Response to Referees

Simulation of cold powder avalanches considering daily snowpack and weather situations to enhance road safety

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Dear Editor,

We greatly appreciate the opportunity to proceed to the next round of the review process. We are thankful for the detailed and insightful feedback provided by the reviewers, which has been invaluable in improving the quality of our manuscript. In response to the feedback, we have copied the reviewers' comments into the blue boxes and gave our responses under each comment.

Sincerely,

Julia Glaus & co-authors

Response to Reviewer R1

0.1 General assessment of the revised version

The authors have clearly made a substantial effort in revising their manuscript, taking the criticism and suggestions of both reviewers seriously. The text is much better balanced and easier to read than before (a few exceptions will be mentioned below). Some figures without particular value for the paper were removed, others that have a specific purpose were added, and many others were improved to make them more informative or easier to understand.

0.2 Remaining issues

The description of RAMMS::EXTENDED has become more self-contained and easier to understand. However, the more detailed description of the closure assumptions has also brought additional problems to light that must be dealt with. The main problem areas are the following:

C1: In the balance of internal energy (directly linked to the temperature of the avalanche core), the source term $\dot{Q}_m$ describes the heat loss due to melting of snow. Equation (11) specifies the form of $\dot{Q}_m$ in terms of an integral over an infinitesimal time interval, which gives an infinitesimally small result if $\dot{Q}_m$ is finite. Moreover, the integration interval dt does not appear on the right-hand side of the equation. This expression is presumed proportional to the temperature difference between the avalanche core and the melting point of ice, $T_\Phi - T_m$. Now, the temperature of a dense or fluidized flow is lower or at most equal to T_m, hence there is no heat flux available for melting. (In a very dilute flow, i.e., in the powder cloud, $T_\Phi > T_m$ is possible in principle.) As far as I can see, the problem arises because only the macroscopic quantity T_Φ is available, while the melting is driven by local heat production, i.e., at the mesoscopic or even microscopic level. Therefore, some assumptions on the link between the macroscopic variables and the mesoscopic ones are needed, but the scheme employed by the authors is not suitable.

In my opinion, the simplest and best solution to this conundrum is to remove the description of melting from the paper because this specific term was not used in this work focusing on cold powder snow avalanches. The problem raises, however, some questions concerning the correctness of papers on wet-snow avalanches published earlier.

We agree to the review comment on the inconsistency of the description of the melting process. As this publication focuses on cold avalanches, we removed this part at the very end of Chapter 3.1 and added a short statement, that more information on the melting process can be found in (Zhuang et al., 2023).

C2: The authors provide an argument why the gravitational term in the momentum balance for the powder cloud is not important most of the time: The momentum injected into it by the mass ejected from the dense core is said to be much larger than the gravitational pull,

$$\dot{H}\,\Phi \to \Pi\,u_\Phi \gg \left(1 - \frac{\rho_\Lambda}{\rho_\Pi}\right) G_\Pi\,.$$

However, this equation is dimensionally inconsistent; the right-hand side should be multiplied by H_Π. This can be a large number, hence the gravitational term does become important as soon as the powder cloud has developed sufficiently.

We have corrected the misspelling and added H_Π to the right-hand side of the equation. We agree that for larger, fully developed powder clouds, this assumption may become critical. Consequently, we have included a discussion in line 311 of the track-changes file to highlight the limitations of these assumptions.

C3: The explanation of the entrainment model remains difficult to understand, possibly due to misunderstandings of the mechanisms at work in erosion and entrainment. Another problem is that Eq. (21) is not positive definite, contrary to the authors' statement. It is unclear whether the code enforces positivity of the erosion coefficient κ or not. At any rate, if κ becomes 0 below a limiting slope angle, erosion during run-out in level terrain or even in counter-slopes cannot be described, even though it has been observed many times.

In the code of RAMMS::EXTENDED, the positivity of the κ coefficient is enforced. We added a comment from line 346 on in the track-changes file on the limitation of the system and mentioned also the case of VdlS 1999 where exactly the case of erosion in the flat terrain / counter slope was observed.

While the authors have added some suggested references to work by other researchers, still well over half of the references are to work carried out at SLF. Most of these references are not out of context, but not all of them are clearly needed. Such a high degree of self-citation in an international research field like snow avalanches is highly unusual and leaves readers with a poor impression. I suggest the authors prune their self-citations by about half.

We agree that this is an unfortunate situation. As the first half of the publication summarizes the individual components of the simulation tool RAMMS::extended, a large number of in-house citations naturally arise, as this tool was developed over many years at SLF. We tried to add more citations.

0.3 Minor Remarks

Please see the annotated manuscript for numerous small corrections and suggestions concerning the language and presentation. In particular, the reference list needs some further corrections and additional bibliographic information.

We worked through all numerous small corrections and adjust the text accordingly to improve the language and presentation.

0.4 Recommendation to the editor

In view of the major improvements of the manuscript and the nature of the remaining issues, I recommend to the editor to accept the paper for publication in NHESS on the condition that the points listed above are satisfactorily resolved in a minor revision.

Response to Reviewer R2

General Comments

Given that the first answers are on the style of "we we can do this or that", I expected much more consise answers of what you acually did. But, most of the answers are still the same vague replies making finding the changes very difficult and cumbersum. I was not able to find all the answers to my first questions. Please carefully check my first review, and discuss with you co-authors if they are properly captured. Most of my comments to improve discussion of subsection 4.3 and 4.4 are not answered or done. Please discuss them properly. I find the story of the manuscript still rather difficult to follow. And the order of informations is not the best. In general, I have the feeling, that the experienced co-authors were not much active during the reviewing of the manuscript and too much effort is therefore handed over to the external reviewers. The publication has definitively improved and has potential, but still needs some more effort. Please help the reviewers by more clearly state what you changed and where.

C4: R2-3: The suggested discussion of the simplification that alpha is temperature independent is missing. The authors only state it's constant value for cold and warm avalanche without clearifying what cold and warm means.

We added the definition from (Köhler et al., 2018) in line 235, in the track-changes file.

C5: R2-4: I can not find the updated part in the "first paragraph on avalanche model" regarding the VdlS calibration. I see one sentence clarifying the calibration process at the very end of the introduction, however, to the reader not familier with the VdlS topography, the sentence is rather confusing. When summarizing the current state of the model from literature (e.g. R2-2), I would like to see also a paragraph about the calibration.

According to personal communication with Perry Bartelt, the simulation tool was originally calibrated based on the friction parameters of RAMMS::Avalanche. During the development of RAMMS::EXTENDED, it underwent continuous calibration by incorporating new features and simulating numerous avalanches. In the track-changes file we added more details in line 66, which now reads as follows: "The model was calibrated using avalanches observed at Vallée de la Sionne (VdlS) (Ammann, 1999), considering only those that did not reach the counter slope according to (Bartelt, 2025). Additionally, avalanches from winter 1999 in Switzerland were used which are presented in Vallet et al. (2001). "

C:6 R2-5: I am still missing the distances along the idealize plane topography (you give elevation and horizontal distance), but your verification works on the distances. Naming: idealize plane (e.g. Fig 4) or inclined plane (e.g. Fig 8). Stick to one. Regarding your definition of avalanche length or runout distance using the longest distance between two points in the affected area: this is a rather unusual definition, but it seem to work on most avalanches as they are much longer than wide. However, please add 1 or 2 sentences explaining this (and the limitation).

We added the runout distance scale in the graphic as you described. Overall, we adjusted the naming to "idealized plane." In the last round of discussion we added the limitations of our methods in line 125 of the track-changes file: "While this method works well for simple avalanches, it requires careful consideration in cases where avalanches exhibit finger formation or the avalanche strongly deviates in the lateral direction. Additionally, the code accounts for the case of perfectly symmetric avalanches (as observed on idealized slopes) by measuring the distance between the two outermost points in the flow direction. "

C7: R2-7: I can not find the mentioned train of thoughts in the publication. I think, that discussion not using the VdlS data is important. (BTW: VdlS has the most complete dataset in the world, your argument with SNOWPACK is of secondary importance and should not be mentioned in the discussion.)

We decided not to include it, as reusing avalanches to analyze a simulation tool that was already calibrated with them did not seem appropriate. Additionally, in the Brämabühl area, the avalanches have a long runout, making it a suitable setup to investigate how sensitive the avalanche runout distance is to the input data. In contrast, the VdlS avalanches run into the counter slope. At this point, the RAMMS model reaches its limitations, as described in more detail in the paper (see the erosion model chapter line 346 of the track-changes file). However, we included one VdlS avalanche that was not part of the calibration process to further validate our approach, as mentioned in line 114 of the track-changes file.

Other Comments

C8:

1. Line 90: Add reference to your supplementary material
2. Line 98: The name of you institute is not correct, but is also not important here. Please remove
3. Line 148 and Line 138: Duplication of sentences. Please reread and revise the text between them
4. Fig14: Color scale of pressure has only one value and the wrong unit

1. References will be added as soon as the supplementary material is published.
2. We removed the institute name.
3. We removed the sentence and rewrote the section according to the review inputs in the additional document from Dieter Issler.
4. We adjusted the color scale and value and added a discussion on the powder cloud values.

C9: Fig 8: remove the points for warm temperatures. You can not show data you are not convinced of, and tell the people to take caution. Just dont show wrong / untested results.

In the text we mention the limitations of the model for warm avalanches. We think it is important to present the full range of parameter variations, to illustrate the numerical behavior of the simulation, even though it is not fully tested.

C10: Line 515: How does the newly added subsection 4.2 on release length / angle and this sentence work together? From 4.2, I understand that only for very steep release areas the powder cloud goes further than the dense part (which is rather counter-intuitive, please discuss in more detail, e.g. what is terrain friction?)

We agree that the term 'friction' is misleading in this context. We have adjusted it to 'terrain roughness,' referring to features such as rocks, vegetation, and old snow structures. Accordingly, we have rephrased the sentence in Line 452 as follows: "On a real terrain, the terrain roughness (cased e.g. by vegetation and rocks) would strongly decelerate the avalanche core and reduce its runout distance."

C11: Line 522: The comparison between estimated front or approach velocity and maximum peak field velocities does not work! Please compare the velocities of Fig2 to equivalent values (extract the approach distances from the model run at the corresponding timesteps). See below to R2-32.

We have corrected the evaluation and present the simulated and measured values of the powder cloud front in the newly added Table 3. As stated in the caption, we only assess whether the front velocity falls within the correct order of magnitude. The purpose of this comparison is to verify that the results lie within a reasonable range rather than to imply that the simulation accurately captures velocity at a highly detailed level.

C12: Line 523ff: I find the comparison with volumes from the drone data rather difficult to follow. Please rephase and explain to the reader what is important. (E.g. what is the difference you want to explain in Line 531?). Fig12 indicates core volume up to 120.000 m^3 and erosion volumes up to 80.000 m^3, but in Line 529 is only 30.000 m^3 deposit. On the other hand, the drone data looks rather like no deposit at all (inside the outline to left and right of the outline has a fairly similar coloring and pattern). Can you explain this more in depth?

We have rephrased the paragraph to provide a more detailed explanation of the calculation for the comparison between the simulated and measured deposition. It is not possible to directly compare the values in Figure 12 with the deposition data calculation, as Figure 12 considers the mass of the entire avalanche, whereas this paragraph focuses solely on the deposition in the runout zone due to gaps in data in the forested area. We have included this clarification in the text to avoid any misunderstanding. Additionally, for the drone data, we have specified that the calculation takes into account the compression of the old snow by the heavy avalanche deposit, proportional to the density increase up to 450 kg/m^3.

C13: Fig 3: One can see that there has been significant erosion in the main part of the track (blue colors are basically no/only little snow left), however, in the deposition area most deposition is at 1m which equates to the normal snow cover prior to the event. Where do you expect is all the mass from the track? Please explain or mention this discrepancy.

As answered in comment C12, the avalanche compressed the old snow cover. Additionally, the color scale shows all snow height values above 2m in red, and not a fixed limit at 2m.

C14: Line 538ff: And what does this mean for the simulation? Is the simulation now bad?

This statement in the manuscript concerns more the approach of averaging local station measurements and deriving conclusions about the conditions in a single avalanche track. With this observation, we show that even though the three avalanche tracks are very close to each other, they were still exposed to different wind influences, and consequently, to varying amounts of wind-drifted snow. Further research on this topic is currently being conducted in the study by (Ruttner-Jansen et al., 2023), where two measurement stations were placed directly in the release areas at Brämabühl to evaluate this approach. We hope to publish a more detailed analysis of this topic next season.

C15: R2-16: I doubt there is a commonly accepted way how to estimate the snow temperature for avalanche dynamics purposes. It would be helpful for the reader to include one sentence to mention the discrepancy between the temperature in the release zone and the nearby flat field station measurements.

We answered this in our statement in comment C14.

C16: R2-29: I dont see the mark at 0.3C/100m. And line 407 shows you changed from 0.3 to 0.5C/100m. Please revise and be consistent.

We adjusted the parameters. As the graphic is already quite overloaded, we decided not to add more information.

C17: R2-30: The sentences still says that a deep snowcover is need for the longevity of PSAs, and your answers suggests that you drive the erodible layer with the new snow. So, please rephrase the sentence and clearify. Also to R2-31: You should present the reader also with the limitations, and that the approximation fails for extreme avalanches that basically erode everything.

- The approach of just taking the new snow layer into account is part of the traditional hazard mapping scheme. Further research is done in the ISSW publication (Glaus et al. 2024) where additional weaklayers are taken into account as described also in the outlook section.
- Limitation of the erosion model: We fixed this discussion according to Dieter Isslers review comments (see C3).

C18: Fig10: Unfortunately, the modification of the figure removed the initial snow temperature of the mountain. Can this broad back? Line 450: "increase by 3C from -8C to -2C" does not work...

The initialization is already presented in Figure 5, and we have removed it here to avoid redundancy but added a comment in the caption. Additionally we fixed the typo in the temperatures.

C19: R2-32: Well, ususally the questions from the reviewer are pointing to some missing information which could be of interest to the general reader and not only to the reviewer alone. So, you are able to extract some velocities of the avalanche in Fig. 2, now here you show the frontal evolution over time. You can estimate a velocity from this plot inbetween the 5s steps, and you have another measurement to compare the simulation against, e.g. in line 520ff.

As answered in C11 we added the comparison in Table 3 and discussed it in the text. We did the comparison to the simulation with dump steps of 1 s to evaluate the velocity inbetween the pictures.

C20: R2-33: Adding half a sentence is not a discussion of other literatur. Please extend the discussion the literature mentioned in the first review round! Similar holds true for R2-35.

As we do not discuss the relation between erosion and frontal velocity in this publication, we also do not include an extended literature review about this topic.

C21: R2-37: OK, you estimate some velocities from the video, but you dont use this information. As mentioned earlier, please add the front velocity to Fig.14 or to a similar figure.

As answered above in C11, we added the comparison in Table 3.

C22: R2-40: I can not find the part in the manuscript where you comment / discuss on the issue. Please refine and clearly point to the part in the manuscript. R2-40: You refuse to either redo the simulations, or to properly discuss the given question. I feel like this is one of the main points of the paper, but not at all discussed.

We agree that it is difficult to explain exactly why the longest avalanches are simulated for a certain release temperature. However, in this publication, we use RAMMS::EXTENDED as calibrated by the developers of RAMMS and evaluate how this calibration performs for the Brämabühl avalanches. The exact location of this peak is not influenced by the authors of this publication. We conducted the same simulations for the Salezer avalanche and one VdlS avalanche from 2016, and observed the same peak. Therefore, rerunning the simulations, which have already been performed multiple times over the last two years, will not alter the results.

C23: Fig15 (comment F16): you did not annotate the arm.

We have revised the paragraph, as mentioned in comment C12 above, regarding the deposition. We can only provide a qualitative description, noting that higher deposition occurs within roughly the same region. However, since there is no data available on the snow cover prior to the avalanche, it is possible that an old avalanche deposit was already present. Furthermore, we aim to avoid giving the impression that the simulation is precise enough to replicate the avalanche with such a high level of detail.

C24: Line 533: Well, the answer you gave in the review manuscript should be given in the paper manuscript. You have to tell the reader what you want to say. Simply say we exclude this and that opens more questions that is helpful in any way.

We added those information as described in comment C12.

C25: Line 535: Still unclear.

We rephrased the paragraph as described in comment C12.

C26: Line 410: This sentence is still unexpected and does not make much sense without additional information. Please write with a couple of sentences this information and what implication this wind scour involves.

We are not sure which passage you refer to as line 410 is about the definition of the random kinetic energy.

C27: Line 580ff: The last paragraph in the Conclusion should go to Outlook (If one needs an outlook explicitly, better move those thoughts to the discussion).

We adjusted the location of the paragraph as suggested.

References

Ammann, W. J.: A new Swiss test-site for avalanche experiments in the Vallée de la Sionne/Valais, Cold Regions Science and Technology, 30, 3–11, doi: 10.1016/S0165-232X(99)00010-5, 1999.

Bartelt, P.: Personal communication, 2025.

Köhler, A., Fischer, J. T., Scandroglio, R., Bavay, M., McElwaine, J., and Sovilla, B.: Cold-to-warm flow regime transition in snow avalanches, The Cryosphere, 12, 3759–3774, doi: 10.5194/tc-12-3759-2018, 2018.

Ruttner-Jansen, P., Glaus, J., Wieser, A., and Buehler, Y.: A Measurement System for Mapping Snow Distribution Changes in an Avalanche Release Zone, URL https://arc.lib.montana.edu/snow-science/objects/ISSW2023_O10.05.pdf, 2023.

Vallet, J., Gruber, U., and Dufour, F.: Photogrammetric avalanche volume measurements at Vallée de la Sionne, Switzerland, Annals of Glaciology, 32, 141–146, doi: 10.3189/172756401781819689, 2001.

Zhuang, Y., Piazza, N., Xing, A., Christen, M., Bebi, P., Bottero, A., and Bartelt, P.: Tree blow-down by snow avalanche air-blasts: dynamic magnification effects and turbulence, Geophysical Research Letters, 50, e2023GL105 334, doi: 10.1029/2023GL105334, 2023.