Answers to reviewers on "OpenFOAM-avalanche 2312: Depth-integrated Models Beyond Dense Flow Avalanches"

The original authors

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The mayor concerns are addressed in the author replies. Here follow the answers to the comments that were embedded in the manuscript. Following the reviewers request, we just comment to the annotations that we did not implement. Comments are shown in blue, our answers are shown in black, line numbers refer the old version.

Line 1: This reads more like the beginning of the introduction or an abstract sent to a conference, where it is important to give background for non-specialists. Here, I would prefer a text that comes to the main points quickly and gives more technical information. For example, you do not mention that this concerns a two-layer model derived from the Savage–Hutter and Parker–Fukushima–Pantin models, that it includes entrainment and deposition and that you carry out comparisons with the PFP model and back-calculate two observed mixed avalanches.
The current version aligns with the journals guideline.

Line 22: A much earlier, conceptually simpler and less controversial example would be Eglit's (1982) model
We could not find this paper.

Line 31: It could be illunimating for many readers if you inserted the expressions for inertia and drag force before simplifying to the final result.
This would exceed the scope of the intrudction, since expressions for drag and interia have to be combined with assumptions of lenght and time scale.

Line 44: It is true that Sovilla et al. provide experimental evidence, but this fact has been known at least since the 1950s and probably even the 1930s. If I remember correctly, Hopfinger (1983) pointed this out as well.
We agree that Sovilla et al. are neither the only nor first to point this out. However, we

think this publication seems to be best suited to present the various regimes with many high quality figures.

Line 47: I would not consider the incompressibility as a characteristic feature but rather the collisional nature of the granular flow and the (perhaps) negligible role of interstitial air. For example, SL-1D (Issler, 1998), which is still being used by practitioners in Switzerand, treats the fluidized layer as a variable-density fluid and couples it with the variable-density suspension layer.
Dense granular flows can be simulated with incompressible models (see, e.g. Rauter, 2021), so it makes most sense for us to use such a model. We don't see the upside of a compressible model.

Figure 1: $\phi_s = 0.05$ corresponds to $\rho \approx 47 kg/m^3$. u = 100 m/s is close to the maximum of credible values, most PSAs move much more slowly. The maximum observed at Ryggfonn is around 60 m/s. However, the internal velocity a little above the dense-flow–suspension-layer interface could then reach about 100 m/s, as suggested by the lab experiments by Keller (1995). The height of the 1995 avalanche at Scex Rouge in Switzerland (Issler et al. 2020) reached about 400 m, as estimated from a photo, but in most cases the height remains below 100 m. At Vallée de la Sionne, the cloud height near the front was often about 20 m, with the very dilute wake attaining larger heights (at very low velocity). I would recommend replacing the single numbers in this figure by typical ranges, avoiding extremes. Perhaps mention that more extreme values have been reported.
Thanks a lot, we corrected some of them. It felt misleading adding full ranges for all properties, e.g. for height and velocity, which can potentially be 0.

Line 61: Your choice, but I find it fairly tiresome to read. Moreover, the choice of Greek letters Λ and Σ might be understandable if one thinks of German "Luft" and "Schneedecke", perhaps Φ stands for "Fließlawine", but what about Π and what about non-German readers? Earlier authors used notation that is more convenient IMO.
I always thought of Λ being similar to A for air and Σ standing for static. Φ (often the index for the granular phase in granular flow models) and Π standing for the two moving grain layers. Hard to come up with something better and it is good to be in line with something readers might already know.

Line 81: A somewhat haphazard citation...
We cite the publication that we think is most useful for readers to follow up. We think this is a very good book on the Navier-Stokes Equations and CFD.

Line 86: Not a compelling citation either, since this has been discussed many times in the past century.
We cite the publication that we think is most useful for readers to follow up. This publication deals with different ways of describing multiple phases in CFD and seems to fit well here.

Line 93: Better: "tractable"
Not changed.

Line 95: How about a reference closer to the invention of teh technique?
We cite the publication that we think is most useful for readers to follow up.

Line 114: In which sense is there a disparity?
The theoretical description differs from the implementation. Details can be found in the cited paper.

Line 115:Are there really "excessive" curvature terms in those models? In my opinion, there usually are too few...
Yes, exactly. Since there would be an excessive amount of terms, people neglect them in practice, often without explicitly mentioning that.

Line 116: Isn't this what (commercial and non-commercial) CFD programs have been doing since the 1980s???
Not for depth integrated models.

Line 117: I do not quite understand what you want to say here, this seems like you are going in circles!?!
People often think we speak of Navier–Stoke type models when describing the solution in 3D space (see also your remark above). This sentence should emphasise that we are still speaking about depth-integrated models.

Line 123: One can argue that the effect of the centripetal force on the shear stress is of the same order as $u^2 \approx gh$. If you want to present a reasonably rigorous derivation of your model, you would need to explain why you are entitled to neglect one term of O(h/R) but keep the other one.
We refer to existing works that discuss this aspect.

Line 157: I feel that introduction of the surface-aligned coordinate system merely adds confusion. Since derivatives in the x' and y'-directions do not appear in the text, you could instead integrate $\psi(x_b + n\,t)$ over some parameter t in the direction n. In (13), you effectively do that, so you just neeed to change the notation slightly.
We wrote it like this in Rauter and Tukovic (2018). Now I think that notation is misleading and choose to not use it.

Eq. (17): Not explained nearby! Why not call it S^h_Φ?
Explained a few lines below. S^ϕ_Φ because only the grain volume is considered in this flux to be in line with the suspension flow model.

Line 217: from the velocity profile of a stationary solution. It takes the value $\xi_\Phi = 5/4$ for a Bagnold profile appropriate for a granular flow on an infinite inclined plane.
We think the short mentioning is sufficient.

Line 222: I do not agree: h^2 in the denominator makes a significant difference. This friction model is IMO more closely related to the Norem–Irgens–Schieldrop model (Norem et al., 1987, 1989). I would also like to suggest to use a parameter $\psi = 1/\chi$ instead so that this term can be switched off cleanly if so desired.
NIS has the additional cohesion term, which makes a big difference, both in terms of requirements for the numerics and results. The used relation behaves very similarly to

the Voellmy relation, as we found out in Rauter et al. (2016). There are other friction models in the code to use Coulomb friction only.

Line 234: Note that this entrainment formula predicts an entrainment rate *growing* linearly with the flow velocity u if the bed shear stress is kept constant and e_b is a constant. However, it has been shown by several authors that the entrainment rate must *diminish* as 1/u under these conditions because the eroded material must be accelerated to u. This could be fixed by adding $u^2/2$ to e_b.
We agree that the entrained snow masses have to be accelerated to the avalanche velocity. This is taken into account, at least to some extent, in the conservation equations: If the flow depth h grows due to entrainment, and the momentum hU is conserved, the velocity U decreases. We are not sure to which extent this covers your suggestion. The reasons for using this relation is given in the previous author response.

Line 263: It does hold when $\phi_\Pi < 10^4$, which is the case in the early and late phase of a PSA or if the PSA does not fully develop.
But not in general so we have to consider it.

Line 270: volume, mass
We don't see the need to mention volume, as volume conservation is only mass conservation with incompressibility.

Eq. (31): Since I do not agree with Eq. (9), I also object to this equation. Also, the nabla operator should be replaced by its projection along the bed-normal direction. Instead of n · [g ∇_n · (u u)], it should be written [...] · n because n · · f is not possible.
We removed Eq. (9) and reformulated the respective section, so also this equation should be fine now.

Line 328: Are there subaerial turbidity currents???
Considering the context of the paper, one could think of powder snow avalanches as subaerial turbidity currents. Anyway, we use subaquatic here to note that it is a classic "water and sediment" turbidity current.

Line 351: There is a rather important difference, however: Bartelt et al. (2016) describe a variable-density non-suspended flow, i.e., the lower layer can go back and forth between the dense and fluidized regimes.
Bartelt et al. (2016) seems closest from all models that have practical applications, at least at the time of writing this model and manuscript.

Eq. (46): This statement seems to imply that settling from the suspension layer creates a (often very thin) dense-flow layer because it cannot deposit directly on the snow cover. This would seem to make the notion of dense-flow runout rather tricky. This must be explained properly.
This is correct, the distinction is not tricky, as the depositions can be easily distinguished by their height, see simulation examples.

Line 386: This simplification is acceptable since you mention it. However, your argu-

mentation does not take into account that the density at the botom of the suspension layer may well be ten times the density of air. What saves your day here is the tendency for the relative velocity between the layers to be smaller than the dense-flow velocity.
The density would still be small compared to the dense-flow, and, as you say, the velocity difference is expected to be small as well.

Line 374: I agree, but why do you account for it here but not in the suspension-flow model?
It is only applied to the cross-layer flux to limit unrealistically high phase fractions (even bigger than 100%) when the suspension layer is initiated by the dense flow. Such a situation is not possible in the suspension-flow model because the suspension is always picking up sediments itself and must therefore already contain some fluid to pick up the particles.

Line 392: There is one cross-layer coupling that you do not mention but is taken into account in many models since 1982: the weight of the powder-snow cloud increases the bed friction of the dense layer. This is a minor effect in the start but can be substantial under some conditions. It is OK to neglect it, but you ought to mention this.
We are not aware of such models. The powder cloud does not increase the friction of the dense-layer. The ice particles in the powder cloud are suspended so they do not contribute to effective pressure (e.g. Rauter 2020), which is the pressure that is relevant for granular bed friction.

Line 396: In the fluidized front of dry-snow avalanches developing a significant powder-snow part, small particles appear to be quite abundant, though. See (Issler et al., 1996, 2020) for more details.
Small particles have to be present in the dense core before they can form the powder cloud, we think that is clear.

Eq. (53): I_0 is often used as a parameter in the (I) rheology, so it might be wise to choose another index for this quantity.
The similarity is chosen on purpose.

Line 474: I do not understand the purpose of this.
Might be helpful for some readers.

Line 481: Better: (Rauter and Tuković, 2018) if you don't want the readers to contact you directly... Similarly for the other references in this sentence. We think the citation style is correct here but will be checked anyway by the typesetting team.

Line 487: in the sense of ensuring that the equations are solved correctly.
The chosen wording comes directly from the cited paper.

Line 524: Perhaps better: "chosen as"
Left as is.

Line 524: This is a monster avalanche...
The release volume is indeed very big but there is no entrainment, so the total avalanche size seems reasonable to us.

Line 537: This may be true for Austria but is not the case for the hazard zones in Switzerland elaborated by the reviewer between 2000 and 2006, using SL-1D and the default velocity profile derived from laboratory experiments by Keller (1996).
This refers to the equation for the dynamic pressure, not the governing equations. We could not find any guidelines or literature with a shape factor is used in the calculation of the dynamic pressure.

Line 555: Your assumptions for the initial conditions are well beyond what is observed, except perhaps in the Himalayas. Therefore it is hard to draw comparisons with real cases. In realistic situations, the powder-snow cloud often generates an extended "yellow" zone according to Swiss terminology ($p < 3$ kPa), larger PSAs may extend the "blue" zone ($p <$ 3–30 kPa depending on avalanche frequency). I cannot remember having encountered a case where the cloud extended the "red" zone ($p >$ 3–30 kPa depending on frequency). The avalanche released artificially in Vallée de la Sionne on 1999-02-25 exerted a pressure of about 50–70 kPa at the bunker on the oposite slope, but the culprit was not the suspension flow but the fluidized front, which you do not model explicitly.
The release volume is indeed very big but there is no entrainment, so the total avalanche size seems to be fine. Your description is in line with the two cases in the paper. In the avalanche intensity scale of Rapin are powder snow avalanches mentioned with > 10 kPa when they destroy forests. Also some of the observed snow avalanche damages require > 6 kPa, e.g. damaged roofs. Therefore it seems reasonable for the powder cloud to extend the 10kPa zone in some scenarios.

Table 3: Set to infinity?
Erosion was not active in this simulation as stated in the text. This can be achieved by setting the entrainment model to "NoEntrainment" in OpenFOAM. Here this is indicated by giving no parameter.

Line 594: Is there any information available on this matter? Based on my experience, this looks very plausible, but then the fluidized layer is not explicitly included in the model.
If c_D is too small, the powder cloud completly outruns the dense core. We find this unrealisitic as we would see the dense core running after the powder cloud in experiments and real-case avalanches.

Fig. 8: What do these blobs signify?
They are not there when processed with ParaView, so it must be a post–processing issue. We left them in the image because we don't think manipulating result plots is ok. Notably the flow fields are a bit weird in the starting region (numerically vanishing powder cloud height, thus small fluxes change properties drastically).

Line 604: This value is, IMO, at the upper edge of the plausible range and would depend on when the density was measured and what the temperature was. One could of course argue that this is not the issue here, but the mass balance of the dense part plays a considerable role for its dynamics if there is a high degree of suspension. It would therefore be good idea to spen a sentence or two on the degree of entrainment (the same

holds for the Eiskar avalanche).
Yes, it is on the upper edge, and so is the flow density. Anyway, it is convenient to get a round number. More precision would indicate an accuracy that's not there. This is just a rough value that explains the difference between the simulated and the documented deposition height which is roughly a factor of 3.

Line 607: This is not true, see e.g. (Issler et al., Geosci. 10, 2020).
Regarding "as no clear deposition pattern emerges from suspended flows". I am not sure if we looked at the correct publication, but in "The 2017 Rigopiano Avalanche—Dynamics Inferred from Field Observations" we didn't find any information on deposition patterns of the powdercloud, only other traces that also we use as proxies. Anyway, we think this holds, as people documenting these events can usually not delineate the poweder cloud extend with deposition. The deposition is further much more nuanced.

"...), stopping"
Left as is.

Confusing: one might understand this as saying that the dense-flow pressure exceeded 10 kPa, even though column 3 says p = 0 for the dense flow. Given the PSA pressure 3 kPa, please indicate the type of building. If these were masonry buildings that were utterly destroyed, the PSA pressure would likely be underestimated. These are very valuable constraints on the model. To withstand a pressure of 30 kPa, buildings usually must be built in reinforced concrete. Any information available? Not that trees often are broken or uprooted when hit by a PSA with this pressure. These are comparisons of simulated pressure with observed damages. So they do not match perfectly, as also described in the text.

I do not understand this statement very well. Presumably you mean that the model does not predict high pressures where the degree of observed damage precludes high pressure?
Yes, the area of "false positives" is small.

Fig 12: Is there observational evidence for this branch?
This branch is not documented in the official report, the aerial pictures start right below the branching. However, this branch is present in simulations with SamosAT, which is at least some evidence.

Line 659: I believe you mean that the parameters had to be chosen quite differently from the standard values used by WLV.
We fitted the parameter to achieve a match with the observations and they were different between the events. We didn't try to run it with values used by WLV.

Line 727: You do not mention another, usually even stronger approximation due to the height of the PSA cloud often approaching the curvature rasius of the terrain: g_{eff} is not constant throughout the depth of the flow. I concede that this is difficult to capture in a depth-averaged model, but it ought to be mentioned.
This would be true if the flow is really surface aligned meaning that all streamlines are surface tangential. However, I don't even think that this is the case on strongly curved

terrain as eddies will form. Since this seems quite far stretched, we did not include it in the manuscript.