Reviewer #2

The authors expanded the GFDL LM4.1 snowpack processes to include LAPs effects and evaluate the simulated snow albedo across different measurement sites. They found that the resulting snow model with LAP-aware snow reflectivity show a good agreement with measurements of broadband albedo and seasonal SWE over the study sites. They further evaluated the number of snow-days lost due to the deposition of dust and carbonaceous aerosols, which ranges between 5 and 24 days depending on locations. This work is an important improvement for the GFDL snowpack model, which could provide better simulations for future coupled climate runs. Overall, the manuscript is well organized. I have a few specific comments/suggestions for the authors to consider.

We thank the reviewer for providing insightful comments on our work. Please find below a point-by-point response to the comments on the manuscript. We believe addressing these comments would significantly improve the manuscript with respect to our first submission. We report the review in black, and our response in light blue. Excerpts from the revised manuscript with the proposed changes are reported here in *Italic*. The line numbers referenced in our response refer to the position of the excerpts in the proposed revised manuscript.

Specific comments:

1. Is there any canopy snow process included in LM4.1? A brief description would be useful.

The land model GFDL LM4.1 in general resolves snow intercepted by canopy layers, computing its full mass and energy balance. The albedo of leaves is a weighted average which considers the amount of fractional leaf area covered by snow. However, in the current application vegetation is not considered for the sites. In this paper, we limited our analysis to experimental sites with little to no vegetation cover, in order to focus our attention on the effect of impurities rather than on the complex snow-vegetation interactions. Therefore, we have turned off vegetation in the model, which effectively is not allowed to grow throughout the simulation. We have expanded the discussion on this model setting in the manuscript, and we now note that further investigations focusing on forested sites would be an important topic of future research. We propose to add the following text to the manuscript (at line 126 of the revised manuscript):

"In LM4.1, vegetation is represented by a set of plant cohorts which evolve dynamically. Multi-layer vegetation canopies interact with the surface via multiple processes, including turbulent exchange of mass and energy, the transfer of longwave and shortwave radiation, and the interception of liquid and frozen precipitation. For additional details, the reader is referred to

Shevliakova et al., (2024). In this application, we have decided to limit our analysis to sites with little to no vegetation in order to focus our attention on snow and LAP deposition processes. Therefore, vegetation is turned off and canopy layers are not present in the model simulations, following the experimental setup used in Zorzetto et al. (2024)."

However, we believe that further validation of the model proposed here over forested areas would be an important and informative future research objective. We proposed to discuss this in the discussion section of the revised manuscript at line 475:

"Furthermore, the current application was limited to sites with little to no vegetation. We believe further investigation of model performance and of the effects of LAPs for snowpacks in forested areas would be an important addition to the current research."

2. How is the snowpack liquid water treated? Is there a liquid water holding capacity parameter prescribed? How important is it to assign different inter-layer snowmelt water scavenging coefficients for IM vs EM LAPs?

We agree that the treatment of liquid water flow was not discussed in the original paper, and it is a relevant process for the transport of LAPs. We propose to include a detailed description of these processes instead of just referring the reader to the model description paper (Zorzetto et al., 2024). We propose to include the following discussion at line 162:

"At each model time step, the full energy and water mass balance of the snowpack is solved using an implicit numerical formulation, which is required for land-atmosphere coupled model runs with relatively coarse time step (30 min) for which the GLASS was designed. After performing the vertical heat balance of the snowpack, the temperature change and change of phase is evaluated for all snow layers. The vertical balance of liquid water is then evaluated throughout the snowpack, with liquid precipitation providing the upper boundary condition. The snow density is used to evaluate the pore space available for liquid water in each snow layer. Following Vionnet et al., (2012), the liquid water holding capacity for a snow layer k with thickness Δz_k *and local solid-phase density* $\rho_{s,k}$ *is given by*

$$W_{liq,max,k} = 0.05\rho_w \Delta z_k \left(1 - \frac{\rho_{s,k}}{\rho_i}\right)$$

with ρ_i *the density of ice, and* ρ_w *that of liquid water. For a more detailed description, see Zorzetto et al., (2024)."*

With respect to the second point, we now clarify that we are using a single scavenging coefficient for IM and EM LAPs, and propose to discuss potential extensions of the model with more realistic scavenging parameterizations. We propose the following addition at line 453 of the revised manuscript:

"The modelled LAP concentration directly depends on how scavenging processes of LAPs in the snowpack are modelled. In the current model formulation, scavenging coefficients are constant across snow layers, and the three LAPs species considered here do not consider different behavior between e.g., hydrophobic and hydrophylic carbon components. Furthermore, scavenging coefficients do not depend on the mixing ratio of impurities (i.e., internally or externally mixed). Future extension of the model could include more realistic scavenging parameterization depending on the LAP mixing state."

3. How did the authors assign dry and wet deposited LAPs to IM or EM within the snow?

Dry deposition contributes to EM impurities in the snowpack, while wet deposition (from both liquid and frozen precipitation) contributes to IM LAPs. We propose to clarify this feature of the model in the revised manuscript. The proposed addition at line 181 would read:

"The mass of LAPs added to the snowpack by dry deposition is assumed to be externally mixed (EM), while LAPs deposited as wet deposition either due to liquid or frozen precipitation contribute to IM LAPs within the snowpack. In the model, the state of mixing of LAPs within the snow (IM or EM) does not change as a consequence of melt and freeze cycles occurring in a snow layer."

4. How many dust size bins are considered and what are they?

We have considered a single bin size with a single absorption value for dust. This is clearly a limitation, but can be extended in future studies provided that dust bins sizes and their respective optical properties are available as model input data. We now propose to explicitly mention this in the manuscript (line 176):

"The model considers a single LAP size distribution for mineral dust, without tracking separately dust particles of different sizes. "

5. Does melt-freeze of snow change the IM or EM status of LAPs?

In its current configuration, melt/freeze cycles do not change the IM/EM status of LAPs within the snowpack. We proposed to clarify this point in the revised manuscript (line 181):

"The mass of LAPs added to the snowpack by dry deposition is assumed to be externally mixed (EM), while LAPs deposited as wet deposition either due to liquid or frozen precipitation contribute to IM LAPs within the snowpack. In the model, the state of mixing of LAPs within the snow (IM or EM) does not change as a consequence of melt and freeze cycles occurring in a snow layer."

6. Is internal heating within the snowpack column due to light absorption considered?

Yes, as mentioned in the original manuscript we do consider the penetration of light within the snowpack and related heating rates. W propose to discuss this feature of the model in the revised manuscript as follows:

For thick enough snowpack (snow depth > 0.02 m), solar radiation penetrates within the snowpack and absorbed radiation is distributed exponentially

$$Q_s(z) = \sum_{b=1}^{2} (1 - \alpha_b) R_{s,b} e^{-\beta_b z} \tag{6}$$

where for each band b $R_{s,b}$ is the downward shortwave radiation at the surface, and β_b describes the penetration of light within the snowpack. The extinction coefficients for visible and near infrared light are estimated as in Jordan (1991) and Shrestha et al. (2010): $\beta_{NIR} = 400$, and $\beta_{VIS} = 0.003759\, \rho\, d_{opt}^{-0.5}$, with density and optical diameter averaged over the near-surface layer of the snowpack, up to a maximum depth of 3 cm.

7. The snow albedo parameterization from Dang et al. 2015 and He et al. 2018 is for semi-infinite snowpack, which may lead to uncertainties in the albedo calculation here. It will be good to clarify this and briefly discuss this.

We agree that this limitation of the model should be discussed in the manuscript. The snow albedo parameterization employed is indeed for semi-infinite snowpack and thus can give rise to errors in the case of thin snowpacks. We propose to clarify this point in the revised manuscript, instead of just referring to Zorzetto et al. (2024).

Furthermore, in the revised manuscript we propose to explicitly describe how thin snowpacks are treated in the model. In particular, we mention that in the case of thin snowpacks, a snow area fraction is computed and used for albedo calculations. We propose to add the following to the model description:

Note that the albedo parameterization employed here based on the work of Dang et al. (2015) and He et al. (2018a) is derived for a semi-infinite snowpack, and thus in general can lead to biased albedo estimate for thin snowpack. In the case of this snowpack, the model computes a fractional snow cover f_{snow} based on snow depth as follows

$$f_{snow} = \frac{h_s}{h_s + h_{s,c}} \tag{7}$$

with h_s the snowpack depth, and $h_{s,c} = 0.0167$ m. In case of fractional snow cover, surface albedo is computed as a weighted spatial average of snow and snow-free substrate optical properties.

In the discussion we propose to further discuss this issue by stating that (line 467):

“Furthermore, the model could be improved by using more detailed optical models such as the Two-stream Analytical Radiative TransfEr in Snow (TARTES) model (Libois et al, 2013) or the Snow, Ice, and Aerosol Radiative (SNICAR) model (Flanner and Zender, 2005), which are not limited to the case of semi-infinite snowpack as is the case for the albedo parameterization used here. “

8. How did the authors use the snow grain shape parameters to compute the optical diameter?

Prompted by this comment, we plan to expand the description of how snow grain size and shape are used to compute snow albedo. At line 249 of the revised manuscript, we propose to add the following:

"The snow albedo parameterization used here explicitly accounts for the effects of snow grain size (through the snow grain effective radius) and shape on its optical properties. He et al., (2018) introduced Eq. (3) and provided the set of parameters b_0, b_1 *, and* b_2 *tabulated for four different snow grain shapes (sphere, spheroid, hexagon, and Koch snowflake). In GLASS, snow microphysics in each snow layer is parameterized by two parameters (snow sphericity and dendricity) which evolve in time due to the combined effect of dry and wet snow metamorphic processes, as well as due to wind effects (Zorzetto et al., 2024). The coefficients used in eq. (3) are selected at each time step based on snow shape properties in the near-surface snow layer: High-dendricity snow (*$\delta_{ns} > 0.5$*) is idealized as a collection of Koch snowflakes. Snow with lower dendricity parameter is considered as a collection of spheres (if sphericity parameter (*$s_{p,ns} > 0.8$*), spheroids (if* $0.8 > s_{p,ns} > 0.2$*), or hexagonal crystals (if* $s_{p,ns} < 0.2$*)."*

9. How is the alpha_b in equation (5) computed? What is the physical meaning of this parameter?

Here alpha_b is the snow surface albedo, which is computed according to eq. (2) based on physical properties on the snowpack (i.e., grain size and shape). We propose to clarify this by updating the notation used in the paper, and using the suffix b for snow surface albedo also in eq. (2), so as to clarify the dependence of albedo on the shortwave band (b = VIS or NIR for visible or near infrared band, respectively).

10. For daily average of albedo, did the authors use downward solar radiation as the weights?

Yes, we indeed used dimensional quantities as model output (downward and upward radiation fluxes) and computed their ratio at the daily time scale at which results are displayed. We now propose to explicitly mention this in the manuscript to clarify the procedure used in the analysis. At line 326 of the revised manuscript we propose to add the following:

"In order to compare modelled and observed surface albedo, modelled upward and downward shortwave fluxes are averaged daily to correspond to observations"

11. Figure 2A: Why does the model have a consistently high snow albedo than observations after 2014 May (which seems to be a snow-free period)? Also, since there is no snow after May 2014, why is the snow albedo from both observation and model not zero?

We now clarify that the quantity shown both in Figure 2A and Figure 5 of the original manuscript is the surface albedo, averaged at the daily time scale, which does not always correspond to the snow albedo. This is the same quantity provided in the observational datasets used for comparison. We propose to clarify this in the figure captions to explain why albedo values are shown even for snow-free periods. Furthermore, in the revised paper we propose to mention that for thin snowpacks the model computes an effective snow fractional area and therefore surface albedo is effectively based on a weighted spatial average of snow and snow-free substrate optical properties. At line 265 of the revised manuscript we propose to add the following:

Note that the albedo parameterization employed here based on the work of Dang et al. (2015) and He et al. (2018a) is derived for a semi-infinite snowpack, and thus in general can lead to biased albedo estimate for thin snowpack. In the case of this snowpack, the model computes a fractional snow cover f_{snow} based on snow depth as follows

$$f_{snow} = \frac{h_s}{h_s + h_{s,c}} \tag{7}$$

with h_s the snowpack depth, and $h_{s,c} = 0.0167$ m. In case of fractional snow cover, surface albedo is computed as a weighted spatial average of snow and snow-free substrate optical properties.

12. Section 4: the description of the results is too qualitative. Please include some quantitative numbers when presenting the results. Also, more physical explanations/insights could be added to the results. For example, why does the model not capture the variation of the daily albedo variation over most sites (Figure 4) and why does the model results show systematic overestimate/underestimate in some sites (Figure 5).

We propose to add to the results section a more quantitative discussion of the discrepancies between modeled and observed snowpack properties as suggested in this comment.

At line 404 of the revised manuscript we propose to discuss the bias in SWE:

We note that, for the sites which exhibit the largest SWE discrepancies between model output and observations, the effect of LAPs predicted by the model does not appear to be the primary reason for model biases. In particular, accounting for LAPs does not appreciably change the seasonal peak SWE, which the model underestimates by about 22% at two of the sites (swa and wfj) and by about 34% at sap.

For albedo, at line 342:

"During the accumulation phase, some underestimation of daily albedo by the model can be observed at some of the sites (snb, sap, and wfj). It is worth noting that some of the sites where the model exhibits the largest SWE underestimations correspond to sites where a negative bias in snow surface albedo is also reported (e.g., at wfj and especially at sap, which is the site

characterized by the largest SWE and albedo underestimation) so that surface albedo appears the primary source of the SWE bias."

And at line 397 we propose to add:

Furthermore, the temporal variability of modelled albedo is generally smaller than the observed one. This can be the result of effects due to the snow surface grain properties, and can also depend on how the solar angle is included the albedo parameterization through the coefficient $\Phi_b(\mu)$.

Now at line 459 we propose to add a comparison with observed concentration of LAPs:

"Uncertainty in modelled LAP concentrations and snow optical properties also depends on the dataset of input LAP deposition fluxes used here. While the other atmospheric variables used to force the land model consists of in-situ observations, LAP deposition data are obtained from a reanalysis dataset. Therefore, LAP fluxes used here are coarser in space and time and may not be fully representative of the local deposition flux at the sites. The comparison with LAP concentration observations at two of the sites helped us constrain these sources of uncertainty and showed that while modelled LAP concentrations exhibit discrepancies with observations, overall the order of magnitude and seasonal trend throughout a snow season are reproduced by the model."

13. Figure 7: It seems that the model SWE bias is dominated by other model snow or forcing processes instead of LAP effects. This may be worth some discussion.

We agree and propose to discuss this at line 403 of the revised manuscript as follows:

"SWE model predictions show a good fit to observations at most SnowMIP sites (Figure 7). However, for some stations overestimation (rme, snb) or underestimation of SWE (swa, sap, wfj) are observed for the specific snow year examined. We note that, for the sites which exhibit the largest SWE discrepancies between model output and observations, the effect of LAPs predicted by the model does not appear to be the primary reason for model biases. In particular, accounting for LAPs does not appreciably change the seasonal peak SWE, which the model underestimates by about 22% at two of the sites (swa and wfj) and by about 34% at sap.

14. I would suggest adding a subsection for uncertainty discussion. Some of the uncertainties involved in the model are mentioned in my earlier comments. A few key uncertainty factors that are worth discussing: (1) snow grain shape and size evolution, (2) using Eq.8 to combine different LAPs, (3) aerosol deposition flux, (4) missing snowpack processes, (5) LAP meltwater scavenging, etc.

We agree. Given the number of processes involved we believe that a complete quantitative assessment of model uncertainty is beyond the scope of the present work. However, discussing these sources of uncertainty is important and helps to assess our results.

We propose to add a subsection to the Discussion section entitled *"Sources of uncertainty and model limitations"* where we will expand our discussion of model limitations and sources of uncertainty. At line 452 of the revised manuscript we propose the following discussion subsection:

"

5.1 Sources of uncertainty and model limitations

The modelled LAP concentration directly depends on how scavenging processes of LAPs in the snowpack are represented in the snow model. In the current GLASS formulation, scavenging coefficients are constant across snow layers, and the three LAPs species considered here do not consider different behavior between e.g., hydrophobic and hydrophylic components for carbonaceaous LAPs. Furthermore, scavenging coefficients do not depend on the mixing state of impurities (i.e., internally or externally mixed particles). Future extension of the model could include more realistic scavenging parameterization depending on the LAP mixing state.

Uncertainty in modelled LAP concentrations and snow optical properties also depends on the dataset of input LAP deposition fluxes used here. While the other atmospheric variables used to force the land model consists of in-situ observations, LAP deposition data are obtained from a reanalysis dataset. Therefore, LAP fluxes used here are coarser in space and time and may not be fully representative of the local deposition flux at the sites. Furthermore, here we assume that dust absorption properties are constant globally, while in general they do depend on dust mineralogy, which is spatially heterogeneous.

The comparison with LAP concentration observations at two of the sites helped us constrain these sources of uncertainty and showed that while modelled LAP concentrations exhibit discrepancies with observations, overall the order of magnitude and seasonal trend throughout a snow season are reproduced by the model.

Furthermore, the model could be improved by using more detailed optical models such as the Two-stream Analytical Radiative TransfEr in Snow (TARTES) model, (Libois et al, 2013) or the Snow, Ice, and Aerosol Radiative (SNICAR) model (Flanner and Zender, 2005), which are not limited to the case of semi-infinite snowpack as is the case for the albedo parameterization used here. We found that both when considering clean snow and in the model configuration with LAPs, biases in modelled albedo are observed at some of the sites. Using a physically based model such as TARTES or SNICAR might help reduce these discrepancies. We note that despite *the bias observed for specific sites, the model was developed for global applications and was not tailored to terrain or climate conditions of these sites. Thus, multiple physical processes may be at the origin of these discrepancies, as also discussed by Zorzetto et al., (2024), and should be the subject of future model development and testing efforts. Furthermore, the current application was limited to sites with little to no vegetation. We believe further investigation of model performance and of the effects of LAPs for snowpacks in forested areas would be an important addition to this line of research."*

And at line 480:

"While the model was here tested in a land-only configuration forced by an offline atmosphere, LM4.1 and GLASS are designed as components of an Earth System Model and are thus tailored to coupled simulations with an atmospheric model. In this coupled model configuration, currently under development, the computation of the monthly constant mixing ratio of LAPs in precipitation will no longer be needed as both liquid/frozen water fluxes and LAP deposition fluxes will be provided by the same atmospheric model. This would further reduce one of the sources of uncertainty here, due to the coarse temporal resolution of LAP deposition fluxes."

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