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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Mapping snow avalanches hazard in poorly monitored areas. The case of Rigopiano avalanche, Apennines of Italy" *by* Daniele Bocchiola et al.

Anonymous Referee #1

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The paper entitled "Mapping snow avalanche hazard in poorly monitored areas. The case of Rigopiano avalanche, Apennines of Italy" by Bocchiola et al. analyses the tragic event in January 2017 by assessing potential avalanche release depth in combination with numerical simulations. Even though I acknowledge the detailed work of the authors I have several major concerns:

1. The applied models (Poly-Aval dynamic model 1D/q2D) is not state-of-the-art. It is not applied for hazard mapping by practitioners and therefore essential experience is missing. The authors state several times that the model produces "acceptable results" and "is further improved, so we can use this model confidently here". However, there

is absolutely no prove for this. In contrary the authors use often the term "tune" for the model. But this is exactly what should not be done for reliable hazard mapping. A strong indication for this is the applied Mu values. They are separated by the factor 2 for the two applied models and lay way beyond the values usually applied in other models even though the authors claim "the value is somewhat low . . . but is still in line with the present literature". No comparison to state-of-the-art avalanche dynamic models such as SAMOS, RAMMS or ELBA+ is given.

2. There is a lot of relevant state-of-the-art literature missing. In particular in avalanche modelling and large-scale hazard mapping several relevant publications are not mentioned. The literature is insufficiently reviewed. Also for the hazard mapping guidelines only the ones from Italy are referred and all others are ignored (Austria, Switzerland, Norway . . ..).

3. The authors put a lot of effort in assessing the h72 values. However, their argumentation is not easy to reconstruct. There are only seven stations available with very short observation periods (7 – 14 years). So in my opinion it does not make sense to construct "scientific" deductions of extreme values, what also the authors state from time to time in the paper. Furthermore, the wind is not considered even though it is most probably one of the key factors in loading the avalanche release zone.

4. The modelling results and the given uncertainties are not convincing. It is easy to say that the hotel was in the red zone as it was completely destroyed. But the following argumentation about the red, blue and yellow zone lack substantial arguments. In figure 9 the uncertainty seems to be reduced with the local release depth estimation method. But then the red and blue limits are just around 50 m apart from each other, which seems very unrealistic to me.

5. The paper is full of mistakes and vague formulations that make it very confusing to read. Some examples are:

1. Figure 1.: Highest elevation 1200 m a.s.l. instead of 1800 m a.s.l. 2. Figure 4:

wrong entities! 3. A key reference very often referred to (Chiaia et al. 2017) is not I the reference list. 4. P10 L20: (release zone, until 500 m or so) 5. P11 L3: substantially acceptable performance 6. P5 L2: from some sources

In conclusion I state that it would be very dangerous to perform hazard mapping based on this tuned model approach. Therefore, I recommend to reject this paper.

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