Rev 1

The authors do not understand why the Reviewer here remains anonymous. The meaning of open discussion as pursued in NHESS is to openly present fair comments that are publicly spread, and that both authors and reviewers reveal their identity.
Hiding behind anonymous seems unfair.
Also the reviewer puts forward some arguments that are seemingly based upon his/her opinions. This seems not proper normally, and even more when the reviewer remains anonymous, because the authors cannot even be confident of the actual degree of expertise of the reviewer.

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.

This is an opinion, maybe arbitrary. Of course models need be tuned (at the most one could require robustness). And then, if a model is not used by practitioners, is it wrong by definition ?

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.

The present literature, largely reported by the authors, indicates large ranges of variability of μ. Eventually, this is a tuning parameter, and its value depends clearly upon the model set up.
Poly-Aval 1D has been already benchmarked against AVAL-1D, with good results (Confortola et al., 2012).
None of the quoted 2D models is available to the authors, and however the truth is, nobody knows the actual field of depth, and velocity within the avalanche. So, any reasonable representation by any model is worth any other, unless measured values are presented. It is clearly demonstrated in the manuscript that flow velocity is the most relevant trait to impact pressure assessment, and here consistent results for velocity are obtained.
Whenever more data from avalanche events here (i.e., volumes, end marks) were available, a sensitivity analysis to parameters’ tuning (i.e. robustness) could be pursued. It is widely known that (Voellmy -like) dynamic models are very sensitive to μ, and therein some testing would be required. However, it is not the case here, because data from only one event were barely available.

We now discuss this point briefly in the discussion section.

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 : : :.).

Indeed, it seems to make sense that reference is made to Italy, given specific climate conditions, and guidelines for avalanche hazard mapping. Switzerland may be a proper comparison, also given that Swiss Procedure is used, modified, in Italy, and in facts several manuscripts are quoted dealing with Swiss cases study. Three full pages of references are given, which seems an acceptable background. However, the reviewer might have been more specific, and tell us exactly which references may we consider, to provide useful insight to the manuscript.

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.

The situation with short H72 times series is a largely explored topic, and it seems clearly the one introducing more uncertainty. This is the reason why we case focus upon such topic.

The regional approach is widely used, and the authors here have large experience in its application, largely shown in publications upon expert journals in the field.

The wind might have been an issue, albeit possibly it may have affected local patches, and not being so fully determining, given the large detachment area (ca. 8 ha). However, no wind data are available that we know of.

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.

Figure 9 clearly demonstrates the vice-versa, i.e. the regional methods makes estimation much less uncertain.

The statistical nature of H72 estimation, with great uncertainty for high return periods, is exactly the point here. Regional assessment reduces largely such uncertainty, avoiding paradoxical results.

The reviewer may be distracted here by the scale, but fifty meters seems a lot of difference to me in terms of hazard mapping, and however this is given by the threshold of pressure given by the guidelines.

Also, while historical endmark(s) are real, and can in some cases be determined, yellow/blue/red areas are purely statistical concepts for mapping purposes, they do not exist in reality, so what argument may be put forward to demonstrate they are "real" ?

The reasoning that "It is easy to say that the hotel was in the red zone as it was completely destroyed" is fully arbitrary. The evidence that the hotel was destroyed has nothing to do with the goodness of any model, or mapping guidelines, which may or may not put the hotel in red/blue/yellow area. If the results would have shown that the hotel was not in the blue/yellow area, or that it was completely outside the hazard zone, what would the reviewer have said ?

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.

Right, fixed

2. Figure 4: wrong entities!

You mean unit of measure. Right, fixed

3. A key reference very often referred to (Chiaia et al. 2017) is not I the reference list.

Right sorry, we added it now

4. P10 L20: (release zone, until 500 m or so).

This is correct. The release zone covers about until 500 m progressive.

5. P11 L3: substantially acceptable performance

Changed with “the reasonable performance”

6. P5 L2: from some sources

“from different 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.

Another opinion. What does the reviewer mean with dangerous ? The avalanche really occurred and killed many people. Our method consistently displays that the area was at high risk, and that if zoning would have been made by any method likely no building would have been built there. Zoning based on H72, local or regional, assessment is widely diffused. What is the reviewer talking about ? The danger comes with not mapping a hazardous area.

This statement from the reviewer seems not to introduce any real argument, but purely speculative statements instead.