

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 #3

Received and published: 14 February 2019

Paper is addressed to present a new 1D-2D dynamic model (Poly-Aval) and to apply it to simulate the avalanche event of 18th January 2017 in Farindola (PE), the deadly Rigopiano avalanche.

The article presents a lot of bibliographic citations (No. 40), including n. 12 of the authors themselves, but with poor scientific literature in avalanche dynamic sector. There is a lot of literature related to authors of the paper (No. 12 papers), which does not refer to the discussed topic. In the bibliography are cited also some articles in non-international and/or popular journals (besides not subject to revision) (n.6 papers). It is reported also double citation of unique paper (in proceedings and in international

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journal).

In the text there are quotations not listed in the bibliography, including: (i) Galizzi (pag. 26); (ii) Chiaia et al. (2017). The last one is the most important reference: it is not included in the list of references, but many parts of Chiaia et al. (in Italian) are reported (translated) in the submitted paper, instead of referring exclusively to citations.

For the first time, the 1D-2D Poly-Aval model is presented to the scientific community. But the paper focuses on the simulation of the tragic Italian event of 2017, instead of being a scientific paper presenting the new model, and comparing it with other existing and tested dynamics models. To date, there is no scientific paper about the presentation of 1D-2D Poly-Aval model and its validation. It seems that the January 2017 Rigopiano event is the first application of the model. There is no reference in the article on the validation, comparison with the state-of-the-art of avalanche dynamic models, or even with topographic and / or statistical ones.

Furthermore, it is not clear whether the model presented is a dynamic or statistical model. The paper is very confusing. In fact, it is not possible to understand if the simulated avalanche is the “project avalanche” (related to a statistical concept, the only one indicated for the mapping of danger by international guidelines) or the attempt to replicate the event of 18th January 2017.

The statistical basis is also lacking by discussing the data considered for the simulation and the resulting hazard mapping. The choice for the h_{72} values is already questionable, but, moreover, only seven stations available - with short observation periods - are not a robust series for statistical data analysis (note that the concept of return time - at the basis of the definition of “project avalanche” and hazard mapping procedures - is not even considered). If, however, the simulation with this new model, refers exclusively to the replica of the Faridola event in January 2017 (but not useful for hazard mapping), the important action of the wind in the release area, was here forgotten.

The reference to AINEVA or SWISS procedures related to avalanche hazard mapping

is also very confusing.

The article makes a comparison the results given by the new model with ones of the paper by Chiaia et al. (2017): in my opinion, some comparisons are not even feasible (they compared results of different nature).

The paper is scarcely scientific, very confusing from the scientific point of view, badly structured and written. Blunders on basic concepts lead me to suppose that the authors are at the beginning of their experience in the field of snow avalanche dynamics.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2018-358>, 2018.

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