Thank you to editor Fabien Maussion as well as Charles Amory and the other, anonymous, reviewer for donating their time and energy to our manuscript. In order to address all comments, we have responded to each of them individually in blue. Line numbers in our responses refer to the track-changed version of the revised manuscript. Some newly added citations in our responses can be found in the revised manuscript.

We apologize for the very long delay in responding to the reviewers comments and revising the manuscript. We thank the editor, reviewers and editorial office for their patience and understanding that this is the unfortunate result of the first, second and last author having transitioned between jobs, as well as moving, in the last year.

On behalf all authors,

Eric Keenan

Review #1 - Anonymous

General comments:

The article "A wind-driven snow redistribution module for Alpine3D v3.3.0: Adaptations designed for downscaling ice sheet surface mass balance" presents a strategy for downscaling large scale surface mass balance (SMB) predictions using Alpine3D. Alpine3D is a 3-D model that computes the mass and energy balances of snow covered regions by solving the 1-D snow model SNOWPACK at each grid cell. In the proposed methodology, the meteorological conditions are extracted from MERRA-2 and downscaled to the Alpine3D grid. In addition, snow drift events are modeled with a new 2-D advection scheme that takes into account a parameterization for the mass flux in saltation previously implemented in SNOWPACK. In order to correctly estimate snow drift, high resolution wind fields are needed. They are computed offline with the software WindNinja, which takes into account the small scale topographic features through a digital elevation model (DEM). The proposed approach has the potential to improve our understanding of SMB variability at small scales and can easily be applied to other locations. Even though the snow drift model needs further improvement, the coupling of MERRA-2 outputs, WindNinja wind fields and a snow drift scheme with Alpine3D has significant scientific value. In addition, from the comparison between Alpine3D and the measured annual-averaged snow accumulation over a 130 km transect, the authors show the importance of wind redistribution of snow to the local SMB. However, I think the manuscript can be improved, both from a strategic and scientific points of view. Besides the scientific comments presented below I have one general comment:

1) The article risks promising more than it gives regarding the snow drift model. Emphasis is given to snow drift in the title, in the abstract and in the introduction. However, even though the treatment of erosion and deposition presented in section 2.4 can be considered new, it is highly dependent on the parameterizations for the fluid threshold friction velocity and the mass flux in saltation (eqs. 3 and 4). In particular, it is shown and stated by the authors that equation 4 is highly uncertain, as it relies on a poorly constrained parameter. These equations are standard in SNOWPACK and no improvement is suggested by the authors. In addition, it is not clearly stated why this snow drift model is better than the one previously implemented in Alpine3D (Lehning et al. 2006). In this way, I would suggest counter-balancing the focus on the snow drift model with an extended description of the peripheral developments that are of the utmost importance for a successful downscaling: the downscaling of MERRA-2 meteorological forcing to the Alpine3D grid, the use of WindNinja with the ICESat-2 DEM, and the coupling of WindNinja to Alpine3D. In my view, the technical details of

these contributions are of interest to the users of Alpine3D or other models alike. In addition, it is aligned with the scope of the GMD journal.

We thank the reviewer for this feedback and we agree that indeed some more context would be helpful to include for the reader. First, we would like to mention that we consider redeveloping the saltation mass flux parameterizations, as the one we used in Eq. 4, to be out of scope for this study. Particularly since we have achieved satisfying results using this parameterization in earlier work (Keenan et al., 2021, Wever et al., 2022). We consider the study presented in this manuscript as a 2D expansion of the 1D modeling performed in those studies, which we think justifies leaving the current saltation mass flux calculations in SNOWPACK untouched. Furthermore, it is important to note that the snow drift module as used before by for example Lehning et al. (2006), Mott et al. (2008), and Groot-Zwaaftink et al. (2013b), requires 3D wind speed fields at high temporal resolution, which would require a weather model downscaling tool, which adds substantial complexity to such a study. The fully 3D model is also computationally intensive, as it currently is not parallelized. This means that earlier model studies were restricted to shorter time periods (i.e., simulating a winter season, or a case study) and/or relatively small domain. Finally, it is more difficult to maintain mass balance in the current implementation of the 3D model, since snow and precipitation may remain in suspension and never reach the surface, while erosion is restricted to one layer per time step for code simplicity. Solving the fully 3D suspension, considering the added computational effort, is also not guaranteed to provide much better agreement, as discrepancies with observed snow accumulations have been reported (e.g., Mott et al., 2008, Mott et al., 2010, Gerber et al., 2017), such that we think it is justified to test simpler approaches like the one in our study. In our approach, we avoid these computationally challenging issues. Also note here that the full 3D snowdrift model in fact calls the exact same snow drift functions in the SNOWPACK model to determine drifting snow mass flux as we are applying in this study. We now provide more explanation in the manuscript (see L83-87).

Specific comments:

l.13-14: Taking into account the focus that is given in the Conclusions regarding the effect of the parameter L, I suggest moving the focus in this sentence from the underestimation of SMB variability to the sensitivity of the snow accumulation patterns to the saltation model employed.

Excellent point. We have revised to (see L14-17):

“Despite these improvements, our results also demonstrate that considerable uncertainty stems from the employed saltation model, confounding simulations of surface mass balance variability.”

l.19-21: The terms drifting and blowing snow are used both to define the processes of aeolion snow transport and the particles aloft. This paragraph focuses on processes (precipitation, sublimation, etc). Hence, I suggest rephrasing so that drifting and blowing snow are presented as processes. An example is provided to clarify the comment made: "Additionally, local SMB is influenced by wind redistribution of snow. This process is generally defined as drifitng snow (when the snow particles are transported by the wind in the first 2 m above the snow surface) or blowing snow (when the snow particles are transported by the wind at greater heights - above 2 m height). We refer to deposition when drifting and blowing snow lead to net mass gain and to erosion when they lead to mass loss."

In retrospect, we agree that our description was rather clunky. We have revised to the following (see L22-27):

“Additionally, local SMB is influenced by the process of wind-driven snow redistribution, which we refer to as deposition in the case of local mass gain and erosion in the case of mass loss (Lenaerts and van den Broeke, 2012). Wind-driven snow redistribution is generally divided into two categories, drifting snow, where snow particles are transported by the wind in the lowermost 2 m of the atmosphere, and blowing snow where snow is redistributed at heights greater than 2 m.”

l.34-35: More recent works can be cited describing the effect of interparticle cohesion not only on the fluid threshold but in the whole saltation dynamics (e.g. Comola et al. 2019, Melo et al. 2022).

Thanks for bringing these studies to our attention. Indeed they both provide recent insight into saltation dynamics and snow particle cohesion. Thus we have added their citations to the corresponding sentence (see L40-41).

l.39: I suggest citing also the early work of Schmidt (1980) on the impact of interparticle ice bonds on the fluid threshold.

Thanks for the comment. We unintentionally neglected a discussion of impact forces on the development and sustainment of saltation. We’ve revised the mentioned sentence to the following (see L44-46):

When the combined effect of surface wind stress and impact force from saltating particles exceeds cohesive forces at the snow surface, saltation of snow particles is initiated or maintained within the lowermost 10 cm of the atmosphere (Schmidt, 1980; Pomeroy and Gray, 1990).

l.42: The work of Amory et al. (2021) can also be cited here - a parameterization for drifting snow compaction is also proposed in their work.

Good point. We have added this reference (see L62).

l.85-86: Is this a standard assumption? Maybe the authors can clarify its validity.

We are not sure if this assumption could really pass with the qualifier “standard”, but it has been noted in literature before. For example, Fig. 1.3 in Ligtenberg (2014) shows the dissipation of seasonal temperature fluctuations in the uppermost 10m of the firn, to which the author makes the following remark: “... and the local temperature is equal to the long-term average surface temperature.” As we already noted in the manuscript, we also applied this boundary condition in our previous study (Keenan et al., 2021). Furthermore, Alley and Koci (1990) note that “At dry-snow sites such as GISP2, the temperature at 10 m depth typically is within a few tenths of a degree of the mean annual air temperature, with the firn usually colder than the air; however, the difference can be as large as a few degrees”. From this, we believe our boundary condition is a reasonable simplification, particularly in the absence of a clear alternative approach, without having the need to simulate the full firn column.

In order to provide more justification for this assumption, we have revised the text to (see L112-116):

“We follow Keenan et al., 2021, by applying the MERRA-2 mean annual surface temperature as a Dirichlet thermodynamic boundary condition at the bottom of the firn column. This assumption is supported by observations from the dry snow zone of the Greenland ice sheet, where differences between mean annual air temperature and firn temperature at 10m were found to be typically within a few tenths of a degree (Alley and Koci, 1990). Ligtenberg (2014) also shows in a model result that the seasonal cycle in firn temperature disappears around 10m depth.”

l.121: Even though Φ has units of mass flux (kg/m2/s), it can only be considered a mass flux if the mass rate of saltating particles per unit width, Q (kg/m/s), is assumed to be deposited along a fetch of L meters long. From my point of view, only in this way it makes sense to describe Φ as the mass rate of particles "crossing" the section Ly times Lx, where Ly is the width and Lx is the fetch length L. Is this the meaning of L? Even though this parameter is not well constrained, I think an effort should be made to better define it.

“Even though Φ has units of mass flux (kg/m2/s), it can only be considered a mass flux if the mass rate of saltating particles per unit width, Q (kg/m/s), is assumed to be deposited along a fetch of L meters long.” This is correct and indeed how we conceptualize L.

To clarify the definition of *L*, we have added (see L156-157) the following brief explanation and reference to our Keenan et al. (2021) paper in which we go into greater detail on the saltation scheme.

“*L* can be conceptually understood to represent the distance over which the originally upwind and now saltating particles have been eroded from the snow surface (Keenan et al, 2021).”.

l.124 (eq.4): The numerator of this equation corresponds to the expression proposed by Sørensen (1991) for the transport rate (see page 75 of the article, eq. 3.22). This expression is in units of g/cm/s (this is stated at the end of page 72 of the article, below equation 3.9). This poses a problem because the coefficients 0.0014 and 205 are not dimensionless values - 205 has velocity units (cm/s) and 0.0014 has units of s2/cm. If we want to express Q in units of kg/m/s, these factors should change to 2.05 and 0.14, respectively. The dimensionally correct expression predicts much higher values of Q and its validity to model snow saltation is still to be assessed. This issue with the Sørensen's expression was previously pointed out in the PhD thesis of Vionnet (2012), page 103, Fig. 5.3 (french only). In the mentioned PhD thesis as well as in Vionnet et al. (2014), the use of the latest expression of Sørensen (2004) is proposed. This can be a good option for Alpine3D as it does not deviate significantly from the dimensionally wrong Sørensen equation (see Fig.5.3 in the PhD thesis). Independently of the approach chosen by the authors, I believe it is advisable to present Q - the numerator of eq.4 - in a separate equation and cite the respective article.

Thank you for pointing this out. Indeed, after looking into the original reference, we agree that the magnitudes of the parameters in our implementation are incorrect. Furthermore, we have implemented your suggestion and now explicitly define the numerator of eq. 4 as Q.

Unfortunately this error seems to have been introduced in SNOWPACK by Lehning and Fierz (2008) and at this point remains unfixed. We have created an issue in our source code repository to be fixed in the future.
https://github.com/snowpack-model/snowpack/issues/24

Now, we would like to recall here that we build upon the work by Keenan et al. (2021) and Wever et al. (2022), who showed how the erosion and deposition calculated using the currently implemented code yielded satisfying results. The fact that an incorrect parameterization could yield satisfying results can be understood when considering that Q is already scaled by the poorly constrained tuning parameter L, such that any changes in the formulation of Q could be accompanied by varying the fetch length L to maintain similarly good performance. Lastly, correcting this parameterization and updating the simulations for this manuscript would require substantial additional effort, that, in light of what we mention above, may have little impact on the conclusions in this work. Nevertheless, transparency of this issue is necessary, thus, in order to account for your astute (and very welcomed!) comment, we have added the following text (see L179-190):

“Additionally, it has come to our attention that SNOWPACK’s parameterization of Q does not perfectly match the original parameterization proposed by Sørensen (1991). As noted in Vionnet (2012), the parameters 0.0014 and 205 in Eq. 4 reflect units for Q of g cm−1 s−1, whereas here we define Q with units of kg m−1 s−1. This implementation error in SNOWPACK was introduced by Lehning and Fierz (2008), but since we build upon the previous work by Keenan et al. (2021); Wever et al. (2022), who showed satisfying results for erosion and deposition calculated using the currently implemented code, we did not correct this error. Practically speaking, this error leads SNOWPACK to underestimate the magnitude of Q compared to the intended parameterization described in Sørensen (1991). However, it is also important to note here that Q is ultimately scaled by the poorly constrained tuning parameter L, such that any changes in the formulation of Q could require a new choice for the fetch length L to maintain similarly good performance. Thus, it is not certain that updating our parameterization of Q would lead to improved or more physically meaningful results. That said, future studies could consider using the updated parameterization of Q introduced in Sørensen (2004) which Vionnet (2012) showed to produce similar results to our presently dimensionally incorrect parameterization of Q (Vionnet et al., 2014).”

l.131-132 (point 1): The wind field at multiple vertical levels cannot be computed with WindNinja?

WindNinja could in fact save output at multiple vertical levels. However, what we try to illustrate here is that this is not necessary in our approach. We only need to save and calculate the wind speed at 10 m. This allows for a highly efficient coupling to the Alpine3D model. To make this more clear we have revised to the following (see L170-171):

“full prognostic solution of blowing snow transport via suspension requires numerically expensive calculations using wind vector fields at multiple vertical levels (Lehning et al., 2006, Sharma et al., 2021)”

l.133-134 (point 3): This is not advisable at high wind speeds because the aeolian transport of snow stops being governed by the wind field close to the ground alone. In addition, the saltaion velocity considered in eq.7 would have to be revised (it describes saltation only as suspended particles are expected to have velocities comparable to the wind speed).

Given your remark and the remarks in the review by Charles Amory on the importance of suspension versus saltation, we now do agree that the argument that we use the fetch length to naively account for suspension is not as justified as we made it sound, as you pointed out. It is indeed better not to aim to include suspension in saltation given the different advection speeds. We rephrased this paragraph to better reflect these notions (see L173-178).

l.144 (eq.6): I believe the variable us should be defined in a more clear way: does it represent the particles speed or the wind speed in the saltation layer? Pomeroy and Gray (1990) proposed 2.8u*th as the average particle speed inside the saltation layer. However, if eq.6 is a mass conservation equation, where the quantity Ms is being advected by the flow, us should represent the wind speed. The wind speed in the saltation layer must be higher than the particles speed so that the particles are continuously accelerated. Do the authors consider these two quantities to be equal so that the mass in saltation is considered a passive scalar?

We indeed defined u_s similar to Pomeroy and Gray (1990), so it represents the particle speed. To make this more clear, we now write (see L198): “In our implementation, u_s represents the saltation particle speed and is defined as parallel to the 10m wind speed unit vector…” Since the wind speed concerns the speed of the molecules in the air, any quantity associated with that should indeed be advected with the wind speed (for example, temperature and humidity). However, the saltating particles do not travel with the same speed as the air molecules, but at a lower speed. This is due to the constant interaction of the particles with the surface, leading to decelerations and accelerations relative to the governing wind speed. So we cannot assume that the wind speed and particle speed are the same. So in this case, the quantity of interest is advected by the flow of saltating particles, not air particles, and we maintain that mass conservation is not violated in Eq. 6.

l.175-176: It is not clear if Alpine3D is ran for some time before the time period of interest in order to improve the initial state of the firn column. I suggest making it clear in the text.

Excellent point. Indeed we did not make this clear. Also the other review indicated that more explanation of the spinup procedure is required, which we added (see L240-L249). We do not run Alpine3D for any period of time before the analyzed time period (2015 - 2020). We have updated the text to the following in order to more clearly reflect this implementation (see L249-251):

“Alpine3D downscaling is then launched at the beginning of the analysis period (2015 in this study), meaning that although we initialize Alpine3D with a spun up firn column, its properties initially reflect the non-downscaled MERRA-2 climate.”

l.191: Please specify over what years was the annual average performed.

Unfortunately, answering this question uniformly is not possible. In simple terms, these observations are produced by counting the number of annual layers in the top 50 m of the firn column. Because annual layer thickness varies spatially, the number of years over which the average is performed also varies spatially. We’ve added the following sentence (see L270-272):

“This product intends to represent the annual-average SMB. However, because of the finite thickness of firn isochrones and spatially variable accumulation rates, the annual average is calculated over varying periods depending on the location.“

l.190-195: Can the authors say something about the uncertainty/accuracy of the snow accumulation predictions derived from the firn thickness?

The authors of the dataset do not provide quantitative uncertainty estimates for their annually averaged accumulation dataset. However, as stated in the text, the 25 km averages are set to that of MERRA-2. Thus, large scale uncertainties are controlled by MERRA-2.

That said, the authors do report relative accumulation errors that they describe as “negligibly small with a mean error of 0.002 (interpreted as 0.2%).” We have added the following to the text (see L272-273):

“No quantitative uncertainty estimates are assigned to the absolute SMB observations, however the authors report an average relative accumulation error of 0.2%.”

Fig.4 and 5: The observations signal (red line) seems the same in Figures 4c and 5c. However, the simulations correspond to different time periods (2015 only vs 2015-2020). Is the observation signal indeed the same? If yes, to what time period does it correspond?

Great question. Yes, the observational transect is the same in Figures 4c and 5b and represents a long-term annual average (i.e., comprising recent decades because this is a relatively high accumulation area and the product is built using radar soundings of near-surface firn). The exact time frame this transect corresponds to is unknown because 1) it is not reported by Dattler et. al. 2019 and 2) uncertainty associated with firn isochrone tracking. We revised the discussion in Section 2.7, following previous comments (see above), which we hope would also make this more clear.

l.225: The authors assume R-squared to be a proxy of the variance explained. As this is not accurate for all distributions, I suggest the authors to justify this assumption.

Thank you for bringing this point to our attention. We now state in the manuscript that we assume that the modeled and observed SMB are linearly related. Since we use Ordinary Least Squares to perform the regression analysis to calculate R^2, we also can assume that the mean of the residuals is zero, such that R^2 corresponds to variance explained. We revised this in the manuscript (see L302-304).

l.245: Is the discrepancy between the Alpine3D and the MERRA-2 transect results completely explained by snow being drifted from the analysis domain to the 15km border? It is not clear why the comparison between Alpine3D and MERRA-2 along the whole analysis domain is directly related to the comparison between these two models along the transect.

You raise an excellent point. Thank you. Indeed the discrepancy between mean Alpine3D and MERRA-2 averaged over the analysis domain is largely explained by the divergence of drifting snow out of the analysis domain and into the 15 km border, which leaves less mass available for accumulation along the transect. To make this more clear, and to also point to the possibility of net divergence in the area due to the large scale wind field, we revised the sentence to (see L324-327):

"The discrepancy between MERRA-2 and Alpine3D along the transect is most likely explained by net divergence of saltating snow out of the analysis domain (Fig. 2), resulting in less mass available for accumulation along the transect. Since wind speeds generally increase from the top right to the bottom left over the model domain, some net divergence over the transect also may occur."

l.255-256: In the manuscript, surface mass balance variability is completely attributed to snow redistribution. Even though it is well known that snow redistribution plays an important role, it would be interesting to compare the outputs of the Alpine3D downscaling with and without the snow drift model. This would isolate the effect of snow redistribution from the effect of spatially varying heat fluxes.

Agreed. The other reviewer likewise raised this concern and we provide the same response here. In the revised manuscript we include an Alpine3D simulation for the year 2015 without horizontal snow redistribution (see newly added Fig. 10). In the revised manuscript, we discuss the effect of simulated snow redistribution on simulated SMB variability, over variability caused by other processes (see newly added Section 3.7, L397-405).

l.265: It would be interesting if the authors could present how the process of wind-driven compaction is modeled in Alpine3D. For example, are the properties of deposited snow prescribed? Or do they depend on the properties of the previously eroded snow?

This is an appropriate request and in line with a question from Reviewer 2. In order to answer your question as well as improve the readability of the manuscript, we have added the following text (see L164-168):

“Following erosion and subsequent redeposition, several snow microstructural properties are updated in SNOWPACK according to the "redeposit" scheme presented in Keenan et al., 2021. For example, the density of redeposited snow layers are parameterized according to wind speed (Eq. 4 in Keenan et al., 2021) while sphericity and dendricity are both set to 0.875. The grain radius and bond radius are set to 0.2 mm and 0.05 mm, respectively while albedo is defined by Eq. 7 in Groot Zwaaftink et al. (2013).”

Although certainly of some interest to this paper, because these developments have been published elsewhere, we believe that the full details of SNOWPACK’s microstructural implementation are best left out of this manuscript. For this reason, we refer readers to Keenan et al. (2021) and Groot Zwaaftink et al. (2013) for further explanation.

l.317: The definition of SMB is not very clear in the Conclusions. The definition presented in the Introduction is more accurate. Please consider rephrasing.

Good suggestion. Noteballly, this comment is consistent with the other reviewer’s suggestion. We have revised to (see L408):

“The primary way ice sheets accumulate mass is through net snow accumulation at the ice sheet surface. SMB quantifies the balance between processes which accumulate and ablate mass at the surface of ice sheets.”

Technical corrections:

l.44: The word "recent" is doubled.

Fixed. Thank you (see L63).

l.64-67: I suggest referring to the respective sections as in lines 60-64.

Good catch. Implemented (see L92-93).

l.69: I suggest revising the need for section 2.1. The content of this section is mainly an introductory paragraph of section 2. Hence, it can be included below the title "Methods" without the need for a new subsection.

Thank you for the suggestion. After careful consideration, we have decided to keep the subsection title as we believe it will be useful in guiding readers who are interested in finding a quick methodological summary.

Figure 1: It would be interesting to add the remaining applications to the scheme (e.g. MeteoIO, WindNinja, ICESat-2, MERRA-2).

Thank you for this suggestion, which we incorporated in the updated Figure 1.

l.91: Consider replacing "off" by "on".

Fixed. Thank you (see L121).

l.100: Consider replacing "cheaper" by "computationally lighter".

Good feedback, thank you. We have updated it to “computationally cheaper” (see L130).

l.120: Consider replacing "layers" by "snow layers".

Implemented. Thank you (see L151).

l. 135: Taking into account that Alpine3D is more than a wind redistribution module, this title might mislead the reader. Consider removing "Alpine3D:".

Agreed, even though we still think it is important to make clear at this point that the section describes the workings of Alpine3D. Therefore, we have rephrased the subsection header to: “Numerical treatment of deposition and erosion in Alpine3D” (see L191).

l.165: I suggest considering the option of moving subsections 2.5-2.7 to a new section. It can be called "Case Study", for example.

Thanks for bringing this idea to our attention. Although these three sections could certainly be considered descriptive of a single case study, we believe that they provide unique information that benefits from their granular heading. For this reason, we respectfully keep the 2.5 – 2.7 subsections.

l.184: Even though the meaning of "efficiency" is clear in the text, please keep in mind that it has a specific meaning in high performance computing (see parallel efficiency).

Good point. To avoid confusion, we have revised to “computational speed” (see L259).

l.184: I suggest writing all numbers in a consistent way: either delete the comma in number 27126 (l.182) or add it in number 1130 (1,130).

Good point. We now use commas in both cases (see L259).

l.185 and 187: Taking into account that Alpine3D includes SNOWPACK, I believe it is not vary precise to talk about "SNOWPACK and Alpine3D" in this context. Consider replacing by Alpine3D only.

Thanks. We have adopted this suggestion (see L259 and L261).

l.187: Considering replacing "cheaper". What about "computationally less expensive"?

We have adopted your suggestion and replaced “cheaper” with “computationally less expensive” (see L262). Thanks for pointing out this opportunity for improved clarity.

l.232: The word "decreasing" is misspelled.

Fixed. Thank you.

l.243: It is not very clear what is the "2015-2020 period" and the "long term average". Please consider rephrasing.

Good point, we are now more explicit after revising to (see L320-323):

“Furthermore, it is worth noting that over the length of the transect, MERRA-2 2015-2020 mean annual SMB exceeds that of observations (504 and 461mm w.e. yr^{-1}), indicating that the 2015-2020 simulated SMB exceeded the 1980-2017 MERRA-2 mean annual SMB (Section 2.7).”

l.268: Is it R or R-squared?

Indeed it is R. We report R instead of R-squared to make clear the positive correlation.

l.339: Consider adding "that" after "shown".

Added (see L430).

l.347-348: Did the authors consider adding the model to the gitlab of Alpine3D?

Thank you for the suggestion. Although certainly worth considering, we have decided not to because we have already made our version entirely open source (https://github.com/snowpack-model/snowpack/tree/driftingsnow) and we do not control the SLF gitlab source repository.

Reference used here, but not cited in manuscript:

Groot Zwaaftink, C. D., Mott, R., and Lehning, M. (2013b), Seasonal simulation of drifting snow sublimation in Alpine terrain, *Water Resour. Res.*, 49, 1581– 1590, doi:10.1002/wrcr.20137.

Review #2 - Charles Amory

General comments

This paper describes a new computational chain for downscaling the surface mass balance (SMB) over ice sheets, by combining the snowpack model SNOWPACK driven by MERRA2 reanalysis with an offline coupling between the wind downscaling model WindNinja and the wind redistribution model Alpine3D. This numerical design, while maintaining attractive computational costs, provides promising results by partially resolving the spatial variability in SMB due to wind redistribution of snow along a 130-km long transect in West Antarctica along which radar-derived SMB retrievals are available. Sensitivity to horizontal resolution and to a prescribed, yet influential parameter not constrained by observations, is explored. The proposed method is innovative and clearly fits with the scope of GMD, some limitations are addressed and the work is put in perspective of future developments. Furthermore, the study it concise and very pleasant to read. I think the paper deserves publication after the authors have addressed the following comments. I would particularly advise further discussion of the limitations of the modelling approach, notably regarding lacking suspension, atmospheric sublimation, feedbacks with the atmosphere and the attribution of the spatial variability in SMB to mostly saltation transport, in order to reach the level of rigour of the other discussion elements of the manuscript.

We thank Charles Amory for the positive feedback, and all the comments and suggestions. We improved the discussion on the limitations of our modeling approach, and also included some broader context of the results. Please find our detailed response to the issues raised below.

Specific comments

Abstract, L1: Snow accumulation is more the resultant of the SMB than a component of the SMB. The actual main components that lead to snow accumulation are snowfall, condensation/deposition and wind-driven snow deposition. Would you please reformulate a little to get this clearer?

Thank you for pointing this out. Indeed we agree that our formulation was rather imprecise. For improved clarity and readability, we have revised the first sentence to (see L1):

“Ice sheet surface mass balance describes the net snow accumulation at the ice sheet surface.”

L24-26: You should mention atmospheric sublimation as a source of surface mass loss when defining the SMB. Currently the definition in the first paragraph only describes wind redistribution.

We intended sublimation to be inclusive of both atmospheric and surface sublimation. But in retrospect it makes sense to make this explicit. We have revised the mentioned sentence in the first paragraph of the introduction to (see L20-24):

"Mass accumulation is composed of precipitation as well as condensation and deposition of atmospheric water vapor, whereas ablation processes remove mass from the ice sheet surface via meltwater runoff, both atmospheric and surface sublimation, and evaporation. Additionally, local SMB is influenced by wind-driven snow redistribution, which we refer to as deposition in the case of local mass gain and erosion in the case of mass loss (Lenaerts and van den Broeke, 2012)."

L39-40: Would this assertion still be valid knowing that blowing snow layers can extend up to hundreds of meters above ground in Antarctica (Palm et al., 2017)? I find this assertion actually quite questionable, and one can expect the ratio of saltating to suspend snow mass to depend a lot on the area considered, and then to be very specific to the local snowpack, topographic and atmospheric conditions, or to the boundary conditions of the experiments. I did not find the materials in the referenced literature necessary to support such a statement. Gromke et al. (2014)'s results, which are based on wind tunnel experiments of limited dimensions, would not account for the well-developed bowing snow layers of hundreds of meters in depth, in which transport in suspension most likely dominate over saltation. Beside, these numbers are actually not demonstrated by Gromke et al. (2014), but just mentioned in the introduction and borrowed from Kind (1990), with no reason to consider it universally valid, especially when translated into an Antarctic environment. Moreover, contrary statements can be found in the literature. See for instance Bintanja (2000) p345: "Most of the snow transport occurs when snow is in suspension, with the saltation transport becoming rapidly less important as wind speeds increase (Pomeroy and Male, 1992; Mann, 1998).". Please nuance and adapt your sentence accordingly.

We agree that with the reviewer's remarks in mind, our statement was not sufficiently nuanced. There is indeed research indicating that at high wind speeds, suspension seems to carry most of the mass. We now include those studies in the revised manuscript (see L54). We also note at this point that for example Mann et al. (1999) indeed concludes that: "Blowing snow mass transport whilst in suspension was shown to dominate over transport by saltation.". However, this statement was based on measurements in the suspension layer while applying the Pomeroy (1988) model for the saltation layer, while Melo et al. (2021) suggest that the Pomeroy model may underestimate mass transport in saltation. Thus, we feel that we can not confidently claim that also long-term average transport is dominated by suspension. Nishimura and Nemoto (2005) show measurements from Antarctica and calculated that the mass transport expressed in volume in the suspension layer is orders of magnitude smaller than in the saltation layer, which obviously can be compensated for by the possibly orders of magnitude deeper suspension layer at high wind speeds. An important aspect to note here is that the larger particles, which carry most of the mass, remain close to the surface. When the blowing snow layer extends more than 100 m in the atmosphere, the particle size at that elevation is very small and not much mass is transported, even though it could be picked up by remote sensing, or visually appear as a cloud of particles. We now provide more extensive discussion regarding this point, which we think is now also more balanced (see L46-59).

L44: “recent” appears twice.

Fixed, thank you (see L63)!

L80-84: How did you choose those lapse rates, particularly that of ILWR?

2 m air temperature: We aimed to choose a rough and sensible value. -6℃/km is close to many estimates of the atmospheric moist adiabatic lapse rate and is consistent with a bore-hole derived estimate of atmospheric lapse rate (-6.8℃/km) derived from the Antarctic Peninsula (Martin and Peel, 1978).

ILWR: The negative lapse rate is designed to capture the effect of decreasing air temperature with height. Our specific value of -31.25 W/m^2/km is borrowed directly from another Alpine3D based modeling study (Michel et al., 2022).

These references have been added to the revised manuscript (see L107-108 and L112).

L164: Another limitation that you might also consider to discuss is the net domain-integrated erosion-deposition balance equal to zero, due to the absence of transport off the continental margins in this approach, which might be of some significance at the coastal grid points over steep continental margins. I also recommend to elaborate on the expected consequences of 1) assuming that the saltation mass flux accounts for both the contribution of suspension and saltation and 2) not taking into account atmospheric sublimation and its influence on surface sublimation, at least qualitatively, and more particularly for continental-scale simulations for which these processes are believed to be responsible for significant ablation at the surface. We can expect several other processes (vertical advection and sublimation of suspended particles, local turbulence and synoptic wind not related to local topography) to contribute to the net erosion/deposition balance and thus to the spatial variability in

SMB. For instance, could they play a role in the discrepancy between modeled and observed SMB described in Sect. 3.2?

We thank the reviewer for the helpful suggestions to put our approach in a broader context of processes impacting the spatial variability of SMB. We expanded this section considerably to now better discuss this (see L220-227).

Section 2.4: Could you add to the text a recall on if and the, how, snow (microstructural) properties (sphericity, radius, bond radius, density, albedo) are altered by deposition of wind-driven snow?

We agree that the manuscript was missing important information, as also noted by the other reviewer. For this reason, we have added the following text:

“Following erosion and subsequent redeposition, several snow microstructural properties are updated in SNOWPACK according to the "redeposit" scheme presented in Keenan et al., 2021. For example, the density of redeposited snow layers are parameterized according to wind speed (Eq. 4 in Keenan et al., 2021) while sphericity and dendricity are both set to 0.875. The grain radius and bond radius are set to 0.2 mm and 0.05 mm, respectively while albedo is defined by Eq. 7 in Groot Zwaaftink et al. (2013).”

L174-177: One could expect more info on the initialization procedure of SNOWPACK here, not provided in Sect. 2.3. How long is the spin-up before the period studied? Does the initialization procedure also account for the possible influence of erosion and deposition on the firn column? You could also refer to Keenan et al. (2021) and mention the reasonable agreement found with observed firn temperature and density profiles to strengthen your argumentation there.

Thanks for this suggestion, which was also suggested by the other reviewer. We now include more detailed information about the spin-up procedure in this section, including some more detailed references to the Keenan et al. (2021) study (see L240-251).

Section 3.2: To evaluate the benefits of a newly-implemented parameterization, it is usually recommended to compare two simulations, one with and one without. By doing so with two Alpine3D downscalings, you could then quantitatively disentangle the contribution of erosion-deposition from that of SEB to the spatial variability in SMB, and attribute part of the enhanced variability and performance that is due to the modelled erosion-deposition process.

Agreed. The other reviewer likewise raised this concern and we provide the same response here. In the revised manuscript we include an Alpine3D simulation for the year

2015 without horizontal snow redistribution (see newly added Fig. 10). In the revised manuscript, we discuss the effect of simulated snow redistribution on simulated SMB variability, over variability caused by other processes (see newly added Section 3.7, L397-405).

L307: “Simulations results” of what ? please specify.

Upon consideration, we realized that this sentence does not make sense. Thus, we have removed it.

L316: This sentence could be rewritten to present a more correct definition of SMB. You could for instance refer to as “Net snow accumulation” to describe mass gain, as SMB is not necessarily positive.

Good point. We have adopted your suggestion and aim to be more precise with (see L407-408):

“The primary way ice sheets accumulate mass is through net snow accumulation at the ice sheet surface. SMB quantifies the balance between processes which accumulate and ablate mass at the surface of ice sheets.”

Conclusion: I think briefly recalling that atmospheric sublimation and suspension are both omitted in your calculation chain would make the conclusion more exhaustive.

Agreed. We now end the Conclusion section as follows (see L433-L436):

“Further, we note that our implementation makes important simplifications by neglecting drifting and blowing snow sublimation as well as horizontal redistribution by way of suspension. In regions where these processes are significant drivers of local SMB, future downscaling efforts would likely be improved by inclusion of these processes into Alpine3D.”

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