Nat. Hazards Earth Syst. Sci. Discuss., 1, C1827–C1832, 2013
www.nat-hazards-earth-syst-sci-discuss.net/1/C1827/2013/

Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Computational snow avalanche simulation in forested terrain" *by* M. Teich et al.

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Received and published: 20 November 2013

GENERAL COMMENTS:

Your paper address an important topic in the field of avalanche dynamics: the forest – avalanche interaction. You address the topic with a research approach but also with a practical view, in the sense that you present all (methods and results) always in the perspective of suggesting something to the practitioners – I like that. In fact, the results of the scientific work are directly useful and usable by avalanche experts in the avalanche hazard assessment. Also the presentation of two study cases is in this direction: they are helpful in the understanding of the suggested method. In general, the manuscript is clear and fluently readable.

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.

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.

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).

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

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.

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.

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. . .

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.

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.

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.

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

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.

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

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?

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.

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.

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.

Lines 502-514: See comment for line 393.

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.

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?

TYPING AND TECHNICAL ERRORS (PROBABLY NOT EXHAUSTIVE):

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

Line 249: ". . . are maxima over time. . ."

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

Line 259: ". . .model outputs with . . ."

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

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

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

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."

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

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

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

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. . .

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 1, 5561, 2013.