Dear Referee 1,

thank you for the careful reading of our article. We highly appreciate your comments, which will help to clarify and improve this manuscript. Below you find the point-by-point replies to your comments.

General comments:

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.

>> We stress that in the Monbiel example we use actual observed velocities (Sovilla et al. 2012, Vera et al., 2015). These velocities were extracted from video recordings. In the Täsch example we also have a video recording, including the height of the powder cloud. This also places limits on the avalanche speed and validates the simulation results. Of course, the data is not exact, but it allows the exclusion of high velocities in the runout zone of Monbiel. In Täsch the immediate post-event examination of the avalanche deposition field resembles the simulation results. Therefore, we do not rely exclusively on avalanche simulations to validate our calculations. Instead, we selected example problems where we had reliable evidence of the avalanche and used the evidence to validate the simulations. We do not inherently assume the velocity in simulations to resemble the truth.

In the Bavarian events we had no information concerning the avalanche velocity, but considerable evidence concerning the runout, forest destruction and snow conditions. In these cases we applied the Voellmy model using parameters for extreme events and therefore calculated the extreme (maximum) velocities. If anything, the velocities are too high in these calculations. The Bavarian events were used to predict the observed forest damage, using the model parameters derived from the better documented avalanche events.

In Täsch we tested the RKE model of RAMMS which provided realistic flow velocities in various case studies (Bartelt et al. 2011, Bartelt et al. 2012). The RKE model, in general, produces velocities that are higher than the standard Voellmy model (for the same runout distance).

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)).

>> The forest interaction model has been published at length by (Feistl et al. 2014; Teich et al. 2014). The model with variable flow regime based on avalanche temperature has been published by Vera et al. 2015 and the cohesion model by Bartelt et al., 2014 (We will include the citation of this proceeding in the text). The influence of cohesion on the model results is small, especially for the dry snow avalanche case.

In our presentation, we did not want to stress the simulation models, rather the different calculation procedures for the impact pressures, which can be applied independent of the simulation model. In this paper, we want to propagate the idea that to accurately predict impact pressures requires the consideration of the avalanche flow regime. Therefore, we wanted to make a short, concise presentation of the model, stressing the pressure calculation for the four different flow regimes.

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.

>> We reread the text and we think we created a misunderstanding by citing the Froude number. What we call the standard model is used in calculation guidelines in Switzerland and elsewhere in Europe. This equation is simply $p = c_d \rho u^2/2$ (Eq. 6). We find this formula can be used for flowing avalanches with Fr > 1. We do not believe this formula can be used for Fr <1 without applying large c_d factors. Our conclusion is that wet snow avalanche impact pressure cannot be explained with Eq. 6 without assuming unrealistic values for c_d. That is in accordance with other studies (Salm et al. 1990, Johannesson et al. 2009, Sovilla et al. 2010). We will delete '(Fr>1)' to prevent misunderstandings. However, our biggest problem is that we think Eq. 6, a dynamic pressure formula, **should not** be applied to predict quasi-static pressures with unrealistic c_d values. In the limit, when the velocity goes to zero, the pressures can be high because of the static loading. We therefore have a problem with the underlying physics of the guidelines. There are better approaches, and we show that these can be applied to avoid non-physical solutions.

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.

>> The magnification factor D is used in our calculations. The magnification factor is based on additional loadings (1) snow on branches which increases load asymmetry, (2) low-lying branches that increase the impact area in comparison to the stem diameter, (3) large woody debris hitting tree stems, (4) snow on the ground which increase the torque arm and (5) inertial effects of the impulsive loading. These effects exist and should not be ignored in the calculations. In the paper, we are able to quantify them and show that they all increase the

bending stress. Although it is ad-hoc, we think the reader should be aware that these effects exist. Further validation is necessary here.

Technical corrections:

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])?

>> In this paragraph we highlight two possible approaches how to parameterize forest effects on avalanche dynamics:

1. If trees are broken, overturned and detrained, the turbulent friction is increased (Bartelt and Stöckli 2001, Christen et al. 2010a).

2. If trees remain rigid obstacles, snow is detrained which leads to deceleration and runout shortening (Feistl et al. 2014b, Teich et al. 2014).

These are called friction and detrainment approach, respectively. In Feistl et al 2014 we do not question the friction approach if trees are broken and entrained. Therefore we think that this sentence is fine here.

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?

>> In avalanche modeling the snow flow is described as a depth averaged continuum which is a simplification but a good approximation for dense flows. The density, velocity and consequently the pressure is then uniformly distributed along the flow height (see Figure 2). In the streamwise direction the density and velocity can also vary. To clarify this we will change the sentence to: '…uniformly distributed along the flow height and defined by the bulk flow density $\rho_\ominus^d$ and the depth averaged velocity u'.

539, 17-19 'The impact pressure ...'. What do you mean by impulsive?

>> The standard impact formula assumes that the force acting on a rigid obstacle is derived from the change in momentum. We call this impulsive loading. This is the dynamic pressure. We derive the dynamic pressure formula for granule impacts.

541, 8 '.. flow regime.' How does cd 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.

>> The Froude number is only in part a description of the flow regime. The flow regime is not only defined by avalanche velocity and height (Fr) but also avalanche density. The Froude number is misleading at low velocities because the avalanche pressures are given by the quasi-static pressure which depends on the terrain surrounding the obstacle. Moreover, the Froude number describes the avalanche, but not the terrain (e.g. roughness, slope angle) around the obstacle.

We cite the Swiss guidelines on avalanche dynamics calculations and the report from the European commission on the design of avalanche protection dams here (Salm et al., 1990 and Johannesson et al. 2009). They state that cd accounts for the obstacle geometry and flow regime. In the guidelines values for cd between 1 and 6 are proposed to account for various obstacle geometries and dry, saltation-like or wet snow layers. As stated above, our static pressure approach does not depend on the Froude number, rather terrain, avalanche size and flow regime. We include the citation (Salm1990, Johannesson2009) at the end of this sentence and delete '(Fr>1)' to clarify, that this is a citation.

541, 10-12 '.. slow drag flow regime.' This sentence is confusing, what do you mean by slow drag flow regime?

>> We also do not like the term 'slow drag flow regime'. It does not reflect the physics of the problem, which is better expressed as a 'quasi-static' flow regime or 'static' pressures, according to the European guidelines. 'Drag' suggests a velocity around a stationary object. Unfortunately, the term 'slow drag flow regime' has been introduced in the literature. We will gladly change the expression 'slow drag flow regime' to 'quasi-static flow regime' or 'static pressures' throughout the paper, in agreement with the European guidelines.

541, 15-24. How do you define dynamic in this context? You should clarify: e.g. slow drag, wet, (quasi) static, dynamic, impact, ...

>> We will try to clarify the terms static, dynamic, impulsive, slow drag flow regime. Your remark that these terms are confusing is right and we are thankful that you mention it. Flows calculated with equation 6 will be termed dynamic and flows calculated with equations 19 and 23 will be termed quasi-static throughout the paper. We no longer use the terminology slow drag, see above.

544, 19-21 'We therefore ...'. This explanation is not satisfying: besides average density both, velocity and impact height enter the bending moment Mg (even quadratically) and should be higher for the intermittent layer than for the dense flowing core.

>> For the same impact velocity and the same impact height, we showed that the dense flowing core produces the highest bending stresses. The dense flowing avalanche calculations are based on a mean density, whereas the intermittent pressures are based on hard singular, granular impacts that depend on the granule density and the no. of hits per unit time (bulk density of the flow). Of course, the bending stresses from the intermittent regime can be the controlling process if the velocity of the intermittent layer is much larger

than the core. The flow height of the intermittent layer is larger than the flow height of the dense core, but as the single granules hit there is no stagnation depth additionally added to the flow height. Impact height ha is considerably larger than flow height h_Φ in fast moving dense avalanches. Single granules are certainly faster than the average dense flowing core but we question that the overall velocity is higher. Besides in avalanche hazard modeling maximum impact pressures are always calculated for the dense flowing core. The formulas derived for Mg and M_Φ are similar except for the density. If $\overline{\rho}_g > \rho_\Phi$ then $M_\Phi > Mg$!

545, 6. How would fluidization be the relevant process for this variation?

>> Fluidization implies a change in the mean density of the flow. Avalanches that are fluidized (powder avalanches) have smaller flow densities in comparison to non-fluidized avalanches. Avalanche fronts can be fluidized, whereas avalanches tails are often dense.

546+547, Eq 22+24. What would be the impact height ha 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 lv and ψ)?

>> ha is slightly higher than h_Φ in the wet snow case due to the generally small velocity. It is calculated with equations 17 and 18.

We did not explicitly observe accumulations of snow behind trees in the presented manner. We assume snow pushing on obstacles along force chains that develop behind obstacles as observed in granular experiments (i.a. Geng 2005). We will include a sentence in the first paragraph of section 2.2.5 to clarify this. Force chains develop randomly with differing opening angles and changing volume lengths. An exact definition of these parameters is not possible to date as observations are missing. In this article we present a simplified calculation approach that captures the physics but has to be validated in the future. Besides accumulations are not similar to the amount of snow that is pushing on a stem. We calculated bending stresses for certain values for lv and ψ. These values are not validated yet (see 'conclusion'). What we want to emphasize is that the approach is generally providing bending stress values in the right dimension in comparison to the dynamic pressure approach.

Section 2.3. Are you actually applying this magnification in your calculations?

>> We applied a magnification factor D = 4 for evergreen trees and D = 3 for leafless trees.

548, 1-8. I find the values for D a bit low Considering D _ _w/_flow = 2 – 4

>> You are right if assuming the density of snow compared to the density of trees. But if you assume a mixture of snow and trees you end up with values for D between 1.5 and 2.

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?

>> Yes, D would be betwien 1.1 and 1.2 (see Peltola et al. 1997). Second order bending effect is the effect of snow on branches increasing the downward weight of the tree if already slightly bent by the avalanche.

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 _ independent of any other tree parameter (size, age)?

>> We will include a separate section as suggested. The authors we cite in this paragraph do not distinguish between age and size of trees. We assume wood bending strength to be independent of age and size.

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.

>> Good point. We still believe that tree pulling experiments are closest to the avalanche impact scenario.

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?

>> The avalanche in Monbiel was modeled by Vera et al. 2015. We took their simulation results and did not change any parameter. To discuss their parameter choice is out of the scope of this article. We therefore refer to Vera et al. 2015. Small changes of the parameter values did not change our general result, that dynamic impact pressures cannot explain the forest damage in Monbiel.

For the avalanche simulation in Täsch we provide the reader with detailed information on the chosen parameter setting (Fig. 7). We will include information about the assumed entrainment (0.5m in an elevation of 2500m decreasing by 10cm every 100 height meters, velocity driven entrainment law with κ=7 (frontal ploughing, Christen2010) and snow density of 200kg/m3). The same entrainment law was used to calculate the avalanches in Bavaria with a reference altitude of 1500m. The stopping criterion does not significantly change the simulation results and was set to 5% of the moving mass. As indicated in the first paragraph of section 3.1 we used the latest RAMMS version with the mentioned model extensions. The basic implementation of cohesion as described by Bartelt 2015 is also presented on the RAMMS

homepage:http://ramms.slf.ch/ramms/index.php?option=com_content&view=article&id=57&Itemid=74 homepage and in the proceedings of 'Numerical Methods', Bartelt 2014 ('Numerical simulation of snow avalanches: Modelling dilatative processes with cohesion in rapid granular shear flows'). The mentioned article (Bartelt2015) will be published in the near future. Cohesion does not change the calculated velocities and flow heights significantly. For the avalanche in Täsch we chose c=0 as the avalanche consisted of dry cohesionless snow. We do not want to extend this section too much as the applied model is described in various articles.

Yes we take the slope angle into account (see Eq. 1 and line 20/21 on page 539).

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).

>> In this paragraph we are talking about this one wet snow avalanche in Monbiel, where the velocities of the simulation resemble the observed velocities. Vera 2015 presented many example cases that proof, that their wet snow modeling approach is applicable. That is not part of this study where we just use simulations that resemble the real avalanche flow. On which studies is your assumption based on, that the simulation results of Vera 2015 are questionable? Observations of the flow velocity in Monbiel proof that the simulations are reasonable (Sovilla2012). We do not draw the conclusion that all simulated velocities of avalanches are true. But models are often the only way of gaining knowledge on avalanche velocities, pressures and flow heights.

551, 11 How did you estimate $h\_$ and ha for the CPM and SBM model (see comment above)?

>> The impact height is based on the flow height of the avalanche. See Eq. 17, Eq. 18.

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?

>> In the extended RAMMS version the Coulomb friction changes from 0.55 to values close to 0.1 that explain wet snow avalanches moving on very flat terrain (Vera 2015). The Coulomb friction for the SBM is based on these values and on the study of Feistl et al. 2014a. We assume the values for μ to be between 0.1 and 0.2.

The impact pressures in Table 4 are calculated with eq. 7, 15, 19 and 23.

554, '...dynamic drag terms.' What are dynamic drag terms?

>> We no longer use the term 'drag', see above.

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.

>> We will change the abstract according to your comment.

223. 7-16 See comment above, also velocity and impact height should be taken into account when comparing to dense core.

>> see comment above