Dear Reviewer,

thank you for your careful review which will help us to improve our manuscript. We highly appreciate your constructive comments. Regarding your concern about the insufficient description of the physical basis of the avalanche model incorporated in the evaluated simulation software RAMMS: We think that this concern is more related to the Paper published by Feistl et al. (2014) and we prefer to not give more details about the modeling approach in the present paper. The interested reader can study physical explanations in detail in Feistl et al. (2014), which is cited throughout the present manuscript. The subject of the present paper is an evaluation of the modeling approach and should highlight the practical application of the simulation software for simulating small- to medium scale avalanches in forested terrain. We hope that you agree with our argumentation.

In the first part of our following reply we address your general comments and concerns (not listed here); in the second part your "specific comments" are addressed point-by-point.

Reply to GENERAL COMMENTS:

>> Regarding your concern about a failure in the description and implementation of the model approach: In the code, we extract momentum when detraining. That is, we solve the equation:

$$h\frac{\partial \mathbf{V}}{\partial t}+h(\mathbf{V}\cdot\nabla)\mathbf{V})=\mathbf{G}-\mathbf{S}-\frac{1}{2}\nabla(g_z h^2)-\dot{h}_d\mathbf{V}$$

In the text of this paper as well as Feistl et al. (2014), we often mention detrainment in the sense of removing mass from the flow. You are correct, if mass is extracted it will lead to spurious "accelerations" similar to the rocket equation (see discussion paper of Erlichson, 1990). However, in the implementation of the model we do account for this non-physical behavior by not allowing accelerations when detraining mass. We will state this clearly in the revised version of the manuscript.

You would like us to include an additional "drag" force to model the effect of trees. This drag force would be proportional to the velocity squared of the avalanche and the drag coefficient. You argue that the magnitude of the force is large and cannot be neglected. Our reasons for not including drag are threefold:

(1) When the avalanche hits the tree (and the tree remains standing) mass is instantaneously stopped. We account for this momentum loss in the governing equations by removing the momentum of the stopped mass which is parameterized by the coefficient *K*. Our field observations show that as soon as the mass is stopped an aerodynamic-type wedge is constructed around the trees (see Feistl et al., 2014). This reduces the drag coefficient significantly and allows the snow to flow with little resistance around the tree groups. We assume that the friction on the wedge like structures is zero and that the primary stopping effect is from the momentum loss. Moreover, "drag" is not the primary stopping mechanism.

(2) Our attempts to model forest avalanche interaction with velocity dependent drag terms are simply not satisfactory. We tried this approach for example in Christen et al.,

2010, “Back calculation of the *In den Arelen* avalanche with RAMMS: interpretation of model results”. The problem is simple: such drag terms slow the avalanche down, but never stop the flowing mass before reaching the valley floor. That is, drag type models do not predict any runout shortening, simply a loss of velocity. This is reflected in the inability to find drag coefficients for certain forest types (Teich et al. 2012b) were the systematic variation of ξ had no comprehensible influence on modeled runout distances and/or relation to the observed forest structures in the avalanche path. Moreover, drag friction does not appear to be the primary mechanism stopping the avalanche in forests.

(3) We want a model that we can experimentally verify. With the detrainment *K*-model we can predict the amount of mass stopped by the avalanche and compare this value directly with observations. Having a model where we can determine the model parameter based on field observations is an advantage. Again, we have not been able to calibrate drag coefficients according to snow and forest characteristics.

In summary, drag type models do not agree with our field observations, they cannot be calibrated and furthermore appear not to reflect the underlying physical process of forest-avalanche interaction.

>> In the proposed model we solve Eq. 1, but parameterize $\dot{h}_d$ according to

$$\dot{h}_d = {K}\big/{\rho\|\mathbf{V}\|}.$$

This leads to the momentum equation:

$$\frac{\partial \mathbf{V}}{\partial t} + (\mathbf{V}\cdot\nabla)\mathbf{V} = \frac{\mathbf{G}}{h} - \frac{\mathbf{S}}{h} - \frac{1}{2h}\nabla(g_z h^2) - \frac{\mathbf{V}}{h\rho\|\mathbf{V}\|}K$$

Therefore, the forest exerts a constant deceleration ${K}\big/{\rho}$ on the flow. This deceleration is associated with the mass lost. We admit that our approach is phenomenological: rather than to describe in detail the physics of tree-snow interaction, we simply say the trees stop so much mass and extract the associated momentum from the flow. The stopping process involves a myriad of different mechanisms that we cannot describe in a depth-averaged model. We don’t make a difference between mechanisms - we simply calculate the stopped snow mass. For this reason, we agree with you that the terminology “mass is instantly subtracted from the flow” is misleading. We will revise this in the next version of the manuscript. (Note that $1/h$ is missing in the last term on the right hand side of Eq. 3 in your review supplement.)

>> Another criticism is that the parameterization should be more complex – that the amount of mass extracted does not only depends on forest structure, but also on the avalanche flow velocity and length. Obviously, we would like to include more advanced models. At present, however, we simply do not have the experimental data necessary to validate more complex parameterizations. Now, we have only estimates of the mean amounts of mass stopped by avalanches in forests. Even this data is not easy to obtain. We are now performing laboratory

experiments to determine more detailed detrainment models. At present, our goal is to explain to the community why we believe that detrainment and friction models represent the same process (avalanche deceleration in forests). Furthermore, we want to excite the snow community to make accurate field observations (see also line 28 page 5582 to line 7 page 5583). At the very least we are proposing a model that can be refuted using field measurements. The drag model we have already discarded because of the discrepancies between observations and theory.

We hope that you agree with our argumentation.

SPECIFIC COMMENTS:

line 9 page 5567: g_n *is the surface normal component of the vector of gravitational acceleration g - (0;0,*g_z*). Your vector (*g_x*,*g_y*) should only be slope parallel.*

>> Thank you for this correction. In Equation 5, we will change g_n to g_z and further correct $g = g(g_x,g_y,g_z)$ as well as adjust the text.

line 10 page 5567: $\|\mathbf{U}\|$ *is the magnitude and direction of the mean flow.* $\|\mathbf{U}\|$ *does not have a direction.*

>> We agree and will delete “and direction”.

line 2 page 5568: “The extracted mass stops promptly and, thus, is instantly subtracted from the flow..." This is what happens, but it is not how you try to model it. Your approach needs a better description in this paper that the paper is self-contained.

>> See also reply to your general comments: We agree with you that the terminology “mass is instantly subtracted from the flow” is misleading. We will revise this in the next version of the manuscript.

line 7 page 5568: The use of Pa as unit for K is deceive even formally is correct. K describes the mass loss / detrainment and not a stress, I propose to use kg $m^{-1}s^{-2}$.

>> We will change Pa to the unit kg $m^{-1}s^{-2}$ throughout the manuscript.

line 12 page 5568: As mentioned K accounts for the detrainment and not for the braking power.

>> We agree and rephrase these lines: “Parameter *K* accounts for the amount of mass detrained by different forest types per unit area and time and, therefore, depends on forest characteristics.”

“This relationship indicates that the higher the velocity the less snow is removed from the flow." No, amount of snow removed from the flow is independent of the velocity. The relationship indicates at which rate you have to extract snow in your model to have extracted the right amount of snow at the end in your model. With that your rate should be depended on the avalanche length and its mean velocity.

>> We agree and we will delete the direct link to the velocity and revise this part of the manuscript.

line 4 page 5571: μ *is dimensionless.*

>> We will correct this and delete the unit throughout the manuscript.

line 5 page 5575: ... difference $\Delta runout_{ref}$ (Eq. 11) revealed overestimations by RAMMS up to 700% for the chosen parameters K - 0,μ – 0.29, and ξ – 1500 m s^{-2}.

>> We will insert "…for the chosen parameters $K = 0$, $\mu = 0.29$, and $\xi = 1500$ m s^{-2}.".

line 25 page 5579: still overestimated when applying the smallest chosen value of 100 m s^{2}... This is no surprise. As long as $\tan\phi > \mu$, a Voellmy model will not stop.

>> We agree with your comment that it's not surprising that the Voellmy model is not stopping for such small avalanches and we will address your comment in the revised version of the manuscript: "Moreover, simulating small-scale avalanches with a model based on frictional relationships of the presented type only is generally questionable (Sailer et al., 2008), since the avalanche will not stop as long as the slope angle is larger than the friction angle, i.e. $\tan\phi > \mu$. Therefore, including... ". This is a strong argument for applying the proposed detrainment approach when modeling small- to medium-scale avalanches in forested terrain.

line 6 page 5580: forests influence (K - 0) highlight the importance of modeling local braking effects of forests on avalanche flow. as I understand your approach you are not modeling a braking rather than mass loss. That this could lead enhanced braking is a secondary effect in your case due to a possible reduction in flow height.

>> With regards to your previous comments and our replies, we will carefully revise the link between K and "braking" throughout the manuscript and clarify that K is associated with a mass loss induced by forests in the avalanche path which leads to runout shortening.

fig 7: Figure 7 is meaningless without an indication of the topography. Furthermore, to me it looks like there is quite a discrepancy between the observations and the simulation in the figure on the right.

>> See also reply to Margherita Maggionis comment to Fig. 7. We will add contour lines to the figures which might also better explain the two flow channels of the Brecherspitz avalanche due to specific topographical features. However, we still think that runout distance, which was the only considered response variable here, is relatively well predicted by the model; the simulated runout distance stopped within 3 meters compared to the observed one.