We thank both referees for their constructive comments. In the following document, detailed responses to every referee's comments are presented point by point, and modifications to the manuscript according to their suggestions are proposed. Notably, both referee's comments led us to deeply reorganize the manuscript and reduce its length by approximately 20%. We also bring a detailed and reasoned response to the concerns raised by the referee#2 about the relevance of the point-scale flux evaluations and need for 2D evaluations.

In the following, author comments are in black and reviewer comments are in **blue.** When we present text passages from the revised manuscript, they appear in "*italics*".

1 . Response to Referee #1's comments and associated manuscript modification

There are many 1D models of blowing snow, several 2D models (operating on decametre grid scales as far back as the 1990s), and even 3D coupled snow-atmosphere models. An operational model that can be run on large scales is certainly of interest. The authors give a fair summary of previous work and a nearly complete description of the model being introduced.

We first would like to thank the reviewer for their constructive comments. In the following, we will give a detailed answer to each of them.

The paper is very long, however, for the amount of original material being presented; some of this is due to repetition and poor organization of material.

We understand the major concerns of both reviewers about the length and lack of clarity of the paper. In order to address this issue, we propose to extensively modify the structure of the paper to improve its readability. We will split Section 2 (Methodological choices) into two separate sections. The first one will be dedicated to a literature review largely reduced in length by focusing on the topics for which an added value is provided in SnowPappus compared with existing works. It includes discussions about the representation of saltation flux and lower boundary condition for suspension flux and terminal fall speed parameterizations. This will allow to make the logics of the manuscript clearer. For processes that are simply implemented in SnowPappus following existing models or previous literature, the literature overview will be considerably shortened, or even suppressed if it is not useful for the remaining part of the article, such as the discussion on sublimation (L449-452). Then, a second section will be dedicated to a concise model description without any theoretical interruptions between the description of the different implementations. We believe this organization will help the reader to understand more quickly our modelling choices.

In addition, several parts of the manuscript will be strongly shortened, primarily the description of numerical performance, various repetitions and unnecessary information or discussion will be withdrawn and a more concise writing style will be adopted when possible. Besides, the modification of the paper structure, which splits the section "Methodological choices" into 2 parts do not result in additional length, as the number of subsubsections does not changes a lot and repetitions will be avoided. Overall, the planned changes will allow to suppress Fig. 15 and move Fig. 14 in the appendix and to reduce overall text length by 20% (without taking appendix into account).

In the following, we will first list the text passages that will be strongly shortened, or withdrawn, in their order of appearance and identified by their section number and if necessary line number in the preprint. Then, we present a revised text outline to apply the described changes. New (sub)sections are written with their titles underlined, and there contains in plain text. Parts of the outline in which the organization remain identical with the preprint's outline are marked as UNCHANGED, although some length reductions have been also applied in these parts.

shortened text passages (with paragraph and line number from the initial manuscript):

2.1 Target, opportunities and constraints
2.2.2, wind profile
2.3.2 Blowing snow trajectories and transport modes
2.3.4 L257-263 discussion on the influence of particle size distribution on the suspension transport
2.3.3 L207-241 - Different types of transport models
2.3.7 L334-358 equations of simple saltation parameterizations P90 and S04
2.3.8 Influence of fetch distance
2.5 L502-508 Influence if snow transport on snow surface properties (state of the art)
3.2 L 560-570 Wind downscaling description
3.5 L 623-634 Methods for blowing snow occurrence measurements
4.6 L734-739, domain decomposition for parallel computing
5.1 L799-805 Discussion on the outlier in blowing snow fluxes evaluation

withdrawn text passages :

2.4 L449-452, Sublimation
4.5 L712-717, Evaluation of blowing snow fluxes at *Col du Lac Blanc*, discussion on the outlier (repeated in the discussion)

new outline in the revised manuscript :

1. Introduction

General introduction followed by the target, opportunities and constraints of the development of SnowPappus (corresponds to Sect. 2.1 in the preprint)

2. Blowing snow flux computation : state of the art

2.1 Blowing snow occurrence

Useful theoretical background for blowing snow occurrence detection (Sect. 2.2.1 in the preprint)

2.2 Horizontal blowing snow fluxes

2.2.1 Notations and geometric considerations

2.2.2 Blowing snow particle trajectories and transport modes

State of the art on the trajectories of blowing snow particles and transport modes, focused on saltation and suspension (corresponds to Sect. 2.3.2 in the preprint)

2.2.3 Suspension transport modelling

-Existing types of suspension transport models (part of Sect. 2.3.3 in the preprint)
-Literature review on the effective terminal fall speed (mainly informations in Sect. 2.3.5 of the preprint)

2.2.4 Transition between saltation and suspension

- The way it is treated in other models
- State of the art on this transition zone
- Problems of definition of the lower boundary condition for suspension transport (corresponds mainly to Sect. 2.3.6 of the preprint)

2.2.5 Simple saltation models

-Short description of Sorensen et al., 2004 (S04) and Pomeroy et al., 1990 (P90) saltation parameterizations

-Discrepancies between S04 and P90 and discussion of the possible causes (informations in Sect. 2.3.7 of the preprint)

3. Model description

3.0 Crocus description

Very short description of Crocus snow model

3.1 Blowing snow occurrence

Equations of the different options implemented in SnowPappus for threshold wind speed for transport (Sect. 2.2.3 in the preprint)

3.2 Horizontal blowing snow flux

3.2.1 Suspension transport

- Reasons for the choice of the model type
- Logarithmic wind speed profile (Sect. 2.2.2 in the preprint)
- Equations for suspension transport in SnowPappus (corresponds to Sect. 2.3.4 in the preprint)
- Parameterization of terminal fall speed used in SnowPappus (informations in Sect. 2.3.5 of the preprint)
- Maximum height of suspension transport as a function of fetch distance (part of Sect. 2.3.8 of the preprint)

3.2.2 Saltation transport and transition with suspension

- Description of the two options (using S04 and P90) of flux computation in the saltation zone and the transition with suspension in SnowPappus (included in Sect. 2.3.7 of the preprint).
- Influence of fetch distance on saltation transport in SnowPappus (included in Sect. 2.3.8 of the preprint, with Fig. 3 appearing)

3.3 Sublimation

Sublimation options in SnowPappus

3.4 Mass balance

Mass balance implementation in SnowPappus

3.5 Influence of snow transport and deposition on snow surface properties

- Properties of deposited snow
- Blowing snow induced snow metamorphism

3.6 Implementation in SURFEX

How SnowPappus is included in SURFEX code (mainly Sect. 2.7 of the preprint, but includes the description of domain decomposition for parallel computing)

4. Evaluation : methods

UNCHANGED (with length reductions)

5. Results

mainly UNCHANGED but addition of 1 paragraph (see below) and Fig. 14 moved in appendix, as partly redundant with Fig. 13 .

5.1 Comparison of saltation parameterizations

Comparison of blowing snow fluxes obtained with S04 and P90 implementations in SnowPappus and implications for the comparison of S04 and P90 (corresponds to the end of Sect. 2.3.7 in the preprint).

6. Discussion

UNCHANGED (with length reductions)

7. Conclusion

UNCHAGED

Having said that "Blowing snow occurrence evaluation showed SnowPappus performs as well as a currently operational scheme" in the abstract, there needs to be a statement about the need and benefits for SnowPappus.

We agree with the reviewer that the needs and benefit of SnowPappus were insufficiently stressed out in the abstract. We thus propose to replace L 2-4 by "*Thus, the evolution of operational snow modelling systems towards 100-500 m resolutions requires representing this process at these resolutions, over large domains and entire snow seasons. We developed SnowPappus, a parsimonious blowing snow model coupled to the Crocus state-of-the-art snow model able to cope with these requirements.*"

Besides, the SYTRON operational scheme was indeed already available to simulate wind-induced snow transport coupled to Crocus, but it is designed to work on idealised geometries, where an altitudinal band of a mountain range is represented by 8 slopes with 8 orientations, where snow can be transported from one slope to the opposite one (Vionnet et al., 2018). Thus, this model is not suitable for distributed simulations. It is however used operationally for the detection of blowing snow occurrence. To clarify the added value of SnowPappus compared to this scheme, we propose to make less ambiguous the above-mentioned abstract phrase by changing "Blowing snow occurrence evaluation showed SnowPappus performs as well as a currently operational scheme" in "*Evaluations showed SnowPappus performs as well as currently operational scheme SYTRON in terms of blowing snow occurrence detection, while the latter does not give access to a spatialized information*" .

Moreover, mention to SYTRON and its difference with SnowPappus will be discussed briefly in the end of the introduction, and the goal and strategy of the paper will be clarified. Given our reorganisation of the end of the introduction (see our response on manuscript length and organization), we present below the whole end of the introduction in the revised manuscript, and highlight in **bold** the sentences that address specifically these two issues.

"*In the above-mentioned context of increasing resolution of snow modelling systems, the long-term project of CNRM aims at performing simulations with Crocus at the scale of the French Alps at 250m resolution in an operational purpose, associated with a data assimilation framework requiring ensemble runs of 50-100 members (Largeron et al., 2020; Cluzet et al., 2021). The 250 m resolution allows a trade-off between the need for precisely representing slopes and aspects, influencing mass and energy balance of the snowpack, and the expected computational cost. In this context, a numerically efficient representation of wind-induced snow transport that can be coupled to Crocus simulations is lacking while this is necessary to better account for its impact on avalanche forecasting over French mountains.* ***Two blowing snow scheme coupled with Crocus exist yet : SYTRON (Vionnet et al., 2018) and Crocus-Meso-NH (Vionnet et al., 2014). However, both are unadapted to this geometry and resolution.***

Thus, the goal of this paper is to describe and present first evaluations of a novel blowing snow scheme, SnowPappus, coupled to Crocus and able to be included in the aforementioned large-scale simulation system. Point-scale evaluation of blowing snow flux will be presented to discuss the modelling choices*. In order to avoid a prohibitive computational cost, this scheme shall not be much more computationally intensive than the Crocus model itself, and it will be forced with 2D wind fields downscaled from NWP systems rather than coupled with 3D high resolution atmospheric model.*"

There is no evaluation of the model in large-scale applications beyond demonstrating its computational feasibility.

This concern was also raised by Reviewer #1. In this model description paper, we considered that the evaluation of blowing snow fluxes was the main topic to address as this is the most direct observation of the newly simulated processes. These evaluations do not allow to determine if the spatial patterns of snow properties simulated are improved by the blowing snow module. This limitation is explicitly mentioned L858-860 “Snow redistribution in 2D simulations has not been evaluated in this article, and will be the subject of a future study expected to provide complementary insights to the following discussions”. Such evaluations are definitely necessary but they raise major methodological difficulties which justify a dedicated and separated paper.

First of all, in large scale simulations, several sources of errors are superposed, making it hard to determine if differences between model and observations come from a misrepresentation of wind-induced snow transport or from other sources of errors. In particular, precipitation forcing at high altitudes suffer from high uncertainty partly due to a largely unexplained spatial variability in NWP precipitation outputs, a severe lack of observations to constrain meteorological analysis systems, and various issues in radar precipitation measurements over complex terrains. It usually leads forcing errors to prevail in snow simulation systems that are not forced by local observation (Raleigh et al., 2015; Schlögl et al., 2016; Günther et al., 2019). Then, as point-scale observations are often not representative of the spatial scale of a simulation system (i.e. 250 m resolution in our case), evaluations have to rely on satellite observations which also involve complex retrieval methodologies and an appropriate consideration of associated uncertainties. Finally, methodological developments are required to compare simulated and observed snow maps as consistent spatial patterns may be simulated with slight localization inaccuracies, making traditional evaluation scores often unadapted (Gilleland et al., 2009).

Therefore, we also prepared a dedicated evaluation paper of the spatial distributions of snow height and Snow Melt Out Date against satellite stereo-imagery (Deschamps-Berger et al., 2020) and optical products (Gascoin et al., 2019). In this evaluation, the contributions of model and forcing uncertainties in simulation errors are quantified and compared. As expected, the forcing uncertainties prevail. This paper (Haddjeri et al.) will be submitted in September 2023 to The Cryosphere and will be complementary to the model description and first evaluations provided in this GMD model description paper.

Moreover, we would like to stress that we follow a strategy similar to other published wind-induced snow transport models operating at similar or lower resolution. 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). These spatial evaluations, when performed, were sometimes conducted in a separate paper (Gerber et al., 2023). Smaller scale blowing snow models were often described along with a purely qualitative evaluation of snow depth patterns on areas ranging from hundreds of meters long transects to a few kilometre square test zones (Liston and Sturm (1998); Liston et al. (2007); Vionnet et al. (2014)), and sometimes without any spatial evaluation (Essery et al., 1999).

Conversely, we provide in the present paper quantitative evaluations of the blowing snow flux and transport occurrence simulated with SnowPappus against a 10-years long observation time series at Col du Lac Blanc. As stressed in our introduction, such evaluations are very unusual in the currently available literature (except in Amory et al., 2021 in Antarctica), so concerning this direct variable, the evaluations provided in our paper are more advanced than similar literature on blowing snow models. These analyses allow to evaluate and discuss the parameterization choices done to compute blowing snow occurrence and fluxes and to question the interest of microstructure-based parameterizations for which the added value had never been assessed. This is also an important added value of our paper.

We propose to clarify the goal and strategy in the introduction. Given the introduction will be merged with Sect. 2.1 (see above our response on manuscript length and organization), we give here the full paragraphs including this clarification. The part of the text which is the most dedicated to it appears in bold.

"*In the above-mentioned context of increasing resolution of snow modelling systems, the long-term project of CNRM aims at performing simulations with Crocus at the scale of the French Alps at 250m resolution in an operational purpose, associated with a data assimilation framework requiring ensemble runs of 50-100 members (Largeron et al., 2020; Cluzet et al., 2021). The 250 m resolution allows a trade-off between the need for precisely representing slopes and aspects, influencing mass and energy balance of the snowpack, and the expected computational cost. In this context, a numerically efficient representation of wind-induced snow transport that can be coupled to Crocus simulations is lacking while this is necessary to better account for its impact on avalanche forecasting over French mountains. Two blowing snow scheme coupled with Crocus exist yet : SYTRON (Vionnet et al., 2018) and Crocus-Meso-NH (Vionnet et al., 2014). However, both are unadapted to this geometry and resolution.*

Thus, the goal of this paper is to describe and present first evaluations of a novel blowing snow scheme, SnowPappus, coupled to Crocus and able to be included in the above-mentioned large-scale simulation system. Point-scale evaluation of blowing snow flux will be presented to discuss the modelling choices"

In addition, L858-860 in the second part of the discussion will be replaced by :
" *Snow redistribution in 2D simulations has not been evaluated in this article, due to several methodological challenges including dealing with the superposition of errors coming from the precipitation fields and finding relevant metrics. It will be the subject of a future study expected to provide complementary insights to the following discussions.*"

But the main problem is that this manuscript requires major copy editing for clarity and concision, beyond what can be achieved in review

As mentioned above, we propose a revised manuscript with very substantial changes, including a revised structure of the literature review and model description to improve the clarity of the manuscript. The length is also reduced by 20%.

I will expect to have more scientific comments on an easier to read revision, but here are a few corrections that I have noted:

There is, I think, a u* missing in equation 13

Indeed, the start of the equation will be corrected. The corrected one will start with :
Q_{susp}=c_r z_r u_*/(k (1- gamma(u_*))

For reproducibility, quoting the values of F(T), A and B in equations 25 and 26 would save the reader some cross referencing. \mu_{satc} and Rh_i are essentially the same thing. Some signposting would help anyone hoping to find the SnowPappus code in the extensive SURFEX repository.

For reproducibility, we propose the following changes in the revised manuscript

- L 462, "A, B constants defined in (Gordon et al., 2006)" will be replaced by ",*A = 0.0018 and B=3.6*"
- in Eq. 26, 1-RH_i will be replaced by *\mu_{satc}*
- F(T) expression, which is quite complex, will be provided in appendix, to limit the increase of the size of the manuscript.
- -A Readme with path towards the most useful SURFEX routines for SnowPappus will be provided with the SURFEX repository.

Wind is measured at Huez and Chambon, so why no simulations forced by observed wind speed in figure 10?

We decided not to perform simulation with observed wind speed at Huez and Chambon as the wind data do not have the same quality as at the *Col du Lac Blanc* observatory. These date would required more treatment in order to get continuous wind time series. In particular, wind sensors are located at a fixed 3,5 m height above ground, so their height must be deduced from measured snow height. They are sometimes of very low quality, with numerous discontinuities and gaps.

Red points masked by the legend in figure 12a appear pink. Move the legend.

Will be fixed.

Vincent Vionnet is a co-author of this paper, so it should not be necessary to be so speculative about differences from Vionnet et al. (2018) in section 5.1.

We would like to apologize for this unclear formulation that did not correctly reflect our clear understanding of this issue. In fact we *were able to* retrieve the original simulation outputs from Vionnet et al. (2018) and found there was not any issue about it. As said L793 we "applied our evaluation process to these data[...] We obtained results very close to our own Sytron run with the original data". Regarding the dates, we were able to obtain the information on the precise temporal window used in Vionnet et al. (2018), which is from 01/11 to 15/04. Therefore, their temporal window is larger than ours, so it is not possible that the change in the temporal window by itself would cause a perfect detection in their case and not in ours. As a consequence, we are sure the reproducibility issue comes from an unreproducible data post-processing, that was applied when compiling the results at the daily time scale in Vionnet et al. (2018). This step was not applied in our evaluation dataset. Of course, this unreproducible data processing is not satisfactory and we took a special care in this publication to respect the FAIR principles with all our data and provide all details in the Code and Data availability section to prevent such inconveniences.

To clarify this point and taking into account the reviewer's call for concision, we propose to replace L789-794 by
"*We were able to retrieve the original simulation outputs of Vionnet et al. (2018) and applied our evaluation process to these data (see code availability), obtaining results very close from ours . Thus, after discussion with the authors, it is clear that the issue comes from unreproducible data post-processing applied to the SPC data to compile results at the daily time scale.*"

2 . Response to Referee #2’s comments and associated manuscript modification

The manuscript “SnowPappus v1.0, a blowing-snow model for large-scale applications of Crocus snow scheme“ by Baron et al., presents a model development for the Crocus snow model to include drifting snow processes. Given the operational applications of Crocus, it potentially is an important step forward. This would warrant publication in a journal like GMD.

We first would like to thank the reviewer for their constructive comments. In the following, we will give a detailed answer to each of them.

However, having said that, I think that the major drawback of the current manuscript is that the goal (which is somewhat implicitly stated in the introduction) is not corroborated by the right validation data to determine if the model developments regarding the drifting snow module are actually an improvement. In other words, I interpret the goal of the model development to be to better capture the spatial distribution of snow (l.23-28). However, the

actual goal stated by the authors in the Introduction is vague (l.51): “to carry out simulations at the scale of the French Alps.” One would expect here to read something like: “to carry out simulations that improve the spatial distribution of snow depth at the scale of the French alps”. The only validation data presented are the blowing snow measurements, which are point measurements. This kind of point validation data makes it hard to justify if the spatially explicit, 2D treatment of drifting snow is in fact useful. 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.

I also would like to stress here that I think that for operational applications, there should also be a demand by the operational users for any validation of model output that is going to be used in an operational product. How would an operational team judge simulated spatial patterns of snow depth when they cannot be certain how well the model reproduces those? Observed blowing snow fluxes at only three points in the domain hardly provide confidence that spatial patterns of snow deposition are in fact correctly reproduced.

Note that drifting snow also impacts the snow microstructure and density profiles by forming wind slabs. This did not seem to be the focus of the authors, but it would require snow pits to validate the results. In any case, the Introduction should discuss this snow microstructural aspect in more depth, I think.

So unfortunately, I think that this is a more serious flaw of the study that makes it hard to further judge the study for possible publication. If I were to recommend major revisions, I would need to see a path forward for how revisions, including new analysis or simulations, could better support the conclusions. But since to me it is not clear at this point what the goal of the model development is, it is very hard to judge what is needed and if it can be deemed feasible. I think either the focus needs to be on the concentration profiles at 1D simulations, and compare those with observations. Or include observations of spatial patterns of snow depth to investigate to what extent the model reproduces those patterns. However, all these options require major redesign of the study and a big overhaul of the manuscript.

The reviewer raises here major concerns about the goals of the article and its relevance in reaching these goals. In the following, we first clarify these goals and then discuss our choice of presenting point-scale flux evaluation rather than spatial patterns of snow accumulation or stratigraphies.

First of all, we would like to apologize for the lack of clarity in our goals in this model development, and we will clarify them in the revised manuscript. Our general goal is to develop a simulation system of the snowpack evolution at 250 m resolution covering the whole French Alps, based on the Crocus operational snow model and for various applications (avalanche forecasting, water resource monitoring for hydroelectricity, snow climatology, ...). To be able to represent a realistic spatial variability of snow properties, the implementation of a blowing snow module coupled to Crocus is necessary. The specific goal of this paper is to present the new blowing snow module dedicated to this specific application, including (1) an accurate model description with appropriate scientific justification considering existing literature, (2) a direct evaluation of blowing snow fluxes based on the available data and (3) an assessment of the numerical applicability of this scheme at our target resolution and spatial domain.

In this model description paper, we considered that the evaluation of blowing snow fluxes was the main topic to address as this is the most direct observation of the newly simulated processes. We agree with the reviewer that these evaluations do not allow to determine if the spatial patterns of snow properties simulated are improved by the blowing snow module. This limitation is explicitly mentioned L858-860 "Snow redistribution in 2D simulations has not been evaluated in this article, and will be the subject of a future study expected to provide complementary insights to the following discussions". Such evaluations are definitely necessary but they raise major methodological difficulties which justify a dedicated and separated paper.

First of all, in large scale simulations, several sources of errors are superposed, making it hard to determine if differences between model and observations come from a misrepresentation of wind-induced snow transport or from other sources of errors. In particular, precipitation forcing at high altitudes suffer from high uncertainty partly due to a largely unexplained spatial variability in NWP precipitation outputs, a severe lack of observations to constrain meteorological analysis systems, and various issues in radar precipitation measurements over complex terrains. It usually leads forcing errors to prevail in snow simulation systems that are not forced by local observation (Raleigh et al., 2015; Schlögl et al., 2016; Günther et al., 2019). Then, as point-scale observations are often not representative of the spatial scale of a simulation system (i.e. 250 m resolution in our case), evaluations have to rely on satellite observations which also involve complex retrieval methodologies and an appropriate consideration of associated uncertainties. Finally, methodological developments are required to compare simulated and observed snow maps as consistent spatial patterns may be simulated with slight localization inaccuracies, making traditional evaluation scores often unadapted (Gilleland et al., 2009).

Therefore, we also prepared a dedicated evaluation paper of the spatial distributions of snow height and Snow Melt Out Date against satellite stereo-imagery (Deschamps-Berger et al., 2020) and optical products (Gascoin et al., 2019). In this evaluation, the contributions of model and forcing uncertainties in simulation errors are quantified and compared. As expected, the forcing uncertainties prevail. This paper (Haddjeri et al.) will be submitted in september 2023 to The Cryosphere and will be complementary to the model description and first evaluations provided in this GMD model description paper.

Moreover, we would like to stress that we follow a strategy similar to other published wind-induced snow transport models operating at similar or lower resolution. 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). These spatial evaluations, when performed, were sometimes conducted in a separate paper (Gerber et al., 2023). Smaller scale blowing snow models were often described along with a purely qualitative evaluation of snow depth patterns on areas ranging from hundreds of meters long transects to a few kilometre square test zones (Liston and Sturm (1998); Liston et al. (2007); Vionnet et al. (2014)), and sometimes without any spatial evaluation (Essery et al., 1999).

Conversely, we provide in the present paper quantitative evaluations of the blowing snow flux and transport occurrence simulated with SnowPappus against a 10-years long observation time series at Col du Lac Blanc. As stressed in our introduction, such evaluations are very unusual in the currently available literature (except in Amory et al., 2021 in Antarctica), so concerning this direct variable, the evaluations provided in our paper are more advanced than similar literature on blowing snow models. These analyses allow to evaluate and discuss the parameterization choices done to compute blowing snow occurrence and fluxes and to question the interest of microstructure-based parameterizations for which the added value had never been assessed. This is also an important added value of our paper.

We propose to clarify this goal and strategy in the introduction. Given the introduction will be merged with Sect. 2.1 (see below our response on manuscript length and organization), we give here the full paragraphs including this clarification. The part of the text which is the most dedicated to it appears in bold.

"*In the above-mentioned context of increasing resolution of snow modelling systems, the long-term project of CNRM aims at performing simulations with Crocus at the scale of the French Alps at 250m resolution in an operational purpose, associated with a data assimilation framework requiring ensemble runs of 50-100 members (Largeron et al., 2020; Cluzet et al., 2021). The 250 m resolution allows a trade-off between the need for precisely representing slopes and aspects, influencing mass and energy balance of the snowpack, and the expected computational cost. In this context, a numerically efficient representation of wind-induced snow transport that can be coupled to Crocus simulations is lacking while this is necessary to better account for its impact on avalanche forecasting over French mountains. Two blowing snow scheme coupled with Crocus exist yet : SYTRON (Vionnet et al., 2018) and Crocus-Meso-NH (Vionnet et al., 2014). However, both are unadapted to this geometry and resolution.*

Thus, the goal of this paper is to describe and present first evaluations of a novel blowing snow scheme, SnowPappus, coupled to Crocus and able to be included in the above-mentioned large-scale simulation system. Point-scale evaluation of blowing snow flux will be presented to discuss the modelling choices"

In addition, L858-860 in the second part of the discussion will be replaced by :

" *Snow redistribution in 2D simulations has not been evaluated in this article, due to several methodological challenges including dealing with the superposition of errors coming from the precipitation fields and finding relevant metrics. It will be the subject of a future study expected to provide complementary insights to the following discussions.*"

Finally, drifting snow highly impacts snow stratigraphy and this is one of the main reasons why spatialized applications of Crocus intending to take benefit from the simulated snow stratigraphies (e.g. avalanche hazard forecasting) require the implementation of a dedicated blowing snow module. We agree with the reviewer that this aspect should be discussed in the introduction, and this will be done in the revised manuscript.

However, in the general case, the evaluation of simulated snow profiles against observations is still highly challenging and appropriate evaluation methodologies are complex to set up (e.g. Viallon-Galinier et al., 2020). This caveat is identified as one of the main unresolved issue in numerical snow modelling (Morin et al., 2020, Ménard et al., 2021) and it can not be resolved in our paper. In the case of wind-blown areas, an additional major limitation is the unavailability of snow pits that would be necessary to evaluate these features. We pointed out L879-883 in the discussion the lack of quantitative information about the interaction between wind-induced snow transport and snow surface properties, which explains why we chose very simple representations of this process, considering the impossibility of any accurate evaluation. As we wrote, observation or experimentation campaigns such as the very recent work of Walter et al., 2023, with measures of snow properties in wind-blown areas would be of primary interest to enhance the representation of these processes in a model.

In the revised manuscript, we propose to discuss this briefly in the introduction. We propose to include this paragraph :

"*A major interest in coupling Crocus with a blowing snow scheme is its detailed representation of snow stratigraphy and microstructure as it may be an opportunity for the simulation of snow transport occurrence (Guyomarc'h and Mérindol, 1998; Lehning et al., 2000). Therefore, we test the added value of microstructure-based parameterizations of snow transport occurrence in the evaluation section. Moreover, it allows Crocus to be used as a tool for avalanche forecasting (Morin et al., 2020). Given that wind slabs formed by wind-induced snow deposition are one of the main causes of avalanche triggering (Schweizer et al., 2003), a blowing snow scheme coupled with Crocus could become a powerful tool for avalanche forecasting, even if evaluation of the simulated stratigraphy is out of the scope of this study.*"

Note that clarifications asked about the interest of using snow microstructure in a blowing snow model are also included in this paragraph (see our response dedicated to it below)

Another major concern is the length of the paper, which I think is mainly a result of insufficient organization and logic. On occasion there is too much detail given, and some discussion is too spread out. For example, Section 2.2.1 treats "Theoretical background", but section 2.3.2 also reads like theoretical background. So while reading, the manuscript is jumping back and forth between theoretical considerations, and implementation details, which makes it somewhat cumbersome to read and follow. But the manuscript would need to be shortened massively and bring its length more in line with the amount of unique content and validation data. Otherwise, a lot of detail is provided which is not helpful to interpret the results. For example, the discussion on sublimation stand all by itself. It is not clear at all how it impacts the simulations.

We understand the major concerns of both reviewers about the length and lack of clarity of the paper. In order to address this issue, we propose to extensively modify the structure of the paper to improve its readability. We will split Section 2 (Methodological choices) into two separate sections. The first one will be dedicated to a literature review largely reduced in length by focusing on the topics for which an added value is provided in SnowPappus compared with existing works. It includes discussions about the representation of saltation flux and lower boundary condition for suspension flux and terminal fall speed parameterizations. This will allow to make the logics of the manuscript clearer. For processes that are simply implemented in SnowPappus following existing models or previous literature, the literature overview will be considerably shortened, or even suppressed if it is not useful for the remaining part of the article, such as the discussion on sublimation (L449-452). Then, a second section will be dedicated to a concise model description without any theoretical interruptions between the description of the different implementations. We believe this organization will help the reader to understand more quickly our modelling choices.

In addition, several parts of the manuscript will be strongly shortened, primarily the description of numerical performance, various repetitions and unnecessary information or discussion will be withdrawn and a more concise writing style will be adopted when possible. Besides, the modification of the paper structure, which splits the section "Methodological choices" into 2 parts do not result in additional length, as the number of subsubsections does not changes a lot and repetitions will be avoided. Overall, the planned changes will allow to suppress Fig. 15 and move Fig. 14 in the appendix and to reduce overall text length by 20% (without taking appendix into account).

In the following, we will first list the text passages that will be strongly shortened, or withdrawn, in their order of appearance and identified by their section number and if necessary line number in the preprint. Then, we present a revised text outline to apply the described changes. New (sub)sections are written with their titles underlined, and there contains in plain text. Parts of the outline in which the organization remain identical with the preprint's outline are marked as UNCHANGED, although some length reductions have been also applied in these parts.

shortened text passages (with paragraph and line number from the initial manuscript):

2.1 Target, opportunities and constraints
2.2.2, wind profile
2.3.2 Blowing snow trajectories and transport modes
2.3.4 L257-263 discussion on the influence of particle size distribution on the suspension transport
2.3.3 L207-241 - Different types of transport models
2.3.7 L334-358 equations of simple saltation parameterizations P90 and S04
2.3.8 Influence of fetch distance
2.5 L502-508 Influence if snow transport on snow surface properties (state of the art)
3.2 L 560-570 Wind downscaling description
3.5 L 623-634 Methods for blowing snow occurrence measurements
4.6 L734-739, domain decomposition for parallel computing
5.1 L799-805 Discussion on the outlier in blowing snow fluxes evaluation

withdrawn text passages :

2.4 L449-452, Sublimation
4.5 L712-717, Evaluation of blowing snow fluxes at Col du Lac Blanc, discussion on the outlier (repeated in the discussion)

new outline in the revised manuscript :

1. Introduction

General introduction followed by the target, opportunities and constraints of the development of SnowPappus (corresponds to Sect. 2.1 in the preprint)

2. Blowing snow flux computation : state of the art

2.1 Blowing snow occurrence

Useful theoretical background for blowing snow occurrence detection (Sect. 2.2.1 in the preprint)

2.2 Horizontal blowing snow fluxes

2.2.1 Notations and geometric considerations

2.2.2 Blowing snow particle trajectories and transport modes

State of the art on the trajectories of blowing snow particles and transport modes, focused on saltation and suspension (corresponds to Sect. 2.3.2 in the preprint)

2.2.3 Suspension transport modelling

-Existing types of suspension transport models (part of Sect. 2.3.3 in the preprint)

-Literature review on the effective terminal fall speed (mainly informations in Sect. 2.3.5 of the preprint)

2.2.4 Transition between saltation and suspension

- The way it is treated in other models
- State of the art on this transition zone
- Problems of definition of the lower boundary condition for suspension transport (corresponds mainly to Sect. 2.3.6 of the preprint)

2.2.5 Simple saltation models

-Short description of Sorensen et al., 2004 (S04) and Pomeroy et al., 1990 (P90) saltation parameterizations

-Discrepancies between S04 and P90 and discussion of the possible causes (informations in Sect. 2.3.7 of the preprint)

3. Model description

3.0 Crocus description

Very short description of Crocus snow model

3.1 Blowing snow occurrence

Equations of the different options implemented in SnowPappus for threshold wind speed for transport (Sect. 2.2.3 in the preprint)

3.2 Horizontal blowing snow flux

3.2.1 Suspension transport

- Reasons for the choice of the model type
- Logarithmic wind speed profile (Sect. 2.2.2 in the preprint)
- Equations for suspension transport in SnowPappus (corresponds to Sect. 2.3.4 in the preprint)
- Parameterization of terminal fall speed used in SnowPappus (informations in Sect. 2.3.5 of the preprint)
- Maximum height of suspension transport as a function of fetch distance (part of Sect. 2.3.8 of the preprint)

3.2.2 Saltation transport and transition with suspension

- Description of the two options (using S04 and P90) of flux computation in the saltation zone and the transition with suspension in SnowPappus (included in Sect. 2.3.7 of the preprint).
- Influence of fetch distance on saltation transport in SnowPappus (included in Sect. 2.3.8 of the preprint, with Fig. 3 appearing)

3.3 Sublimation

Sublimation options in SnowPappus

3.4 Mass balance

Mass balance implementation in SnowPappus

3.5 Influence of snow transport and deposition on snow surface properties

- Properties of deposited snow
- Blowing snow induced snow metamorphism

3.6 Implementation in SURFEX

How SnowPappus is included in SURFEX code (mainly Sect. 2.7 of the preprint, but includes the description of domain decomposition for parallel computing)

4. Evaluation : methods

UNCHANGED (with length reductions)

5. Results

mainly UNCHANGED but addition of 1 paragraph (see below) and Fig. 14 moved in appendix, as partly redundant with Fig. 13 .

5.1 Comparison of saltation parameterizations

Comparison of blowing snow fluxes obtained with S04 and P90 implementations in SnowPappus and implications for the comparison of S04 and P90 (corresponds to the end of Sect. 2.3.7 in the preprint).

6. Discussion

UNCHANGED (with length reductions)

7. Conclusion

UNCHANGED

l.76-77: "The ability of Crocus to distinguish different snow types at the surface may be an opportunity for the simulation of snow transport (Guyomarc’h and Mérindol, 1998; Lehning et al., 2000)." is vague. Do authors aim to validate this in their study or not?

In this sentence, we point out that different studies used parameterizations based on snow microstructure for blowing snow occurrence. However, in the particular case of Crocus, added value of microstructure-based parameterizations compared with simpler ones was not demonstrated in earlier studies (Guyomarc'h and Merindol, 1998; Vionnet et al., 2013; Vionnet et al., 2018), hence our use of *may.* Therefore, in our paper we test this hypothesis in Sect. 4.3 by comparing Guyomarc'h and Merindol (1998) parameterization with a simpler one. We propose to clarify this point by changing L76-77 in

"*A major interest in coupling Crocus with a blowing snow scheme is its detailed representation of snow stratigraphy and microstructure as it may be an opportunity for the simulation of snow transport occurrence (Guyomarc’h and Mérindol, 1998; Lehning et al., 2000). Therefore, we test the added value of microstructure-based parameterizations of snow transport occurrence in the evaluation section.*"

Note that, due to the reorganisation proposed above, as the whole Sect. 2.1 this will be part of the introduction.

On a final note, given that Vincent Vionnet is listed as co-author, it is actually very strange to read:
L.790-791: "precise temporal window within the year of Vionnet et al. (2018) which was not given in their article and may differ from ours" and l.792: "We could retrieve the original simulation outputs of Vionnet et al. (2018)". It shouldn't have been so problematic to resolve these issues, since Vincent Vionnet is co-author. In fact, given his co-authorship, a much more solid discussion of the SnowPappus results with his earlier work is expected at this point.

We would like to apologize for this unclear formulation that did not correctly reflect our clear understanding of this issue. In fact we *were able to* retrieve the original simulation outputs from Vionnet et al. (2018) and found there was not any issue about it. As said L793 we "applied our evaluation process to these data[...] We obtained results very close to our own Sytron run with the original data". Regarding the dates, we were able to obtain the information on the precise temporal window used in Vionnet et al. (2018), which is from 01/11 to 15/04. Therefore, their temporal window is larger than ours, so it is not possible that the change in the temporal window by itself would cause a perfect detection in their case and not in ours. As a consequence, we are sure the reproducibility issue comes from an unreproducible data post-processing, that was applied when compiling the results at the daily time scale in Vionnet et al. (2018). This step was not applied in our evaluation dataset. Of course, this unreproducible data processing is not satisfactory and we took a special care in this publication to respect the FAIR principles with all our data and provide all details in the Code and Data availability section to prevent such inconveniences.

To clarify this point and taking into account the reviewer's call for concision, we propose to replace L789-794 by :

"We were able to retrieve the original simulation outputs of Vionnet et al. (2018) and applied our evaluation process to these data (see code availability), obtaining results very close from ours . Thus, after discussion with the authors, it is clear that the issue comes from unreproducible data post-processing applied to the SPC data to compile results at the daily time scale."

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