Nat. Hazards Earth Syst. Sci. Discuss., 3, C1075–C1082, 2015
www.nat-hazards-earth-syst-sci-discuss.net/3/C1075/2015/

Natural Hazards and Earth System Sciences
Open Access
Discussions

Interactive comment on "Point release wet snow avalanches" *by* C. Vera Valero et al.

Anonymous Referee #2

Received and published: 24 June 2015

The manuscript by Vera et al describes a back analysis of four wet snow avalanches released in the Chilean Andes conducted with the help of numerical simulations of avalanche propagation.

The authors use a model recently described by Vera et al (J. Glaciology 2015) which is based on depth-averaged equations implemented with a Voellmy friction law whose coefficients (dry and turbulent friction coefficients) are not constant but depend on the decay of the so-called fluctuation energy derived from the depth-averaged energy balances, as earlier proposed by Bartelt and co-workers in previous papers since 2006. The energy balances allows an estimate of the amount of heat energy produced by snow avalanches, the evolution of snow temperature and of the production of melt water (when the temperature reaches 0°C) while the avalanche propagates.

In addition, the model considered in the present paper includes (i) an erosion/deposition model (crucial for the problem tackled here given that the ratios of released volumes to final deposit volumes are very small), and (ii) a basal dry friction dependent on the content in melt water. The latter empirical law was initially developed by Colbeck (1992) for friction at interfaces between solids and snow.

At the end a huge number of model parameters and input parameters (see Table 1 and Table 2; the latter is even not exhaustive because some parameters are missing) are needed to be able to run the simulations.

In want of theoretical prediction of some model parameters and/or of any detailed well documented information regarding the input physical variables, the authors proposed the following method: some parameters were roughly estimated from field observations, other parameters (regarding the snow cover) stemmed from numerical simulations done with SNOWPACK, and others are determined by some authors' expertise from previous simulations (if not arbitrary fixed). The authors present a collection of simulation results regarding avalanche run-outs and deposit heights (figs 3-6), evolution of snow temperature and of melt water production (figs 7), and evolution of the volume (figs 8-9) for the four avalanches. Maximum velocities distributions for two avalanches (fig. 10) are also presented.

General comment

The topic addressed by the manuscript by Vera et al is of crucial importance because it deals with wet snow avalanches that are becoming a growing risk in the context of global warming. Moreover, the study is specifically focused on the problem of a primary industrial road to mines in Chilean Andes threatened by wet snow avalanches. Applying advanced numerical avalanche models ("wet snow version" of RAMMS developed in Switzerland) to past avalanche events that occurred in Chilean Andes in order to better forecast avalanche risk is for sure of great practical interest. However, I am sorry to say that I did not find any new scientific finding in the present manuscript. In its current form the manuscript appears to be nothing more than a summary of data collected from a numerical model including many parameters with a very poor comparison to field observations.

I got the feeling that the authors felt in dealing with the complex problem of calibrating a (complicated) model with a great number of input and model parameters (recently proposed by Vera et al., J. Glaciology 2015) while the quantitative information available from the field and needed for the model and input parameters (properties of snow, eroded snow, etc.) remains poor. While some bricks of the model would need more validation, applying such a complicated model to full-scale snow avalanches should be questioned.

The model includes many conservation equations: for mass (for ice and liquid water), for momentum (for 2 directions in the depth-averaged framework), and for energy (2 equations). The energy balances include two additional parameters α and β. Many closure equations are needed such as Eq.(5), Eq.(6) and (7). Friction parameters μ and ξ are not constant but become variables that are derived from the fluctuation energy R. This involves a parameter R_0 in addition to traditional parameters μ_0 and ξ_0. Moreover, Eq.(6) which accounts for the effect of melt water on basal friction needs two additional parameters (μ_{wet} and h_m).

In addition to traditional inputs parameters such as fracture depth and initial volume, other input parameters are needed to deal with melt water production: both the temperature and the snow water content not only of the initial released mass but also of eroded snow cover in the avalanche track (in addition to the expected depth of the eroded snow cover). More careful and detailed discussion should be given regarding the estimation of all input parameters. It would be essential to provide the reader with some typical uncertainty (I guess the uncertainty is weak for some parameters but much higher for some other parameters). And the authors should show a sensitivity analysis of the results to the variability of these parameters.

I strongly believe that entrainment is the key factor in the simulations considered in

the manuscript, when looking at the very low ratios of released volume to final deposit volumes (between 1/20 and 1/64!). I do not see any detailed equation stating how the entrainment rates for ice and water phases are calculated. What is(are) the parameter(s) for the entrainment model (in mass equations for both ice and water)? It is not acceptable to read a scientific paper which is not self-contained, in particular for such a crucial point. Furthermore, is it relevant to consider a uniform depth of eroded snow along the whole avalanche tracks? As snow entrainment plays a key role the modeling of entrainment processes needs much more discussion, including an exact description of basic equations, the values chosen for each parameter, and assumptions made regarding the eroded snow cover distribution.

I strongly believe that the huge gap between the complicated model through its great number of closure equations and related parameters on the one hand and the poor information regarding input and output field data on the other hand makes the study quite irrelevant. I am worried that the authors cannot extract crucial findings/conclusions from their approach. A more pragmatic and scientific method would have been to consider more simple models (but already complex!), then increase further the complexity of the model, and finally cross-comparing the results between the different (more and more complex) models. A traditional depth-averaged model (mass and momentum equations only) implemented with a Voellmy law and constant friction parameters, and with an entrainment model as well (crucial here) would be able to predict relatively well the avalanche run-outs. Adjusting the dry and turbulent friction coefficients, the erosion/deposition parameters and the snow distribution along the avalanche track would be a first step. Then the results from the simplified model (with some sensitivity to the choice of each parameter) would serve as a reference case to show how the following additional ingredients would be likely to change the results:

- Effect of changing the friction coefficient depending on the fluctuation energy via the energy balances and Eq.(7)? - Effect of melt water production on the "dry" friction in the second step by considering energy balances and Eq.(8).

I have many other additional important objections/concerns which I am providing thereinafter.

I hence cannot recommend publication of the manuscript by Vera et al. (2015).

Specific comments

p. 2884, line 6: "documented case studies"... I have the feeling that the quantitative available information remains very poor. This comment would need to be qualified.

p. 2884, line 10: the key role of snow entrainment should be primarily mentioned here.

p. 2885, lines 17-19: is it just an observation or is there any underlying physics supporting this statement?

p. 2885, lines 25-30: these lines can be summarized by reminding that energy balances are proposed in depth-averaged forms, so we cannot expect more.

p. 2885, line30 and p. 2886, lines 2-5: what do you want to say? As snow entrainment is crucial in your study (very low ratios of released volume to final deposit volume), it seems obvious that the properties of snow cover along the track are much more important than properties of snow in the released area. I am not sure that result stems from your model.

Section 2, Eq.(1): u_Φ should be defined here (the reader should not wait for page 2889)

Eq. (4): many variables are not defined: all $\dot{Q}$? $\dot{E}$? $\dot{W}$? The notation $\dot{X}$ (where X is the variable considered) should be properly defined. The paper must be self-contained.

p. 2888, lines 6-8: what is the relation between g', g_z and f_z? The reader should not have to guess (Is g' the sum of g_z and f_z?). Again the paper has to be self-contained!

Eq.(5), p 2889, line 2: R refers to "fluctuation energy" while R refers to the "mechanical free energy" on page 2887, line 19. Please be more precise on the semantics used and the underlying physical processes.

Eq.(6): how the values of μ_{wet} and h_m are chosen? Do you have physical arguments for these values? This equation established by Colbeck (1992) and arguments regarding its application to snow avalanches would merit much more discussion. A graph showing the variation of μ against both R and h_W would be very useful for that purpose.

p. 2889, lines 17-18: very elusive... what about the snow cover distribution? Please discuss the assumption of a uniform depth distribution across the width of the avalanche path and along the avalanche path?

p. 2889-2890 (up to line 14): more detailed information and discussion on how SNOWPACK calculations were made would be needed.

p. 2890, lines 20-22: very unclear... which variable are you comparing at the end between the field and the SNOWPACK simulations?

p. 2890, lines 24-30 and Table 2: why μ, and β are kept constant? Why α is changed (0.07 instead of 0.08) for one avalanche? May I suspect a problem of convergence if alpha would be 0.08 for this avalanche... you must justify the choices made here for the values of μ, β and α!

Table 2: given that the orientations of avalanche paths are different, I am surprised not to see any difference in the values of some parameters (snow properties, depth of eroded snow, etc.) between the LGW-2 and the other three avalanche tracks.

Table 2: some parameters are missing in table 2: R_0? What is its value and how that value is chosen? You must define all parameters, give their value, and explain, justify your choice.

Table 2: why mentioning the cohesion C here? I am not sure that cohesion is used in the model equations...

Table 2: snow densities are not so high meaning that great quantities of air are present (typically around 70

Section 3, p. 2891, lines 11-12: what is the information from mine staff in Fig. 8? Do you refer to Fig. 2 instead? What were the techniques used by mine staff: eyewitness observation, expert knowledge of the site, survey after avalanche, instrumentation used, etc? The manuscript is generally very elusive regarding the field data available from mine staff.

Main test in sections 3.1 to 3.4 could be replaced by a summary table with relevant parameters/information regarding each avalanche track.

p. 2894, lines 6-7: this sentence looks very speculative. What is the relation between the dissipated heat energy and the maximum velocity fields shown in Fig. 10? I do not understand...

p. 2894, lines 9-13: I am not sure that your conclusion directly stems from the results of your model. Your model includes many physical processes (erosion/deposition, fluctuation energy affecting μ and ξ, production of melt water affecting μ) in addition to many input parameters. As a result it is very unclear to me to distinguish between the weights/contributions of each process and choices which you made in the final avalanche run-out.

Discussion, p. 2895, lines 18-22: I do not like this part of the text. Avalanche movement is firstly controlled by the balance between gravity force (proportional to the sinus of the slope) and friction force (proportional to the cosine of the slope). The main inclination angle and the slope geometry (lateral spreading), and the available volumes of snow along the tracks are of course the key factors: "without mechanics, no avalanches!" Are the closure equations (such as Eq.(7) and Eq.(6)) well validated against well documented and controlled experiments for each process (fluctuation energy, production of melt water) to be able to be conclusive in the case of full-scale avalanche events which remain poorly documented?

p. 2896, lines 24-25: I would add that using simple models (with a reduced number of both model and input parameters) but some good statistics (sensitivity of the results

to parameters, confidence intervals, etc.) would have been a better strategy for very poorly documented avalanches.

p. 2896 -2897, end of section 5: this is a very poor (obvious) conclusion while looking at the huge ratios of final deposit volumes to the released volumes for the four avalanches...

section 6 – conclusion: yes, precondition 2 appears to be essential in your study but the erosion/deposition model which you are using is not described and an uniform eroded snow cover is assumed. Other assumptions regarding the distributions of the eroded snow cover would lead to a noticeable variability of the results in terms of avalanche run-out and velocities (before looking at the effect of melt water production). These points should be further discussed in the manuscript.

Some typos /edits

p. 2894, line 21: Figure 8 instead of figure 7

p. 2895, line 6: Fig. 10 instead of Fig. 6

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