimum, the model configuration files should be included, as should any code that processes the data prior to being fed into the model.

As we answered ablow, we will provide all lis.config files used to run the simulations in supplemental materials. And original and reformatted airborne gamma SWE data (as well as the R code used to reformat them) as well as time series outputs from the OL and DA runs (as much as the size is allowed) will be available for download at [will add a link to data from Zenodo, currently being setup with an ODC Attribution (ODC-BY) license for access without restrictions].

Author Contributions:
I'll leave this to the editors, but I'm not sure "provided the funding" is a good reason to include for co-authorship. While in this instance it doesn't matter since SVK and CMV provided other input that would merit co-authorship, explicitly listing "provided funding" seems like it could be problematic.

Thank you for the comment. We removed the listing "Acquired the funding and the resources" here. As we stated, SVK and CMV helped the formal analysis, particularly localization DA implements in LIS, provided technical and scientific throughout a series of meetings and discussions, and actively reviewed and edited the manuscript.

Reviewer 2

Review of " Assimilation of airborne gamma observations provide utility for snow estimation in forested environments" by Cho et al.

SUMMARY:

Overall, I think this paper is relevant for publication in HESS with a clear presentation. This paper leverages airborne gamma observations to estimate snow water equivalent using data assimilation. This is great work for the snow community that shows the potential of remote-sensed gamma observations to improve snow estimates. That said, clarification of how localization is implemented with DA would be helpful for others to repeat your work and interpolate the results.

Thank you for your positive feedback and the valuable comments on our manuscript. We have carefully revised our manuscript based on your comments.

Line-by-line comments:

1. L125: For Equations (1) to (4), make sure to use the same format for the uncollided gamma count rates (e.g., $40K_b$ $^{40}K_b$).

Thank you for your keen eye. We edited this.

2. L138: From my understanding, COOP snow depth is first converted to SWE which is assimilated (rather than snow depth) to get UA SWE. Please make sure this sentence won't cause any confusion.

We agreed with the comment. The sentence was edited to clarify the data development procedure as below.

"The UA SWE is the ground observation-based 4-km gridded SWE product developed by consistently assimilating the snow telemetry (SNOTEL) SWE and NWS Cooperative Observer Program (COOP) snow depth measurements (which was first converted to SWE using a newly developed snow density parameterization) with the Parameter-elevation Regressions on Independent Slopes Model (PRISM) temperature and precipitation data over the continental United States"

3. L186: For MERRA2, are bias-corrected precipitation or uncorrected precipitation used as inputs? In line 308, the authors mention that overestimated SWE is likely attributed to precipitation phase partition. Would bias from precipitation contribute to the overestimation?

We used the uncorrected MERRA-2 forcing in the original manuscript because we focused on demonstrating the feasibility of the gamma SWE DA to improve the model estimates of SWE, particularly in forested areas, using the atmospheric forcing as is (i.e., without bias-correction), which is a typical case of operational prediction or monitoring systems. However, in order to analyze the effect of the atmospheric forcing on the snow accumulation, we conducted additionally experiments using the bias-corrected MERRA-2 forcing (as well as different parameterization schemes to address another Reviewer's comments). The figure below shows that the use of bias-corrected MERRA-2 forcing was effective in improving the SWE estimates during the snow accumulation period as compared to the case using the original (i.e., uncorrected) MERRA-2 forcing. Here, it is also worth to note that assimilation of the gamma SWE data (using the uncorrected MERRA-2 forcing) provides similar SWE estimates to the case of using the bias-corrected forcing when the gamma SWE observations are available during the snow accumulation period.

We included these analyses in the revised manuscript as follows:

"These issues can be attributed to parameterization schemes and/or atmospheric forcing employed in Noah-MP. Parameterization options for the precipitation phase partitioning method, ground surface albedo, surface layer drag coefficient, and snow/soil temperature time scheme can affect the snow simulations (You et al., 2020). To further analyze the issues, we conducted additional experiments using different parameterization schemes and atmospheric forcing. That is, the BATS scheme for partitioning precipitation into rainfall and snowfall, CLASS scheme for ground surface albedo, Chen97 scheme for surface layer drag coefficient, fully-implicit snow and soil temperature time scheme, and the bias-corrected MERRA-2 forcing were additionally tested. As shown in ***Figure 8a****, the use of BATS or CLASS schemes do not make a significant difference in the SWE estimates as compared to the original OL results. Although the Chen97 or fully-implicit schemes are effective in delaying the snow removal date, they add considerably more snow during the snow accumulation period and do not help capture snowmelt start date* ***(Figure 8a)****. Furthermore, the effectiveness of each parameterization scheme varies with flight lines and time periods within the study domain as also emphasized by You et al. (2020).*

Figure 8b *shows that the use of the bias-corrected MERRA-2 forcing is effective in improving the SWE estimates during the snow accumulation period, but it still has the issue of rapid snow melting. The combinational use of the bias-corrected MERRA-2 forcing and fully-implicit scheme leads to improved snow removal timing, but largely overestimated SWE during the snow accumulation period. We originally used the uncorrected MERRA-2 forcing to demonstrate the feasibility of the gamma SWE DA for improving the model estimates of SWE, particularly in forested areas, using the atmospheric forcing as is (i.e., without bias-correction), which is a typical case of operational prediction or monitoring systems. Here, it is worth to note that assimilation of the gamma SWE data provides similar SWE estimates to the case of using the bias-corrected forcing with semi-implicit scheme when the gamma SWE observations are available during the snow accumulation period."*

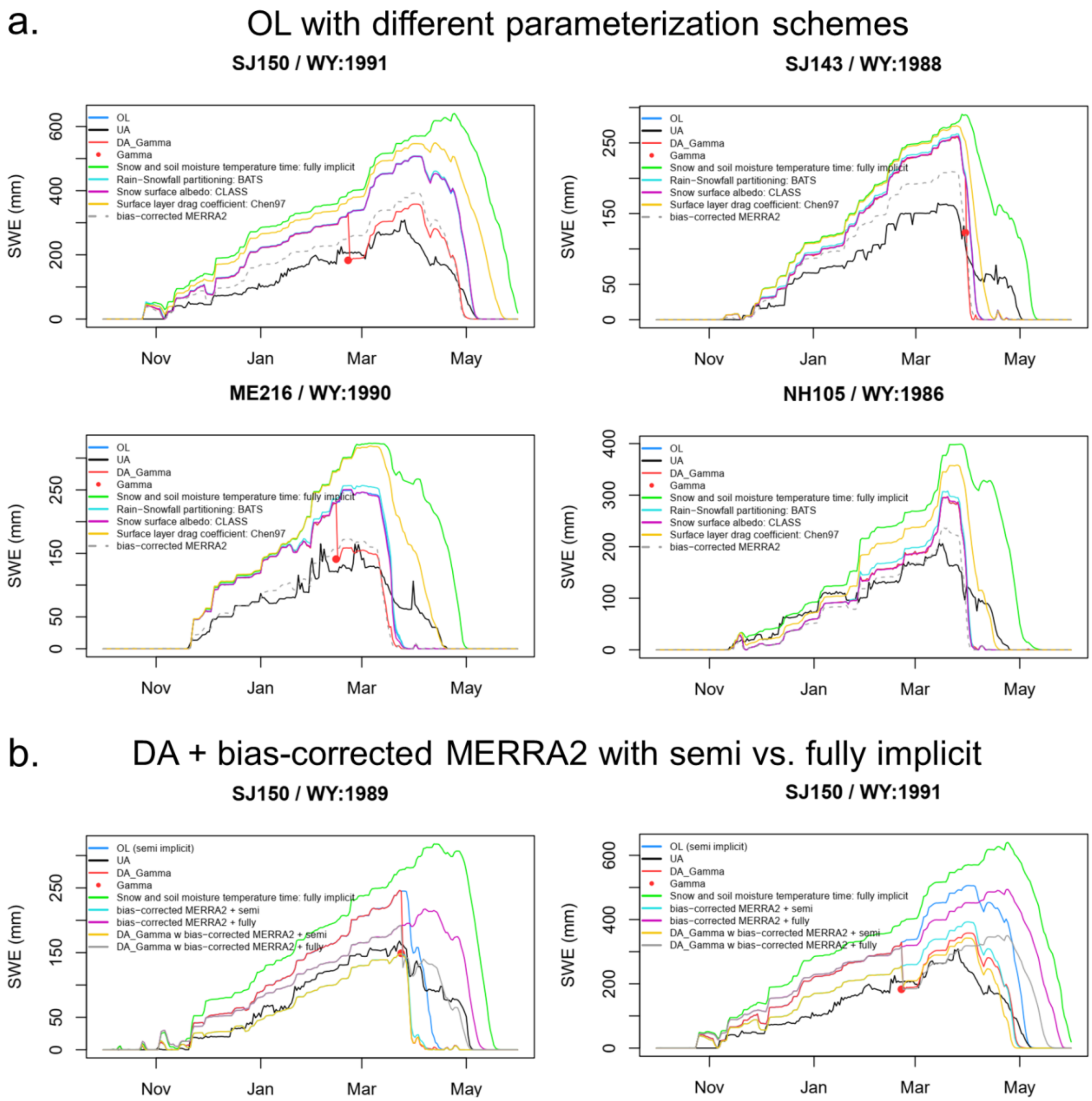

Figure 8. Examples of SWE time series including (a) the additional open-loop (OL) experiments using different parameterization schemes related to snow simulations such as the snow/soil temperature time scheme (semi-implicit vs. fully-implicit), partitioning precipitation phase (Jordan91 vs. BATS), ground surface albedo (BATS vs. CLASS), and surface layer drag coefficient (Monin-Obukhov [M-O] similarity theory vs. Chen97 [original Noah]), and (b) DA runs forced by original vs. bias-corrected MERRA2 forcings with each snow/soil temperature time scheme which is a parameterization option largely affecting snow simulations**.**

4. L196: Could you briefly summarize how Kwon et al. (2021) perturbs the forcings? For example, which forcings are perturbed? How are the parameters chosen? (i.e., Were in situ observations used to

quantify these parameters?) This would be helpful to know if precipitation uncertainties are considered.

Perturbation parameters applied during the OL and DA runs are summarized in the following table, which was included in the revised manuscript. The perturbation parameter values in Kwon et al. (2021) were determined based on previous DA studies (e.g., Forman et al., 2012; Kumar et al., 2009; 2014; 2016; Reichle et al., 2008).

Table 1. Perturbation parameters applied to model prognostic state variables and atmospheric forcing fields during the OL and DA runs.

Variable	Perturbation Types	Std dev	AR(1)	Cross correlations		
Model prognostic state variables				*SWE*	*SD*	
SWE	M	0.01	3 hr	–	0.9	
Snow depth (*SD*)	M	0.02	3 hr	0.9	–	
Atmospheric forcing fields				*SW*	*LW*	*P*
Shortwave radiation (*SW*)	M	0.3	1 day	–	-0.5	-0.8
Longwave radiation (*LW*)	A	50 W m^{-2}	1 day	-0.5	–	0.5
Precipitation (*P*)	M	0.5	1 day	-0.8	0.5	–

M: multiplicative; A: additive; AR(1): first-order autoregressive temporal correlation.

*** References**

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Kumar, S.V., Reichle, R.H., Koster, R.D., Crow, W.T., & Peters-Lidard, C.D.: Role of subsurface physics in the assimilation of surface soil moisture observations. Journal of Hydrometeorology, 10 (6), 1534–1547, https://doi.org/10.1175/2009JHM1134.1, 2009.

Kumar, S.V., Peters-Lidard, C.D., Mocko, D., Reichle, R., Liu, Y., Arsenault, K.R., Xia, Y., Ek, M., Riggs, G., Livneh, B., & Cosh, M.: Assimilation of remotely sensed soil moisture and snow depth retrievals for drought estimation. Journal of Hydrometeorology, 15 (6), 2446–2469, https://doi.org/10.1175/JHM-D-13-0132.1, 2014.

Kumar, S.V., Zaitchik, B.F., Peters-Lidard, C.D., Rodell, M., Reichle, R., Li, B., Jasinski, M., Mocko, D., Getirana, A., De Lannoy, G., Cosh, M.H., Hain, C.R., Anderson, M., Arsenault, K.R., Xia, Y., & Ek, M.: Assimilation of gridded GRACE terrestrial water storage estimates in the North American Land Data Assimilation System. Journal of Hydrometeorology, 17 (7), 1951–1972. https://doi.org/10.1175/JHM-D-15-0157.1, 2016.

Reichle, R.H., Crow, W.T., & Keppenne, C.L.: An adaptive ensemble Kalman filter for soil moisture data assimilation. Water Resources Research, 44 (3), W03423, https://doi.org/10.1029/2007WR006357, 2008.

5. L215: It is not clear to me how localization is applied in the assimilation. Does it weigh the covariance matrix? It might be better to link equation (7) with the relevant equation mentioned above.

I might not fully understand it, but why localization used to update SWE estimates would impact the open loop results shown in figure 6? I assume localization would only impact DA SWE.

The localization is applied in the assimilation by weighting distances from the flight lines (up to a specified localization distance; *r*) using the Gaussian decay-based function. The degree of the SWE updates for a grid cell from the assimilation is calculated using a localization weight (W) which is calculated based on the distance (*d*) from the updated grid cells overlapped with the flight line.

Yes, the localization only impacts DA SWE. The reason why the OL statistics were changed with increasing the localization distances in Figure 6 is that the effective distance used to calculate the domain-averaged OL SWE was changed according to the localization distance. For example, at a localization distance of 4 km, the domain-averaged OL SWE is calculated using gridded SWE values within an effective surrounding area (4 km distance from the gamma flight lines). At 32 km, the domain-averaged OL SWE was calculated based on larger number of the gridded SWE values within surrounding areas up to 32-km distance from the gamma flight line. The OL/DA statistics in Figure 6 are calculated using domain-averaged time series of OL/DA SWE over the effective surrounding areas by localization distances with the corresponding UA SWE.

We edited and added descriptions in Section 4.3 as below.

"To quantify if the spatially sparse gamma SWE observations can improve the SWE estimates in the surrounding areas, where the observations are not available, we apply a distance-based localization method into the assimilation procedure. The localization is applied in the assimilation by weighting distances from the flight lines (up to a specified localization distance; r) using the Gaussian decay-based localization method as follows:

$$W = exp\left\{\frac{-d^2}{2\cdot\left(\frac{r}{2}\right)^2}\right\} \qquad (7)$$

where d is the distance between the updated grid cells (i.e., flight lines) and grid cells without observations within a specified localization radius r. The degree of the SWE updates for a grid cell from the assimilation is calculated using the localization weight (W) which is calculated based on the distance (d) from the updated grid cells overlapped with the flight line."

When double-checking our calculation for Figure 6, we found a script error when calculating the error metrics. The error metrics were miscalculated using a "static" domain-averaged OL SWE time series with DA SWE time series at different localization distances. They should have been calculated with a domain-averaged OL SWE time series at a "corresponding" localization distance. We reviewed throughout the script, corrected the part, and recalculated the metrics which are shown in the new figure below.

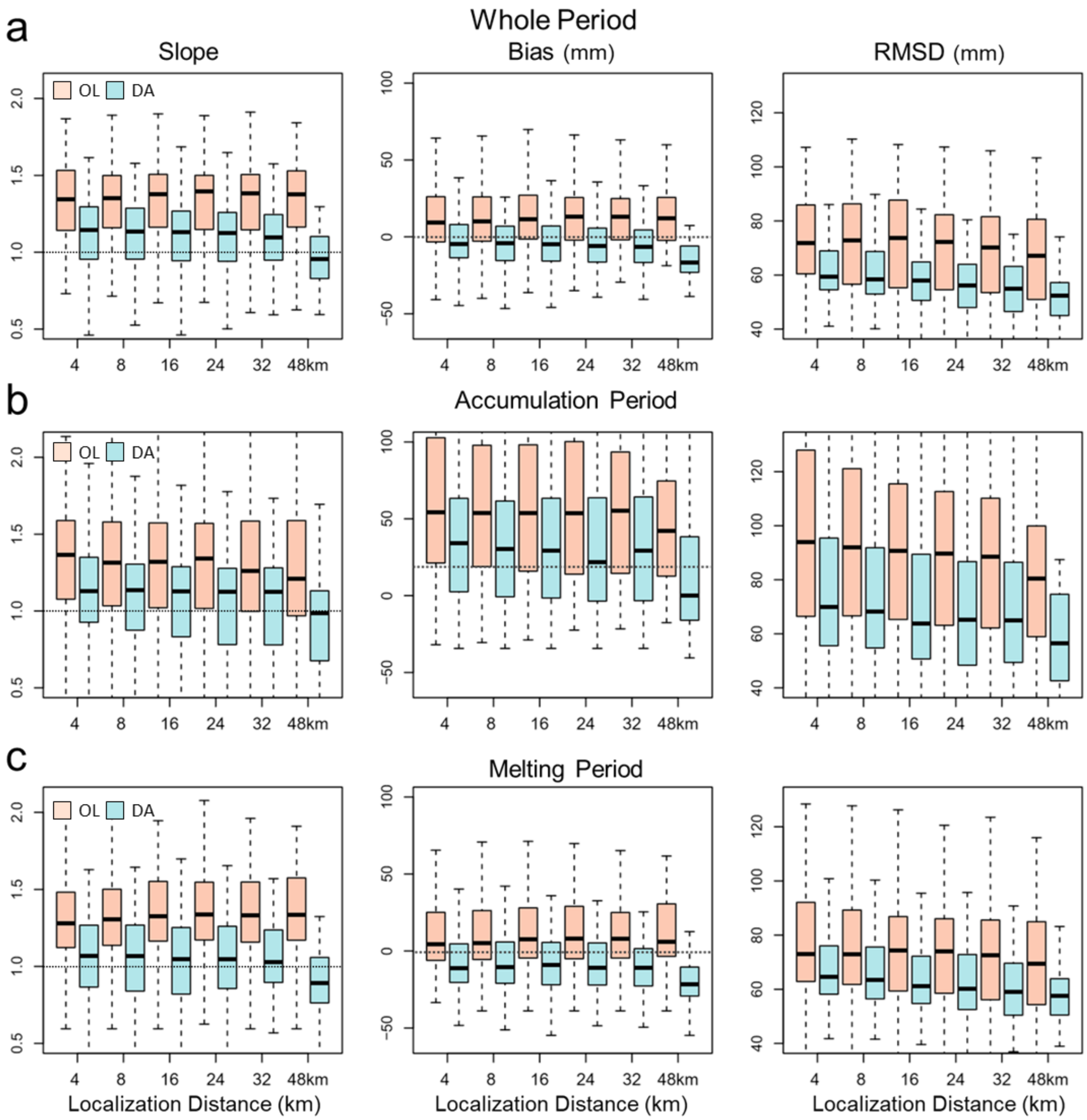

Figure 6. Localized data assimilation (DA) and open-loop (OL) Noah-MP SWE performances as compared to the UA SWE with different localization distances (e.g., 4, 8, 16, 24, 32, and 48 km) for the whole (accumulation and melting periods), accumulation, and melting periods, respectively.

The result parts of the manuscript related to the revised figure 6 were also revised as below.

"For the whole snow season that includes both accumulation and melting periods, the boxplot of the 1:1 slope shows that the localized DA SWE were improved as compared to OL. The slopes of the DA SWE are closer to 1 than the OL's slopes. The bias and RMSD boxplots also show that the DA SWE has lower errors than the OL SWE for all localization distances, expect for bias at 48 km which is too low (median: – 23 mm). The OL's RMSDs slightly increased at the distances up to 16 km (median: 72 mm) and decreased after that, while the DA's RMSD values continually decreased with increasing the distances up to 48 km (median: 53 mm). When the statistics were calculated for the accumulation and melting periods separately, the lower RMSDs and slopes closer to 1 of the localized DA SWE were found consistently. As previously discussed, the efficacy of assimilating the airborne gamma SWE is greater during the

accumulation period, especially for bias and RMSD, than during the melting period. In the melting period, the improvements in the bias, RMSD and 1:1 slope are also achieved up to 32 km"

6. L226: maybe use lowercase "A" in the parenthesis.

Edited.

7. L236: it seems peak SWE might not be correctly estimated if only one data point exists after the accumulation season (Figure 4 SJ150 in WY 1989 and NH106 in WY 1997). It might be worth pointing that out and/or discussing this issue.

We agreed with your comment. To point out the issue, we added a statement as below.
"Also, the peak SWE cannot be corrected if a single gamma SWE exists only after the accumulation period."

8. Please be consistent with either RMSD or RMSE throughout the manuscript.

We edited and consistently used RMSD throughout the manuscript.