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Comment on gmd-2022-28

Anonymous Referee #1

Referee comment on "A wind-driven snow redistribution module for Alpine3D v3.3.0: adaptations designed for downscaling ice sheet surface mass balance" by Eric Keenan et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2022-28-RC1>, 2022

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.

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.

l.19-21: The terms drifting and blowing snow are used both to define the processes of aeolian 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 drifting 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."

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).

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

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.

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

I.121: Even though Φ has units of mass flux ($\text{kg/m}^2/\text{s}$), 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 L_y times L_x , where L_y is the width and L_x 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.

I.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 s^2/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.

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

I.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 saltation 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).

I.144 (eq.6): I believe the variable u_s 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 M_s is being advected by the flow, u_s 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?

I.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.

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

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

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?

I.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.

I.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.

I.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.

I.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?

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

Technical corrections:

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

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

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.

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

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

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

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

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

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.

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).

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).

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

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

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

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

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

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

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

References (only those not present in the manuscript):

Comola, F., Gaume, J., Kok, J. F., & Lehning, M. (2019). Cohesion-induced enhancement of aeolian saltation. *Geophysical Research Letters*, 46, 5566–5574. <https://doi.org/10.1029/2019gl082195>

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Vionnet, V. (2012). Etudes du transport de la neige par le vent en conditions alpines: observations et simulation à l'aide d'un modèle couplé atmosphère/manteau neigeux. Ph.D. thesis, Sciences et Techniques de l'Environnement, Université Paris-Est, France, <http://tel.archives-ouvertes.fr/tel-00781279>

Vionnet, V., Martin, E., Masson, V., Guyomarc'h, G., Naaim-Bouvet, F., Prokop, A., et al. (2014). Simulation of wind-induced snow transport and sublimation in alpine terrain using a fully coupled snowpack/atmosphere model. *The Cryosphere*, 8(2), 395–415. <https://doi.org/10.5194/tc-8-395-2014>