We thank both referees and the editor for their constructive comments which will improve our manuscript. We regret that Referee #2 does not fully appreciate the added value of our work but we believe this is mainly because they would have expected a traditional scientific paper including both model description and extensive evaluations in the same paper, whereas our strategy was (1) to use the opportunity offered by GMD model description papers to provide a more accurate model description than in publications in other journals, and (2) to extend model evaluations in future papers. We understand from the editor recommendations that a compromise has to be found. Therefore, we tried to do our best to account for Referee #2 last comments and include new evaluations of our model outputs. Thus, we chose to include in the revised paper a 2-dimensional evaluation of simulated snow depth against a satellite-derived snow depth map. This preliminary evaluation is complemented with a paper recently submitted to The Cryosphere and providing more robustness in the associated conclusions (Haddjeri et al., 2023). Taking into account the editor's recommendation for further conciseness in the manuscript, we chose to move our study about of the numerical performance of SnowPappus to the Appendix. Although this is an important component of the applicability of the model at large scale, the consistency of the whole paper is not affected by this choice. We also made other small adjustments to avoid increasing the length of the paper despite the new material requested by Referee #2. We hope that the editor and referees would find our manuscript now fully fills the requirements of GMD, and that the publication process can follow its way so that ongoing works based on SnowPappus will have the possibility to be published with reference to a high quality model description paper.

Our detailed response to both referees reports are given below. Author comments are in black and reviewer comments are in **violet**. When we present text passages from the new revised manuscript, they appear in "*italics*". When we refer to a Sect., Fig. or line number, it refers to the version of the manuscript we will resubmit along with this response (the Third version). Second version of the manuscript will be referred as "the second version")

Report #1 (Anonymous referee #2)

The authors presented a thoroughly revised manuscript. However, the authors decided not to modify the manuscript considering one of my main criticisms. They have decided to maintain the full storyline introducing a 2D drifting snow model framework, while still refraining from providing any validation for that part. I cannot find good arguments to do that, because what is the value of the manuscript for the broader scientific community in that case? Even when comparing with field data is difficult, still some kind of validation would be required in my opinion.

As explained in our previous response, the 2D evaluations of Crocus-SnowPappus were intended to be presented in a separate paper due to the need for detailed geospatial analyses of the results and to explore the robustness of the analyses to different precipitation inputs. As this additional paper (Haddjeri et al., 2023) would have met the reviewer's expectations, we initially preferred not to include the 2D evaluations in this GMD article to favour in-depth presentation of 2D evaluations. However, considering the new arguments provided by the reviewer and editor, we decided to include a preliminary 2D evaluation of SnowPappus in this study. We also refer to Haddjeri et al., 2023 in the revised version of the manuscript to extend the scope of the conclusions. We also took into account the reviewer's comment about our lack of assessment of sublimation impact on simulations (see below).

Therefore, in the revised manuscript, the part of the article relying on 2D simulation now includes (i) a sensitivity analysis comparing simulated snow depth at the end of accumulation season with three simulation set-ups (CTRL: no transport, no sublimation, TRANS: transport, no sublimation, TRANS+SUBL: transport, no sublimation) and (ii) a comparison of simulated snow depth with CTRL and TRANS simulations with observed snow depth obtained by state-of-the-art stereo-imagery from Pléiades satellites (Deschamps-Berger et al. 2020), which covers approximately 150 km2 in our test zone. The spatial correlations between observed and simulated snow depth and snow depth distributions above 2700 m are also compared.

The main new results (Fig. 12 and 13) are (i) blowing snow sublimation has much less impact on simulations than blowing snow transport (ii) Wind-induced snow transport enhances snow depth variability at high elevation, making simulated snow depth distribution closer to observations (iii) The spatial correlation between observed and modelled snow depth is significantly improved, although a large part of the observed variability remains unexplained and SnowPappus may overestimate erosion/deposition near high alpine ridges. These results demonstrate a significant added value of blowing snow simulation with SnowPappus. To our knowledge, it is the first study to demonstrate quantitatively an added value of a wind-induced snow transport model on snow spatial patterns for a complete winter season at 250 m resolution and in complex terrain. Two studies had however been conducted at 30-50 m resolution (Bernhardt et al., 2012; Vionnet et al., 2021).

However these results raise many additional questions, including the likely superposition of precipitation patterns errors and need to be strengthened by the use of more images. As explained above, these questions are addressed in a separate paper (Haddjeri et al., 2023) where a detailed sensitivity analysis of spatialized snow simulations to precipitation forcing, blowing snow representation, and model resolution is provided. This study shows that both components highly interact in any evaluation of 2D simulations and that results ignoring these uncertainties should be considered with caution. However, the added value of SnowPappus to simulate the spatial variance of snow depth and snow melt out date at high elevations and around crests is confirmed by Haddjeri et al., with more satellite observations than the ones used in this paper.

In order to include this new work in the manuscript, Sect. 4 (Methods), 5(Results) and 6(Discussion) are re-organized to describe the new methods, results and discussion associated with these 2D evaluations. Besides, to avoid making the article longer than it already was, the description of numerical performance (Sect. 5.7 in the previous version) is moved in appendix, and various small unnecessary text passages are removed, including Sect. 5.3 in the second version, which gives an illustration of 2D simulation output (Sect. 5.3 in the second version). The new outline is given below, with the included modifications highlighted in **bold**:

<u>4. Evaluation : Methods</u>
[UNCHANGED START OF SECT. 4]
<u>4.3 Evaluation data</u>
Description of point-scale flux measurements
Description of Pleiades satellite snow depth maps
<u>4.4 Point-scale evaluations</u>
<u>4.4.1 Local simulations set-up</u>
<u>4.4.2 Evaluation of blowing snow occurrence</u>
<u>4.4.3 Evaluation of blowing snow fluxes</u>
<u>4.5 2D evaluations</u>
2D simulation set-ups
methods of 2D evaluations

<u>5. Results</u>
[UNCHANGED START OF SECT. 5]

5.3 Evaluation of blowing snow ocurrence at Col du Lac Blanc
5.4 Evaluation of blowing snow occurrence in 2D simulations
5.5 Evaluation of blowing snow fluxes at Col du Lac Blanc
5.6 Sensitivity analysis and evaluation of 2D simulations
Comparison of the effect of blowing snow transport and blowing snow sublimation activation on simulated snow height, illustrated with Fig. 12
**Comparison between simulated and Pleiades-observed snow heights
, illustrated with Fig. 13**

6. Discussion

6.1 Point-scale blowing snow flux and occurrence computations
6.1.1 Quality of Point-scale flux prediction and comparison with other studies
6.1.2 Sensitivity, added value and robustness of microstructure-dependent parameterizations
6.1.3 Uncertainty on the used parameterizations

6.2 Use of SnowPappus in distributed simulations
6.2.1 Added value of SnowPappus in 2D simulations
6.2.2 Sources of uncertainty and limitations of the study
Additional discussion from 2D evaluation
6.3 Limits of applicability
6.4 Applicability at large scale

A suggestion I thought of when reading the revised manuscript is to compare the simulated snow cover with simulations using another model, such as SnowModel/SnowTran-3D, or SnowDrift3D. Or the SYTRON or Crocus-Meso-NH (mentioned in L56) schemes that are apparently available in Crocus. Then at least readers would get some understanding of how well the model performs compared to other models.

Performing a model intercomparison would be beyond the scope of this paper which already contains additional 2D evaluations. It is important to mention that SYTRON and Crocus-Méso-NH could not be applied for these comparisons because (1) by design SYTRON can not run on 2D domains (Vionnet et al. 2018), (2) Méso-NH can not be run over a full snow season due to its very high numerical cost. Applying SnowModel/SnowTran-3D would be possible but this comparison would raise many other questions due to the major differences between the snow schemes themselves, again beyond the scope of this publication.

The problem is now as a reader, I just have no idea if this is a useful model framework. It is also important in this context that most, if not all parameterizations and numerical schemes were implemented from other studies. Obviously there is nothing wrong with learning from, and building upon existing literature, but it means that the only trust I can have in SnowPappus comes from the fact that it is so heavily based in existing literature. But that means that the real value right now is in these other studies, not this particular one.

We would like to highlight again that our work presents several new elements compared to previous works. In terms of threshold wind speed for transport, we propose and evaluate a modification of the threshold wind speed formulation developed by Vionnet et al. 2013. In terms of saltation flux, we use the Pomeroy et al. 1990 and Sorensen 2004 formulations only after a precise analysis of the discrepancy between both formulation, whereas previous studies only used one or the other without further justification (Liston et al. 1998, Gallée et al. 2001, Vionnet et al. 2014) or revealed this discrepancy without proposing detailed explanation for it (Doorschot et al. 2002, Melo et al. 2021). Finally, to our knowledge we are the first snow transport model to include a dependence of the terminal fall speed of suspended particle on snow microstructure. Although this work is based on already published experimental studies, we proposed a new parameterization to fill in the gaps in knowledge and calibrated it to new observational data. Furthermore, We show the strong impact it has on the suspension flux, showing that neglecting this dependency could have important effects on the performance of other models. We thus strongly believe that there is a scientific value in this study, which is further strengthened by the inclusion of 2-dimensional evaluations in the new version of the manuscript.

Furthermore, I also mentioned that the option for drifting snow sublimation is introduced, but it's not clear at all how this impacts the simulation. The authors don't seem to have done anything with my comment.

We apologize if we did not initially understand the reviewer's request. The available data do not allow providing evaluations of sublimation fluxes but we can indeed show the impact of this parameterization between 2 simulations. As mentioned above when describing new 2-dimensional evaluations of SnowPappus, we now present the difference between snow depth simulated with TRANS and TRANS+SUBL. This result is shown in Fig. 12b.

I would like to briefly provide feedback on three arguments I could distill to not provide further validation:

1) Methodological challenges obtaining and comparing snow depth data.
As I pointed out, it is also possible to compare with other existing models. Fig. 8 shows up to and over 4m of depositions at the lee-side of ridges. Is that realistic? When the resolution of Fig. 8 is 250x250m, that means that there is a lot of additional accumulation in the lee side. I'm not convinced that that is realistic. Locally behind ridges, corniches can form, or maybe some small patches that fill in with high accumulation. But I'm not sure that it is realistic that on a scale of 250x250m, that there is so much additional accumulation. Similarly, up to 4m erosion, or even more, is simulated in certain areas, which also comes across as excessive over such large grid cells. For example, Fig. 9c in Mott et al., 2010 (doi: 10.5194/tc-4-545-2010) shows that accumulations up to 4m only occur on scales much smaller than 250x250m.

As explained above, comparison with existing models is not so obvious, not possible with all models and beyond the scope of this paper.
Then, the reviewer seems to have misinterpreted our results. In fact, Fig. 8a indicates maximum erosion/deposition of 400 kg/m2 of snow over one year of simulation. At common snow densities, this would correspond to snow depth of 1 to 3 m, but certainly not 4 m. The orders of magnitude obtained in Mott el al. 2010 are not comparable with ours because in our case, the magnitude of erosion is obtained after the whole winter while the study of Mott et al. is performed on a much shorter time period (a single blowing snow event). A more comparable study would be the one of Vionnet et al. 2021 with CHM-PBSM3D where wind-induced snow transport effect can be of more than 2 meters over areas of several hundreds of meters, which is comparable to our results. In the new discussion section discussing the added value of SnowPappus in 2D simulation, we will mention this study by stating:

"*Two-dimensional simulations on the Grandes Rousses test zone (see Sect. ...) showed that activating snow transport has a significant influence on snow height spatial distribution at high elevation at the end of accumulation season, reaching up to 2-3 m of erosion/deposition near high alpine crests. Comparable snow height differences where obtained with PBSM-3D (Vionnet et al, 2021).*"

2) From the author's response: "Due the same methodological challenges, the corresponding publications did neither describe evaluation of the simulated spatial patterns of snow depth or surface properties (Gallée et al., 2001; Amory et al. 2021; Sharma et al., 2021)."
Regarding Amory et al. (2021), they in fact do show an evaluation of surface mass balance with the drifting snow enabled MAR (see Fig. 10 in that paper). Sharma et al. (2021) discusses the differences between the default surface scheme NoahMP and the newly introduced scheme SNOWPACK. Sharma et al. (2021) also additionally shows for example the differences between simulations with and without drifting snow sublimation. So, in my opinion, these examples actually provide some inspiration of how credibility can be given to the model development in the manuscript.

The comparisons between CryoWRF and NoahMP in Sharma et al. 2023 only include evaluations of atmospheric variables such as atmospheric humidity or temperature, which are not direct transport evaluations, on a dozen of point stations. This cannot be seen as an 'evaluation of the simulated spatial patterns of snow depth or surface properties'. In fact, Sharma et al. 2023 does not include any evaluation of wind-induced snow transport, and has recently been accepted in GMD. We believe that this paper do provide insights for the community despite the lack of snow transport evaluations.

However, we did miss a spatial evaluation of snow mass balance conducted on a transect in Antarctica by Amory et al. 2021 and thank the reviewer for pointing this out. This evaluation is carried out on one transect with regular measurements of surface mass balance. Nevertheless, this brief evaluation of a snow transport-related 1D spatial pattern does not dismiss our arguments that a broader study is needed to properly address the issues of spatial evaluation of blowing snow modelling (robustness of conclusions with meteorological forcing, detailed spatial analysis with topography, etc.). This is the case thanks to our recently submitted paper (Haddjeri et al., 2023). A first overview of the results is now presented in the current manuscript and appropriately discussed in light of the most extensive evaluations of Haddjeri et al.

3) The uniqueness of the drifting snow mass flux observations as validation data.
Here I would like to recall that I wrote in my review: "However, if the only goal is to represent drifting snow mass fluxes, it would be necessary to evaluate if a 2D/3D approach is really necessary, or if simply the 1D approach, calculating mass fluxes based on snow cover properties and wind speed is sufficient (i.e, applying Eq. 22) to reproduce that." Later on I wrote: "I think either the focus needs to be on the concentration profiles at 1D simulations, and compare those with observations."
I cannot find a response to that point in the response document. It's still not clear to me if a 2D model is required to reproduce the observed mass fluxes, or if a 1D approach would already yield satisfactory results.

We believe that the goals of our modelling framework are now clearly explained in the introduction of our manuscript. Obviously the intent of SnowPappus is to be applied on 2D domains and to be able to simulate erosion / accumulation which is not possible with 1D approaches. However, it is still useful to check whether 2D models are able to simulate realistic drifting snow mass fluxes at the local scale. Indeed, this is a more direct evaluation than 2D erosion / accumulation patterns which cannot be easily disentangled from other processes explaining the observed variability in snow depth. As mentioned above, we decided to include a 2D evaluation of the model, but we believe that the improvements shown in the 2D simulations are highly strengthened by the confidence in the simulations of blowing snow fluxes demonstrated in our paper (i.e. improvements obtained for a good reason because the realism of the physical process has been checked).

Report #2 (Anonymous referee #1)

I think that this revision is adequate on the large review questions, but I still have many minor comments.

We thank Referee 1 for the improvements they have noticed on our revised manuscript and for the last comments which keep improving our manuscript.

80 (and throughout)
units should not be in italics

fixed

140 (and throughout)
Use exponents rather than vertical inline equation.

In order to make it easier to read, the equations written in the text L140 and 141 were rewritten as an equation separated from the text (Eq. 3).

141
friction velocity not yet introduced

fixed

234
If this were "the main novelty introduced in SnowPappus", it would not merit publication

We apologize for this incorrect formulation which was only referring to the novelties introduced in the threshold wind speed computation. We will replace "the main novelty introduced in SnowPappus" by "*a novelty introduced in SnowPappus*".

253
value of z0 stated twice

fixed

264
use ln for natural logarithm

fixed

312
T is Ta elsewhere, u_wind is U

T_a was replaced by T and u_{wind} by U where it appeared

316
5 m stated twice

fixed

347
GMD style is vectors in bold italics

fixed

395
14443 250 m grid cells do not make 3200 km^2

fixed (changed towards 900 km²).

463
What is "was calibrated provide" intended to mean?

We corrected this expression by "was calibrated to provide"

468
How is d_m = 0 possible?

In fact in this case the factor F defined L278 is equal to 1, although the expression we gave is singular. We replaced the F expression L278 by:

$$F = [\max(1, \frac{d_m}{d})]^{-1}$$

It is equivalent in the case d_m>0 and works for d_m =0 (given we define F only when d>0)

494
Equation 26 could be replaced by reference to equation 13

fixed

Figure 7 caption
.144 should be superscript

fixed

Figure 11c does not show anything.

We understand the reviewer emphasizes that simulated and observed wind speeds do not exhibit a good agreement at Chambon station (FCMB) compared to the other stations. We believe this result must be shown as the difficulty to simulate realistic small scale wind speed largely explains the difficulty to simulate snow transport as discussed in Section 6.2 of the revised manuscript. To account for this comment we rephrased as follows the description of this result:

"It suggests that the accuracy of the downscaled wind speed and/or the 250 m spatial resolution of the simulation are the main causes of the skill deterioration, as confirmed by the significant discrepancies between observed and simulated wind speeds at the three stations (Fig. 11), with a variable skill between Col du Lac Blanc (R^2=0,71, RMSE=3,3 m/s), Huez (R^2=0,49, RMSE=2,5 m/s and Chambon(R^2=0,42, RMSE=3,0m/s) stations and a significant underestimation of the highest wind speeds at all sites."

In addition, we noticed a small mistake in Fig. 11b and c of the second version: wind speeds were not evaluated on the same time period as the blowing snow occurrence evaluation (from 01/12/2018 to 04/01/2019). We corrected this issue, which explains the small difference between the old and new figures. However this correction does not affect our conclusions.

Figure 12 needs space between a and b.

fixed

571
u* is not wind velocity

fixed

Figure A2
Snow3l not explained

We apologize for this missing definition. 'Snow3l' was changed in 'full snow routine' which is defined in main text

Figure A3
Shading for times with no valid observations is not explained

The legend of Fig. A4 (Fig. A3 in the second version) was completed to explain the shading.

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