Response to Referees

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

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Dear Editor and Referees,

We greatly appreciate your detailed and insightful feedback. This has been invaluable in enhancing the quality of the manuscript. We have copied your comments into the blue boxes and enumerated them so that we can address each part separately below. The responses to the comments from the first reviewer are marked as R1, and the responses to the comments from the second reviewer are marked as R2.

Sincerely,

Julia Glaus & co-authors

Response to D. Issler

0.1 Major Remarks

R1-1: **Title:** The present title of the manuscript relates more the vision and long-term goal of the authors rather than the actual content of the work they report on. What they show is that, for a given day when there was high or very high avalanche danger and artificial avalanche release led to three events in the study area, their proposed procedure for estimating avalanche runout would have been able to predict the extent of the run-out areas of these specific avalanches quite well. This is a significant achievement, but to achieve the stated goal of using weather and snow pack data daily to enhance road safety, the method should be demonstrated to work in other situations as well (and preferably also in other places). This discrepancy can easily be removed by adjusting the title and a few text passages, however.

Thank you very much for your suggestion on broadening the scope of the paper. We removed the road safety aspect in the title, resulting in "Simulation of cold powder avalanches considering daily snowpack and weather situations".

R1-2: **Project goal:** The following remark concerns the very choice of the research topic, and the authors need not address it in the revised version of the manuscript, but it is perhaps worth a thought. The work leading to this paper was financed by a project focusing on improving road safety. However, if the objectives of the manuscript are achieved, the method would be of at least equal use when it comes to deciding whether to evacuate specific settlements because of imminent avalanche danger or not—a situation recurring yearly in Norway. Thus, the focus on road safety in the title indicates an unnecessarily constrained scope. Furthermore, I would venture the guess that the majority of substantial avalanche events in paths that threaten roads in the Alps will reach that road with high probability. The primary question for the road safety managers is therefore about the release probability, not about the run-out. This question is not addressed in this paper, however. Moreover, in Switzerland the local avalanche commissions typically know from experience how far avalanches are likely to reach in a given situation, and they will not risk keeping a road open if the simulation indicates that the avalanche will stop 50 m before the road. The uncertainty in the calculation of avalanche run-out is, in my opinion, still larger than we "experts" often make ourselves believe.

Many thanks for the suggestion. We will discuss this aspect further in the introduction.

R1-3: **Fracture depth:** It is understandable why the authors try to adhere to the method for selecting the fracture depth outlined in the Swiss guidelines. However, this may run into problems in the present context: While it is true that the majority of dry-snow avalanche releases involve only the new-snow layer, there is a non-negligible fraction of events that release on a deeply buried weak layer. For example, two very big powder-snow avalanches with average fracture depths of 1.5–2 m occurred at the Albristhorn and at Scex Rouge in Switzerland in January and February, 1995 after a very moderate snowfall (Issler et al., 1996, 2020). If the decision on road closure had been taken on the basis of a simulation with a fracture depth corresponding to the new snow (10–20 cm), the road below the Albristhorn would have remained open, with potentially dire consequences. I do not mean to require that the authors revise their method to encompass this issue, but it is important that they point out this potential weakness,which needs to be addressed before the method and the model can be used safely for such decision

This definitely holds true. We discussed this point in the ISSW extended abstract 2024. We will highlight this point additionally in the discussion.

R1-4: **Studied parameter dependencies:** In my opinion, extra insight could be gained if the authors also varied the fracture depth, the release-zone length and the slope angle in the simulations on the idealized profile. This could presumably be done with little additional effort. The authors' statement that changes in available new-snow depth beyond 1 m do not influence the run-out distance may not be valid under more extreme conditions. A robust procedure for (largely automated) simulations for site-specific forecasting and warning can only be achieved if all relevant dimensions of the parameter space have been explored and included in the recommendations.is presumably dependent on the release mass or fracture depth. In simpler models, non-dimensionalizing the equations helps in identifying the parameters that govern the behavior of the system, but in the present case it remains to be seen whether this leads to additional insight. Again, I do not insist that the authors do all of this in the present manuscript, but I would like to encourage them to put "more meat on the bone" to make the paper more conclusive and valuable. At the very least, a brief discussion of these matters should be added

Many thanks for this input - we will include this analysis for the idealized profile.

R1-5.1: **RAMMS::EXTENDED:** This code plays a pivotal role in the proposed methodical approach, and some parts of the model are explained in detail. However, neither the all-important mass exchange terms between the suspension layer and the dense core and the ambient air nor the source term for the height evolution in Eq. (7) are specified. These source terms must be modeled in terms of the dynamical variables of the system, which introduces additional parameters, which influence the behavior of the avalanche, as is shown, e.g., in (Vicari and Issler, Ann. Glaciol., 2024) but the values of which are not given a priori. The authors effectively pin down these parameters to some unspecified values without studying the sensitivity.

The reviewer is correct that the formation of the cloud from the core is the result of a complex interaction between the core, the ambient air and the generation of turbulence. Our goal in this paper was to express general physical relationships between the core, the cloud, the snow layer and ambient air as our application is directed toward road safety. We will add more details to the mass exchange terms between the suspension layer and the dense core and the ambient air.

R1-5.2: Since the source terms are not specified while RAMMS::EXTENDED apparently has been under incremental development, it is not clear to which degree the model still corresponds to the stages described in earlier papers by Bartelt and coworkers. Since the 2023 GRL paper by Zhuang et al. appears most up-to-date, it would be preferable to mainly refer to that paper, in which the other references are also cited. I presume the authors wanted to keep the manuscript focused on the new work rather than the description of the model. I feel, however, that the present balance is not optimal—many equations are shown, yet many closure relations are kept hidden and have to be looked up in other papers. The authors might want to consider to either reduce the model description to one or two paragraphs succinctly characterizing RAMMS::EXTENDED, or to add the missing relevant pieces of information to make the paper reasonably self-contained. (My preference would be the latter if most of the following criticisms are addressed.)

We agree that including the closure relations would enhance the clarity of the RAMMS::EXTENDED model. We will add statements in the main text indicating where closure relations are applied, and provide detailed explanations of these in the appendix. It is important to note that these relations are primarily derived from calibrations based on observed avalanches, representing those that yielded the best results with the data we have measured so far.

R1-5.3: In (Issler et al., J. Glaciol., 2018), several central aspects of RAMMS::EXTENDED were analyzed and shown to have mathematical or physical inconsistencies. One of the issues raised then has apparently been corrected: The gravitational force on the suspension layer is now included. However, the authors' statement that it is generally negligible is not correct: It does indeed apply as long as the dense core and the suspension layer travel together, but in the runout zone of the latter—after the dense core has stopped—and particularly when climbing a counter-slope, this term is of paramount importance if the suspension layer is well-developed. Moreover, the hydrostatic pressure gradient, which drives much of the lateral spreading of the cloud, is due to the excess buoyancy, i.e., to gravity.

We will remove this statement and open a new section in the discussion where we highlight those points to show more which assumptions are taken with this model.

R1-5.4: Another major criticism in (Issler et al., 2018) concerns the equation of motion of the dense core in the bed-normal direction, for which Bartelt and Buser arrived at a third-order PDE in time for k, the z-coordinate of the center-of-mass of (Langrangean) control volumes, with the time derivative $\dot{R}_K$ of the granular temperature, R_K, as the source term. Instead, $R_K/m - g_z$, with g_z the bed-normal component of gravity, should directly determine $\ddot{k}$. I do not expect Perry Bartelt and myself ever to come to agreement on this point, but it would be in the interest of the readers to alert them to this controversy (unless this part of the code has tacitly been changed, in which case it would be important to state this as a modification of the published model).

We will add this discussion point to the same section as described in R1-5.3.

R1-5.5: Equation (7), which describes the height evolution of the dense core, is written as a conservation equation. Since the explicit form of the source term $D(t)$ [actually, it should be $D(x, y, t)$] is not given in (Zhuang et al., 2023a) either, it cannot be determined whether the equation is correct. However, if D comprises just the dispersive-stress terms, Eq. (7) must be written, not as a conservation equation, but as an evolution equation $(\partial_t + u_\phi * \nabla)h_\phi = D(...)$.

Bozhinski and Losev termed this equation a "volume conservation" equation. We explicitly calculate volumes of air that enter the core as the core expands and contracts. The equation tracks the center-of-mass of the granular ensemble. The equation we write is the equation we solve – for us, it is both a conservation (of air) and evolution (of the center-of-mass) equation. We can add this statement in the publication.

R1-5.6 In Lines 216–217, it is stated that the modified Voellmy model arises directly from chute experiments with flowing snow. This statement is misleading for readers who are not too familiar with granular mechanics because it appears to say that this is the correct form of the friction law. In (Issler et al., 2018), it is shown that this empirical model behaves very differently from true granular models based on kinetic theory or from the experimentally well-tested (I) model at very high speeds. In practice, it should be admissible to use this parameterization since there are extra parameters that must be fitted to observations. It is important, however, to be clear about the heuristic nature of the assumptions that are made.

We will rephrase the sentence from "This formula arises directly from chute experiments with flowing snow" into "This empirical formulation was calibrated based on chute experiments with flowing snow".

R1-5.7 I do not understand Eq. (11). For one, the subscript i is not explained; from Eq. (10) in (Zhuang et al., 2023a), one can see that is meant. It is unclear where the time (increment?) t comes from. It is said that "The term $\dot{Q}$ m represents the latent heat of melting ice", but $\dot{Q}$ m is a (latent) heat flux, so there is a mismatch of physical dimension. This passage must be clarified. From Eq. (10) in (Zhuang et al., 2023a), one can see that meltwater production requires $\dot{Q}_m > 0$ and thus $T_\Phi > T$. What seems to be missing is some sort of heat conductivity (which has dimension (time)–1 and allows to get rid of the ominous integral and ΔT). This conductivity will, among others, depend on the mean particle surface-to-volume ratio.

Yes, thank you. The reviewer is correct. T_i should be T_ϕ. T_i represent the temperature of the solid phase. We do not consider heat conductivity as we assume that the frictional heating process (the shearing/rubbing between the particles) and heat exchange with the air occurs at a much higher rate than the conduction of heat. The ominous integral simply states that all the specific heat energy above the melting is available to drive latent heat exchanges in the time interval dt. This is a common approach in numerical codes, for example SNOWPACK which operates on the same principle to determine surface and sub-surface melting in snowcovers.

> R1-5.8 The mass exchange between the dense core and the suspension layer is assumed to be oneway. This is conceptually problematic because the vertical expansion of the core due to increased granular temperature cannot create a vacuum, hence some fraction of the air–snow mixture in the cloud must be sucked back into the dense core. This is discussed to some degree in (Vicari and Issler, 2024). I do not expect this effect to be major on the scale of an entire avalanche event, but the simplifying assumption should be clearly mentioned.

We will add this discussion point to the same section as described in R1-5.3.

> R1-5.9 An interesting and fairly advanced feature of the PSA model is the extra equation for turbulence. However, earlier one-layer 3D models like SL-3D, the two-layer 2D/3D model SAMOS-AT, the one-layer 1D model by Parker et al. (1986), the one-layer mass-point model by Gauer (1994) and the two-layer 1D model formulated in (Issler, 1998) used similar equations. It would therefore be appropriate to point out in which respect the authors' model differs from the older ones.

We will mention the models and add the reference.

> R1-5.10 From Eq. (9) and Line 197, one sees that the random kinetic energy of the dense core receives a positive contribution from entrainment equal to a fraction ϵ_ϕ of the kinetic energy imparted to the eroded mass per unit time, $\dot{L}_{\Sigma\to\Pi}$. In the suspension layer, it is tacitly assumed that the corresponding coefficient $\epsilon_\phi = 1$. The chosen value of ϵ_ϕ is not specified as far as I can see, and no explanation is given why ϵ_Π should be as large as 1.

In the cloud, air entrainment produces turbulence. As a textbook on turbulence we follow the work of Davidson (2015) which show the equivalence of entrainment and turbulence. Following Davidson we make the assumption that entrainment first produces turbulence, and then decays to heat. This was discussed in the work of Zhuang et al. (2023).

> R1-5.11 The entrainment model Eqs. (20)–(23) assumes the entrainment rate to be proportional to the avalanche speed, without any reference to the stresses exerted by the avalanche on the snowcover. This is problematic because a very thin but fast layer (like the "splash" ahead of the avalanche front) would be able to entrain equally much as a 3 m thick flow. I implemented and tested this entrainment model in a code similar to RAMMS::AVALANCHE some ten years ago and found it to give completely unrealistic spreading of the avalanche under certain conditions. There are several papers showing that the entrainment rate should be proportional to the difference between the shear stress near the flow–bed interface, divided by the flow velocity (rather than proportional to it). With the Voellmy friction law, the drag term then indeed produces a contribution proportional to the speed, but only beyond the threshold given by the shear strength of the bed material. Again, I do not mind if the authors use this model presently as long as they alert the readers to its potential problems.

The reviewer is correct. We have a shear stress cutoff to limit the entrainment of small avalanches and will mention this in the text.

> R1-5.12 Apparently, RAMMS::EXTENDED now assumes a linear density profile and a parabolic velocity profile (rather similar to SL-1D and MoT-PSA), but this is not mentioned at all. When comparing the simulated damage areas to the observed ones, it matters strongly whether these profile functions are applied or not, so this must be stated explicitly.

We will add the statement that we take the mean values for the density and velocity profile. The user must post-process the mean values.

R1-5.13 Another issue left undiscussed is the assumption of constant splitting of highly dynamical quantities like $\dot{M}_{\Sigma}$ and several others. Also here, I do not mind if this simplification is made in the present stage of development, but it should be stated that this is not always a realistic assumption. For example, a slowly plowing wet-snow avalanche will have $\gamma \approx 0$ while this parameter might be much closer to 1 in a fast, strongly fluidized flow over fluffy new snow.

Yes, the reviewer is correct. When we simulate more moist avalanches we take $\gamma = 0$. This publication is focused on dry, powder avalanches. We will mention in the text that this is a simplification that we did.

R1-5.14 An important point that is largely shoved under the rug in the manuscript concerns the large number of parameters in the model (many more than listed in Table 2). This is difficult to avoid with such a complex phenomenon, but it needs to be communicated clearly that many parameters, the values of which are poorly known, were kept fixed in this study and only three initial or boundary conditions were investigated. Testing the influence of the other parameters is a task for the (near) future and—in my opinion—a prerequisite for wide-spread use of the scheme in site-specific warning.

We went through a calibration phase with VdlS avalanches to evaluate those values. We can point out in the publication more the parameters which we calibrated. The procedures were presented older publications.

R1-6.1 **Referencing.** I have never before reviewed a manuscript with an equally high degree of selfcitation before—in more than half of the cited papers, at least one of the authors of this manuscript is a co-author. On the other hand, clearly relevant papers by other authors are left out. In the case of the early Russian work, the book by Bozhinskiy and Losev (1998) is cited, giving the wrong impression that those authors developed a powder-snow avalanche model in the late 1990s while it was Eglit who developed a 1D two-layer model with entrainment in the beginning of the 1980s. Her work was recently extended by Vicari and myself, and the corresponding paper (which has been known to the authors) contains a sensitivity study that addresses complementary aspects of the problem and is, in my opinion, quite relevant in this context.

We acknowledge that our current focus has been primarily on in-house literature, as this publication aims to summarize the foundational research behind RAMMS::EXTENDED. Moving forward, we will include a more comprehensive literature review. The work by Vicari and Issler was published two months after our paper was submitted, so while we regret not having access to it at that time, we will certainly incorporate its insights and citation in this publication.

R1-6.2 The authors took a significant fraction of the text in Sec. 3 more or less literally from earlier papers. Those papers are referenced, but the cut-and-paste passages are not marked as quoted text. I do not know how NHESS handles such cases, but several high-ranking Norwegian politicians recently had to leave their government posts and got their MSc titles revoked because of similar plagiarism in their theses. I would therefore recommend reformulating the corresponding passages or marking them explicitly as quotations.

We checked the manuscript using a commercial plagiarism tool and found no flagged passages beside some of the equations. As the paper builds on our ISSW 2023 work, some sections may resemble previous publications due to the logical structure of explaining the equations, but we will revise these sections to avoid any concerns.

R1-7.1 **Minor Remarks** The density of ice is 917 kg/m^3, not 971 kg/m^3. Interestingly, the same typo occurs in one of the cited papers, in a rather identical-looking sentence. Moreover, the authors state that the density of air is 1.225 kg/m^3, but in reality this value varies considerably with temperature, pressure and humidity. Differences at the percent level will have little effect on the simulations, but it would be better to state clearly that a single typical value has been chosen. Please see the annotated manuscript for further small corrections and suggestions.

Thank you very much for pointing out, we can adjust this.

R1-8.1 **Presentation** The manuscript is organised in a clear way and written in good English (mostly). There are, however, many passages that do not seem to have undergone careful review by the co-authors. In most cases, the work done is explained clearly, without being verbose. The main exception to this, as mentioned above, is that the all-important mass exchange terms between the dense core and the suspension layer and between the ambient air and the suspension layer are not specified. This should be added in the revised version.

The figures generally illustrate the points made in the text well, but the following improvements are desirable:

- I did not understand the message of Figure 1 at all. Does it really contribute something essential to the paper?
- In Fig. 2.a, it would help readers if the avalanche deposits were numbered. Also consider replacing Fig. 2.b, which is repeated as Fig. 15.b, by Fig. 3.a rotated by 90° counter-clockwise, so that it aligns with Fig. 2.a. Figure 3.b can stand on its own because it is not visibly related to Fig. 3.a.
- In Fig. 6, d_0^* is drawn in the vertical direction rather than normal to the ground. If this is correct, this should be mentioned in the text, otherwise it needs to be corrected in the figure.
- The exchange term $\dot{M}_{\Phi\to\Pi}$ in Fig. 7 is characterized as "air blow-out". If this were "just air" (to cite the late Othmar Buser), where would the snow in the suspension layer come from?
- Figures 8 and 9 would be easier to read in one-column layout, i.e., reduced in width. Also consider lines connecting the points in Fig. 8 as in Fig. 9. Note that the keys are hard to read.
- The message of Fig. 10 would be easier to understand if all three plots had the same color scale going from $-12\,°\mathrm{C}$ to $0\,°\mathrm{C}$. Make sure the figure keys do not hide the data points (a single figure key outside the plots themselves would suffice).
- Color nuances for values close to 1600 m are hard to distinguish in Fig. 12. A different palette could help. Also, neither the text nor the figure state which value of the run-out distance was actually observed.
- In the legend of Fig. 14, it ought to be mentioned that this is the result of the simulation.
- Readers would be particularly interested in a map of the suspension-layer pressure for the simulation in Fig. 15 (perhaps add corresponding maps for the other two paths as well).
- In Fig. 16, it is unclear for readers whether this deposit comprises only the dense core or includes the suspension-layer deposits as well.

Point 1: as we will take away the focus of this publication from road safety, we can remove this graphic. Point 3: d_0^* is perpendicular to the surface. we will point this out in the text and adjust the graphic Point 10: We will mention that this only includes the deposition of the dense core. Points 2,4,5,6,7,8: We will implement these points to make the graphs more uniform and readable.

R1-8.2 In my opinion, several aspects of the notation are somewhat unfortunately chosen:

- First, the symbol M is strongly tied to "mass" for most readers, but the authors use it for mass divided by the density and unit footprint area, i.e., essentially for a length or depth. This is an unnecessary source of confusion for the readers.
- Another issue, also arising in connection with M, is the use of a dot to denote a rate: It is well established tradition to use the dot to designate a time derivative. But the quantity $\dot{M}_{\Phi\to\Pi}$, e.g., is not the time derivative of some distinct dynamical quantity in the theory but the mass (or rather volume) flux across the boundary between the dense core and the suspension layer. Often used symbols for such fluxes are Q (which might evoke heat, however) and J. This issue becomes particularly visible in Eq. (10), where $\dot{Q}_m$ and $q_{\Phi\to\Lambda}$ both designate heat fluxes.
- The authors use ΔT and ΔD for the temperature and snow-depth lapse rates. The former has units °C/m, the latter is dimensionless. At least 99% of the readers will immediately interpret the expression ΔT as meaning the difference between two temperatures, again with units °C, rather than the intended lapse rate. Therefore, I suggest choosing a different symbol, say a, with subscripts T or d.
- In Eqs. (5), (6), (8), (17), (18), and (20) and also in the in-line formulas in lines 200 and 258–261, there

are superfluous brackets or parentheses around fractions. This is not strictly wrong, but it does not add clarity, just clutter. Moreover, the use of parentheses and brackets appears haphazard. Most authors use brackets only if there already are parentheses in the expression within the brackets.

- The choice of the double strike letter D in Eq. (7) appears poorly motivated and looks strange, in my opinion. Either use a normal D or give a convincing explanation for your choice.

Thank you for providing this valuable perspective. We agree that the current choice of symbols is less than ideal. We will ensure to find a new solution that remains consistent with the notations used in the existing RAMMS literature. Additionally, we will invest more time in providing a clear explanation of the symbols, possibly through an overview table, and make adjustments to improve clarity where possible.

R1-8.3 Besides the major issue of reference selection, the reference list also needs careful improvement with respect to content and formatting: Some references are listed twice, in some cases the journal name or the page numbers are missing, or article titles are poorly formatted. Please add DOIs where available (this is the case for most articles except ISSW proceedings, where a URL can be supplied instead).

We will clean this up.

R1-8.4 Unless this is taken care of by Copernicus during copy editing and typesetting, the authors need to carefully review the manuscript, distinguishing between hyphen '–', en-dash '—', em-dash '—' and minus sign '−'. Physical units should be written with upright letters, and consistent and correct spacing between the number and units should be applied.

We will review the guidelines again and make the necessary adjustments.

Language

We will implement all proposed changes to language, grammar, punctuation and almost all proposed details to be added. For brevity, we only list and comment the few other recommendations herein:

- Adjustment of the naming of locations and avalanche paths according to Swisstopo names
- Add a table characterising the observed avalanches with maximum powder height etc

References

Davidson, P.: Turbulence: An Introduction for Scientists and Engineers, Oxford University Press, Oxford, UK, 2nd edn., doi: 10.1093/acprof:oso/9780198722588.001.0001, 2015.

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.