Dear Margherita Maggioni,

thank you for reviewing our manuscript. We highly appreciate your constructive comments and suggestions which will help us to improve our manuscript. Our point-by-point replies are given below.

SPECIFIC COMMENTS:

Line 31: you relate K to forest type, crown coverage, vertical structure and surface roughness here in the abstract (and also in Tab. 3), while at lines 83-84 you say that K represents forest characteristics such as forest stand density or mean stem diameters. Then: is K related to which parameters? Are the parameters used by Feistl et al. the same as you found? Please, clarify this.

>> In the Abstract at line 31 we present the results of our analysis while in the Introduction at lines 83-84 we assume that K could represent forest characteristics such as forest stand density or mean stem diameters in order to give some examples for "forest characteristics". Of course, this could be confusing and, therefore, we will refer to Feistl et al. (2014) who showed, based on observations, that the amount of mass which is stopped behind trees growing in the avalanche path depends on stem diameters and/or grouped forests structures.

Reference (updated): Feistl, T., Bebi, P., Teich, M., Bühler, Y., Christen, M., Thuro, K., Bartelt, P., 2014. Observations and modeling of the braking effect of forests on small and medium avalanches. Journal of Glaciology, 60(219), 124-138, doi: 10.3189/2014JoG13J055.

Related to the considered parameters, I would not use "surface roughness" to define what you mean (lines 215-217), as it reminds the roughness of a surface computed starting from the DEM. Better maybe to use something like "surface nature" or "surface cover" ? Be careful at choosing a term that does not remind too much the vegetation cover and to replace the term throughout the whole manuscript, also in tables and figures. Moreover, you used yourself the term "surface roughness" in your previous paper (Teich et al., 2012a) but describing what here is instead the "terrain roughness": another reason not to use it.

>> We do prefer to use the term "terrain roughness" in this manuscript when addressing terrain height undulations determined from a DEM. However, we agree with your concern that the term "surface roughness" might lead to misunderstandings. Therefore, we will change "surface roughness" to "surface cover" also in tables and figures in the revised version of the manuscript.

Concerning "terrain roughness", lines 211-213 are not clear in describing the method you used to calculate it. In Teich et al. (2012a), at pag. 512 the method is clearly described (telling also the story related to the slope angle:" Before that, we calculated a continuously inclining trend raster for each zone of the avalanche area and subtracted it from the DEM to obtain a flattened raster containing local height differences only."). Here, I would either just refer to that paper or describe better the method. As it is something is missing and it seems that you do not consider slope in the method (that would be a large error).

>> Indeed we considered the slope angle when determining "terrain roughness" by applying exactly the same method as described in detail in Teich et al., 2012a. Since the methods are already published and terrain roughness as well as the described variable cross-slope curvature had no significant influence on the response variable Δrunout, we prefer to remove the description of the method defining terrain roughness, but also the detailed description for

the cross-slope curvature calculation in the present manuscript and refer to Teich et al. (2012a): “The terrain variables overall mean slope angle, the cross-slope curvature and terrain roughness were determined from a high-resolution DEM, which was gained from airborne lidar (light detection and ranging) data with a spatial resolution of 2 m and a vertical accuracy of approximately 0.5 m. For a detailed methodological description we refer to Teich et al. (2012a). Cross-slope curvature was categorized in “gully” or concave slope, and ”flat” terrain, i.e. almost no curvature; terrain roughness in “low” and “high” (for details see Teich et al., 2012a).

Concerning the structure of the paper: I would move the section “2. Theory”, and of course all its subsections, in the section “3. Materials and methods”, as the model is a tool used to achieve your aims. Therefore, the section “Materials and methods” would become:

2. Materials and methods

2.1 Theory

2.1.1 Avalanche flow model

2.1.2 Improved avalanche modeling in forested terrain

2.2 Avalanche data

2.3 Simulation and set-up

>> We agree with your comment that the model is rather a tool then a theory introduced here and move Section 2 into the “Material and methods” sections as suggested (see also comments of Alejandro Casteller to Lines 110-169). Moreover, we will rename the heading “Theory” in “Avalanche modeling in forested terrain” as well as merge the two Subsections “Avalanche flow model” and “Improved avalanche modeling in forested terrain” into one section. With the revised structure, we try to direct the focus on the simulations and their analysis and, therefore, on the subject of the presented study.

Lines 219-226 now at the beginning of section 3.2 are actually a repetition and they could be, in the new structure, moved at the end of section 2.1.2 or even deleted.

>> In the “Theory” section we describe the actual avalanche modeling while in the “Simulation software and set-up” section we introduce the simulation software RAMMS in which the models are incorporated. We think that, after the detailed description of the forest avalanche data, these short introductory sentences help to comprehend the simulation set-up and the analysis of simulation results with the AIMEC-approach. We would like to keep the structure, but we will revise it carefully and shorten the sentences wherever needed. We hope that you agree with this argumentation.

Line 124-125: I would not present the examples in the parenthesis, as they are the extreme cases. It can also occur an intermediate situation. The concept is clear anyway, also without the examples.

>> We agree with your comment and will remove the examples in the parentheses.

Lines 149-151: I am not sure about the assumption that snow entrainment in forest avalanches is so small, in particular for wet snow or full-depth glide avalanches. In these cases I guess that entrainment can be important. What do your observations tell? Can you discuss this, starting from your data? Anyway, for the main purpose of your paper, I would *accept your approximation in the modeling approach. I just wanted to say that it is something to think about…*

>> Your concern is an important issue to discuss and needs further investigation. However, since the simulated avalanches all started in forested terrain, we assume that the mass removal behind trees starts immediately after the avalanche is released (see also Teich et al., 2012a) and that, therefore, detrainment is the predominant process. This assumption is also based on the numerical experiment performed by Feistl et al. (2014), but we are not able to discuss this in more detail based on our dataset. Another reason why entrainment was not accounted for: The detrainment approach is only valid for small- to medium-scale avalanches where the forest is not destroyed and the trees act as obstacles. When trees and other woody debris are entrained in the flow, they can become entangled in tree stands, leading to a complex flow state that is difficult, if not impossible, to model. For a more detailed discussion on the detrainment modeling approach see Feistl et al. (2014).

Line 173: when you speak of small to medium-size avalanches, please refer to the international scale (EAWS, 2012), which actually is present in the reference list but it is not cited in the text.

>> We defined avalanches size and referred to the European avalanche size classification (EAWS, 2012) in the Introduction in form of a footnote at Line 44. We prefer specifying avalanche size in a footnote in order to not interrupting the main text.

Lines 189-192: how you determine the release height from the measurements of the surrounding stations? As you reproduced real events, I guess that you used field data when available; if not, did you use simply the new snow in 24h or in 72h? Add this information, please.

>> Release heights were measured in the field for 38 out of 40 observations. Only for two avalanches (#39 and #40) release heights were estimated but not measured based on field visits in combination with measured snow and weather data. We will add this information to the revised version of the manuscript.

Line 199: forest density is always directly related to crown closure? I am not a forest person… This question is related to the first comment on lines 31 and 84.

>> We agree that forest density and crown closure have not to be strongly related in every case. However, for our dataset the forest type represents forest density pretty well since this variable was significantly correlated with the number of stems per hectare as well as with crown closure revealed by pretests of potentially relevant variables as stated at Lines 193-194; we will clarify this. Moreover, the relationship between forest cover density and crown closure was also emphasized by Brändli (2010) and Bebi (1999).

References: Bebi, P., 1999. Erfassung von Strukturen in Gebirgswald als Beurteilungsgrundlage ausgewählter Waldwirkungen. PhD thesis, ETH Zurich. Brändli, U.-B., 2010. Schweizerisches Landesforstinventar. Ergebnisse der dritten Erhebung 2004–2006. Birmensdorf: Eidgenössische Forschungsanstalt für Wald, Schnee und Landschaft. Bundesamt für Umwelt, Wald und Landschaft, Bern.

Lines 230-231: I would finish the sentence at "…forest characteristics." As the following actually is related already to your results.

>> Thank you for your suggestion; we will delete "such as forest density, age or undergrowth".

Lines 273-280: This lines would better fit in the discussion, where actually they are. In fact, lines 526-539 are a repetition of these lines.

>> We agree with your comment, that this is kind of a discussion. However, we discuss this issue already at this point since we tried argument why we chose a pressure threshold P_{limit} of 3 kPa for the further analyses. Therefore, we prefer to keep the main arguments in this section, but we will try to refine and shorten the explanation.

Lines 349-350: Therefore the detrainment is more important for dry snow avalanches than for wet ones? I would have imagined the opposite…

>> We would not say that the detrainment approach is more important for dry than for wet snow avalanches, but that snow densities and thermal snow temperatures also determine the detraining effect of forests as we try to discuss in the Discussion section at Lines 482-484, e.g. as more wet and viscous the snow as slower the avalanche. Such processes need to be incorporated when modeling small- to medium-scale avalanches, but this is not the subject of the paper and therefore we kept snow density constant at $\rho = 300$ kg/m^3.

Line 393: Concerning K_{opt}: is it correlated with the other forest parameters? You cite only the correlation with forest type and some other avalanche parameters (lines 393-399) but not with forest parameters. Then, in lines 400-402 you propose to choose K on the basis of forest type but also of crown coverage, vertical structure and surface roughness… From figures 5 and 6 it is clear that relations exist, but, while for forest type the correlation is significant and shown, for the other three parameters nothing is said. Did I miss something? Can you explain this better?

>> K_{opt} is not correlated with the other forest parameters (see Table 2). However, as you said, clear differences of mean Δrunout between the categories of these forest parameters are visible in Figure 5. We addressed our suggestion to choose K dependent on forest type and the three forest parameters (even if they are not correlated with K_{opt}) in the Discussion (Lines 502-519) and named three arguments underlining our assumption. However, you are right that K should mainly be chosen based on forested type and adapted by K-values for the other forest parameters. We will describe this more accurately in the revised version of the manuscript.

Line 408: Following the above comment, maybe in the choice of the final K value the forest type parameter should have a higher weight than the other forest parameters, as a significant correlation was found only for this parameter.

>> Actually, for a test-version of RAMMS including the detrainment function given to practitioners, we weighted the proposed K-values and assigned the highest weight on the final K-value to the value for forest type. We will specify this in the revised version of the manuscript.

Lines 439: In the two study cases, is it not possible that the avalanches were mixed and that the run-out distance (black lines in Fig. 7) is due to the powder part? Is the model only for the dense part? Concerning Fig. 7: is it possible to add the topographic map? It would help in understanding the avalanche paths, in particular to be able to see in the Brecherspitz case if the two flows derive from specific topographical features.

>> The runouts of the two case studies were mapped and measured with GPS and were clearly defined by depositions of the dense part of the avalanches; a powder part was not

observed and should generally not be important for such small avalanches. Your second concern was also raised by Reviewer #3 and we will add contour lines to the figures.

Lines 449-462: This part fits more to the Introduction, it is actually a kind of repetition. However, It is helpful to go again in a more general view but it could be shortened.

>> We would like to keep the main part of this paragraph since it is important for our argumentation. However, we will revise the paragraph by focusing on the results of our study and discussing the other research in relation to the presented evaluation of the applied detrainment modeling approach.

Lines 502-514: See comment for line 393.

>> See answer to comment on Line 393. We will describe this more accurately in the revised version of the manuscript.

Lines 553-555: Again see comments for line 393. Here in the conclusion, I would much more stress the fact that K can be chosen according to forest type (significant correlation) – and give the values! – and also according to the other parameters only in a qualitative way… At the beginning, the expectation of a reader is to get from your work a table with values of K in term of the forest characteristics (forest type, crown coverage, vertical structure and surface roughness) but actually only for forest type you can make that.

>> We agree with your argumentation. However, we prefer not giving a look-up table at this point since RAMMS including the detrainment function is currently tested by practitioners based on the results of this study, but *K*-values might be refined and we prefer to publish the final *K*-values after the evaluation process is finished. However, as stated in our replies to your comments on Line 393 and Lines 502-514, we will describe this issue more clearly.

Lines 555-556: I am not a forest persons, but all the forest parameters can be determined by remote sensing? Vertical structure? Surface roughness as you define it?

>> As we stated at Lines 555-556 "…the suggested forest characteristics can be largely derived from remote sensing-based data (orthophotographs, lidar-data)…". With "largely" we try to say that most of them can be derived from such data. For example, at this point surface roughness can only be partly determined by analyzing lidar-data. Therefore, we stated at Lines 509-511, "…possibly combined with sporadic field samples, but no extensive measurements are required,". We think that the use of the suggested forest characteristics, which are relatively easily to determine, is a great advantage for practical applications of RAMMS.

TYPING AND TECHNICAL ERRORS:

Line 49: check the year of publication: 2009 or 2001?

>> At this point we refer to Bebi et al., (2009): Bebi, P., Kulakowski, D., Rixen, C., 2009. Snow avalanche disturbances in forest ecosystems - State of research and implications for management. Forest Ecology and Management 257(9), 1883-1892. We will add the reference to the Reference list.

Line 249: "…are maxima over time…"

>> We think both "maximums" or "maxima" is correct; however, we will change that.

Line 251: "…assessment (e.g. Eckert et al., 2010)

>> We will add "e.g.".

Line 259: "…model outputs with…"

>> We will change "output" to "outputs".

Line 316: "…i.e. when K makes runout –> 0, on conditions…"

>> We prefer to keep the phrasing "…, i.e. where K approaches zero of Δrunout, on condition that Δrunout ≥0."

Line 335: "…(Eq. 11), revealed overestimations.."

>> We will delete "further referred to as $\Delta runout_{ref}$" and specify earlier in this paragraph that we explain the results for the reference simulation runs.

Line 408: in the parenthesis you write (see Section 4.2)…we are actually in Section 4.2. Check this.

>> Thank you for this correction. We already changed it to "(see Section 4.3)" in the online version of the discussion paper and will change it to "(see Section 3.4)" in the revised version of the manuscript according to the new structure addressed in a reply to your comments above.

Lines 414-416: "…Therefore, we assigned the "best" K-value to forested areas characterized by the parameters shown in Table 1, i.e. forest type, crown coverage, vertical structure and surface roughness."

>> We will adapt this paragraph based on your suggestion.

Line 429: the reference is it Christen at al. (2010a) or (2010b)?

>> We refer to the reference Christen et al. (2010a) and will clarify this in the revised manuscript.

Lines 438-439: "…forest detrainment function (Fig. 7)."

>> We will delete "with values for the detrainment coefficient *K* dependent on four forest characteristics".

Line 589: This reference is not cited in the text.

>> The reference Bartelt and Stöckli (2001) is cited at Lines 55, 65-66, 452-453 and 455.

Table 3. For the Brecherspitz, the parameter "crown coverage" was "scattered to dense" and K = 125. Actually from Figg. 3 and 5 it seems to me that K should be between 75 and 100. Can you check this? This made me thinking at the sensitivity of the model to the different choice of K. For the two study cases did you made some sensitivity analysis? As this is a new parameter that can be included in a model, it is interesting to see how the model outputs are influence from its choice…

>> Actually, the main part of the forested area in Brecherspitz had a dense structure with some scattered patches. That is why we chose a *K*-value of 125. However, we will check this and may run the simulation again with an adopted final value for *K*. We did not perform a

sensitivity analysis, but Feistl et al. (2014) did and have shown that the model results are sensitive to the selection of the starting mass, snow characteristics, the size and location of the release zone, entrainment processes and terrain features. Another task of further investigation is to include varying *K*-values in order to model the effects of varying forest structures along the avalanche path. For our purpose, we assigned one *K*-value to the forested area and the model predicted the observed runout distances of our case studies relatively well, see Lines 439-442.