Rev. 3

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.

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 noninternational 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 journal).

Not clear which one paper is double cited, we want through the Reference list, but we cannot see what the reviewer refers to, sorry.

The manuscript indeed covers the topic of hazard mapping, which is mainly based upon two pillars, namely i) dynamic modeling, and ii) statistical assessment of H72, for high to very high (300 years) return periods.
Clearly a large (main) effort was dedicated by many scientists towards assessment of dynamic modeling tools, and models. Maybe less attention was dedicated to the assessment of the large uncertainty introduced when taking estimates of H72 for very high return periods, especially in areas where short (snow depth) data series are available.
The authors here dedicated some effort recently in demonstrating that regional approaches to H72 estimation can reduce such uncertainty to a considerable extent.
Accordingly, here we focus more on this topic, which is also in our opinion central to the situation in Abruzzo region, where Rigopiano avalanche occurred. Therein, even if (reasonably) accurate avalanche dynamic modeling would be available, which may be approximated when some basic data would be available (as here, end mark, and release zone, albeit covered by some uncertainty), still very large uncertainty is carried by H82 estimation, as demonstrated in other manuscripts before, and here for Rigopiano.
Therefore, not very much focus is cast upon a review of dynamic models, but rather upon statistical tools for H72 estimation with reduced uncertainty.
Citation of non-international/popular/proceedings papers in our opinion does not indicated *per se* poor referencing. Several papers from gray literature contain good insights, and can be reported and leaned on accordingly. Here for instance, the manuscript by Chiaia et al. 2017 (now referenced, we are sorry about that, it was just a mistake), is of importance given that it presents the only possible benchmark for Rigopiano avalanche that we know of.

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.

Again, it was a mistake, sorry we added now such bibliography. Translation from Chiaia et al. (2017) was necessary, the manuscript is in Italian. Again here, the mere fact that a manuscript is not published on a *peer reviewed* support does not mean it is wrong, or poor.
The authors of that paper are well renowned in the field of avalanche dynamics, and their results provide a credible benchmark, also considering the large uncertainty in the analysis here.

Chiaia, B., Frigo, B., Chiambretti, I., Marello, S., Maggioni, M. (2017). La valanga di Rigopiano: l'analisi dinamica. [Rigopiano avalanche: the dynamic analysis]. 12 pp. Crasc'17 IV, Convegno di Ingegneria Forense. VII Convegno Su Crolli, Affidabilità Strutturale, Consolidamento Politecnico Di Milano, 14-16 Settembre 2017. https://iris.polito.it/handle/11583/2690369#.XGvrMFxKi70

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.

Poly-Aval 1D was benchmarked against AVAL1-D for at least 5 different avalanche events, in Confortola et al. (2012a, on CRST), Confortola et al. (2012b) https://www.aineva.it/wp-content/uploads/2015/12/nv74_5.pdf, and in Arena lo Riggo et al. (2009), https://www.aineva.it/wp-content/uploads/Pubblicazioni/Rivista65/nv65_4.pdf

The two latter are in Italian, however the charts are clearly visible and display good adaptation. The 2-D version as reported was qualitatively tested for some synthetic avalanche geometries, and benchmarked *e.g.* against results for the Vallecetta avalanche in Bormio, largely studied, obtained using AVAL-2D by Riboni et al. (2005) https://www.aineva.it/wp-content/uploads/Pubblicazioni/Rivista55/NV55_3.pdf

The manuscript (P.7, L.3) reports

" The Poly-Aval q2D algorithm has been tested (Negrone et al., 2017) for a series of synthetic (natural like) geometries (e.g. planar slope, concave slope, concave slope with altitude jump), and for a widely investigated avalanche case study (Vallecetta mountain in Valtellina region, e.g. Bocchiola and Rosso, 2008), with acceptable 5 results against 1D/2D reference models (Riboni et al., 2005), and further improved for use in the Rigopiano case study, so we can use this model confidently here."

We now explained this better.

No pointwise numerical comparison was made, but the two models provided acceptably similar results. Clearly here, one indeed performs a comparison between models, because no information of actual velocity and depth is available.

It still remains the reasoning as to why a "new" model should be considered wrong *a priori* ?

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.

This seems not correct.

Section 3.3 is "Avalanche dynamic modeling Poly-Aval 1D, q2D.", and clearly depicts tuning of Poly-Aval for the case study avalanche event in Rigopiano.
Section 3.5 is "Avalanche hazard mapping", and clearly explains how we map hazard using the "design avalanche" concept through the AINEVA guidelines
Section 4.1, and 4.3 respectively provide the corresponding results

The statistical basis is also lacking by discussing the data considered for the simulation and the resulting hazard mapping. The choice for the h72 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).

This seems also far-fetched. The "return period" concept is instead a topic of the manuscript. The term "return period" is used 10+ times in the manuscript, and elsewhere referred to as *T*, or *T-Years*. Here the reviewer is frankly wrong.
Of course 7 stations, and with short series (no more than 14 years) are few for proper assessment of high return period values of H72. However here the point is exactly to demonstrate that using a regional approach less uncertainty is attained than when using a local one, as clearly demonstrated with Figure 9-10 (for mapped areas), and Figure 11(for H72 quantiles). Consequently, we clearly demonstrate that AINEVA guidelines (as any other guideline using H72 estimates with high return period) are better applied using the regional approach.

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.

Again, Section 3.3 depicts tuning of Poly-Aval for the case study avalanche event in Rigopiano (replica of the event), while section 3.5. explains how we map hazard using the "design avalanche" concept through the AINEVA guidelines, using both local and regional approach to H72 estimation.
The wind might have been an issue, albeit possibly it may have affected local patches, without being fully determining given the large detachment area (ca. 8 ha). However, no wind data are available here that we know of.

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).

We explicitly report in many instances that we use the AINEVA guidelines, starting from Line 1 in the Abstract.

In my opinion, some comparisons are not even feasible (they compared results of different nature).

Indeed, it is an opinion. Comparisons between different methods, if fairly carried out, can provide insight of the pros, cons, and limitations of any method. We have largely acknowledged that our method (as any other methods) entails uncertainty, but this is always the case with avalanche hazard mapping, especially in unmeasured areas, and for long return periods of H72.
Also we have clearly explained that we used AINEVA guidelines, widely known in the avalanche hazard mapping field.
Also we do not claim that our approach, i.e. the Poly-Aval, and the regional approach for H72 assessment is perfect (i.e. no uncertainty), and/or any better that other methods.
Here, we simply, fairly, and honestly tackle the issues arising from the need for avalanche hazard mapping in complex, data poor sites, and we suggest that in the specific study area, regional methods may be regarded as useful for hazard mapping, which is indeed lacking hitherto, because they can help in reducing uncertainty.

The paper in 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.

Here the reviewer is utmost wrong, and frankly I am not sure he/she is fairly addressing the manuscript. The authors of the manuscript are not anonymous, and the reviewer can easily verify that in the last decade this authors have produced plenty of manuscripts, studies, and research in the topic of avalanches modeling, mapping, statistical assessment, and generally in snow/ice/avalanche science.