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Interactive comment on “Forest damage and snow avalanche flow regime” by T. Feistl et al.

Anonymous Referee #1

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In this manuscript tree destruction is investigated, assuming different snow avalanche flow regimes. The authors show that it is crucial to not only take impact pressures but also bending stresses into account, when considering forest damage. To quantify the forest destruction, tree bending strengths are considered and compared to bending stresses defined for the different flow regimes. One result is that bending stresses basically only differ with respect to affected area and the bending moment (impact height) for most flow regimes (powder, intermittent, dense). For dense / wet snow avalanches, bending stresses determined by this approach appear to be too low, for this reason pressures exerted by the avalanche and the corresponding bending stresses are determined by static approaches. Necessary input parameters for the stress calculations are determined with an avalanche simulation tool (RAMMS). A unique and valuable data set of observed avalanches and corresponding forest damage is used to evaluate the modeling results. However, some of the authors' argumentations

C156

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Interactive Discussion

Discussion Paper



Interactive
Comment

(e.g. choice of avalanche simulation parameters / taking simulation results for granted; or that is appropriate to take static pressures into account for wet snow avalanches) and conclusions (e.g. to ignore the intermittent regime or that the standard approach (what is *the standard approach*?) does not lead to satisfying results) are questionable.

In their analysis the authors inherently assume the validity and accuracy of the simulation results (especially velocity) and e.g. draw the conclusion that *standard* simulation approaches can not capture the forest destruction (or is this actually referring to the difference between impact pressure and bending stresses?). However, the possibility to utilize the forest destruction observations as validation data for the simulation results is not taken into account. That would probably lead to the conclusion that simulated avalanche velocities are too low along the path (which is in correspondence to observations of others) and would also be a legitimate conclusion. Generally, the description of the employed avalanche simulation model(s) are hard to follow (employed parameters, implementation of entrainment / detrainment, model of Bartelt et al. 2015 (which is not available yet)). Furthermore the authors state that Eq. 6 (*the standard approach*?) is only valid for $Fr > 1$ but conclude later, that it is not applicable for slow flows (prob. accompanied by large flow depths?), which seems contradicting to me. In section 2.3. an ad hoc magnification factor D is introduced, which lacks a proper description of implementation (is it actually used it in the analysis?) and seems rather random to me.

Overall the manuscript is reasonably well written and enjoyable to read. However, some important terms would need additional clarification (e.g. difference between standard / impulsive / (quasi) static / dynamic / impact pressures, wet flow / slow drag flow regime). I also found several typos and formulations that should be double checked by a native speaker / typesetter.

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Line-by-line comments:

- 537, 16-18 '*The effect of tree breaking can..*'. You mean tree flow resistance? Can it really be parametrized like this (Is this not contradicting your own observations that other effects need to be included [Feistl et al 2014])?
- 539, 6-8 '*When the spacing ...*'. I do not understand. Uniformly along what: the flow depth? And is the pressure distribution not also dependent on the velocity and the vertical velocity profile, respectively?
- 539, 17-19 '*The impact pressure ...*'. What do you mean by impulsive?
- 541, 8 '*.. flow regime.*' How does c_d relate to the flow regime? Is the flow regime not rather related to the Froude number ($Fr > 1$) as stated above? And should this maybe also be an indicator for your validity of Eq. 6, when you start using the static pressure approaches, rather than just the velocity? Stating that Eq. 6 is only valid for $Fr > 1$ and concluding later that it is not applicable for slow flows (prob. accompanied by large flow depths?) seems contradicting to me.
- 541, 10-12 '*.. slow drag flow regime.*' This sentence is confusing, what do you mean by slow drag flow regime?
- 541, 15-24. How do you define dynamic in this context? You should clarify: e.g. slow drag, wet, (quasi) static, dynamic, impact, ...
- 544, 19-21 '*We therefore ...*'. This explanation is not satisfying: besides average density both, velocity and impact height enter the bending moment M_g (even quadratically) and should be higher for the intermittent layer than for the dense flowing core.
- 545, 6. How would fluidization be the relevant process for this variation?

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- 546+547, Eq 22+24. What would be the impact height h_a in these cases? Fig 5: did you find any field observations of snow accumulations like this in front of a (broken) tree (also to justify to justify the static approach and the assumptions for the parameters l_v and Ψ)?
- Section 2.3. Are you actually applying this magnification in your calculations?
- 548, 1-8. I find the values for D a bit low Considering $D \approx \rho_w / \rho_{flow} = 2 - 4$
- 548, 12-14. You mean the effects priorly described or what is a *second order bending effect*? Would this would be $D = 1.1 - 1.2$?
- 549, 1-22. I think the tree breaking is very important for your paper and should be an extra section, e.g. 2.4. *Tree breaking*. Is the tree strength σ independent of any other tree parameter (size, age)?
- 549,8 '...vary... if the load is applied dynamically or statically.' If the values vary for dynamic and static case - why do you not take this into account in your analysis - since you also look at a static case for wet snow avalanches.
- 549+550, section 3.1. I was not able to find the paper Bartelt et. al 2015 (N/A yet). For this reason it is very difficult to follow this section. Generally i think you should enhance this section and describe in more detail what parameters, which RAMMS version, which stopping criterion, entrainment/detrainment, path characteristics ... you used. Do you take into account that the modeled flow depths are measured normal to the surface, while your impact heights are in direction of gravity?
- 551, 2 'velocities, flow heights, ... to resemble the real avalanche flow.' The velocities have been checked for one wet snow avalanche run out area ($< 5m/s$) - I think you cannot draw this conclusion (especially not for the dense dry flow).

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- 551, 11 How did you estimate h_ϕ and h_a for the CPM and SBM model (see comment above)?
- Table 4, Fig. 7. How do you justify the use of totally different Coulomb friction parameters in your different models (e.g. $\mu_{RAMMS} = 0.55$ vs. $\mu_{SBM} = 0.1$) - with this parameter choices your conclusion 536, 16 '(3) quasi-static pressures of wet snow avalanches can be much higher than pressures calculated using dynamic pressure formulas' is not surprising. How is the impact pressure in tab. 4 calculated?
- 554, '...dynamic drag terms.' What are dynamic drag terms?
- 555,21 - 556,6. This paragraph is a very clear and well written summary of your work and should be highlighted (abstract), e.g. *Although the applied impact pressures can be small, bending stresses in the stem can be large due to the torque action of the blast.*
- 223. 7-16 See comment above, also velocity and impact height should be taken into account when comparing to dense core.

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