

The authors would like to sincerely thank Dr. Alessandro Leonardi for his comments and suggestions on the submitted research paper. Each comment and its corresponding reply are addressed in the following text.

The paper does an excellent job in highlighting the importance and urgency of the research presented. The review of the state of the art is complete and clear. The language is good, and while some errors and typos are present in the manuscript, they do not reduce the readability of the paper. The comparison with full-scale experimental recordings is a very strong aspect of the paper. Comparison with field measurements is particularly difficult for mass flows, and makes the results much more credible. The choice of testing a DEM model with cohesion is of great interest, because it shows a different perspective on a long-standing problem in the field. In fact, it is widely known that standard DEM models (i.e. with dry, adhesionless particles) have strong limitation in the simulation of water-rich debris flow.

The authors thank the referee for confirming the importance and relevance of the topic.

I found the article pleasurable to read. However, many points need further clarifications, and possibly a few more simulations would much strengthen the impact of the study. I list here the major issues:

- *The adoption of a non-standard contact model, while being the most interesting aspect of the work, is not followed by an appropriate analysis of its capabilities. The varied parameters only include friction and restitution, which are parameters that would intuitively be chosen for adhesionless granular assemblies. I would have suggested to also test the influence of parameters such as the “minimum force”, which based on Eq. 1 and discussion, seem to control the adhesive part of the model.*

In the sensitivity study of the model, different parameters have been investigated such as the microscopic friction angle and the restitution coefficient. However, the restitution coefficient is meant here as the ratio of k_1/k_2 (Luding, 2008). Although it shares the same name, it is not the same as for cohesion-less models which defines the ratio of velocities before and after an impact of two objects (Schwager and Pöschel, 2007). Thus, to avoid ambiguity, the term k_1/k_2 would be used in the revised version instead of restitution coefficient. A more in-depth analysis of the model parameters will also be carried out including testing the effect of k_c which defines the minimum force of Eq. 1.

- *Many problematic aspects of the simulations, such as the absence of a deposit, or the creation of a dilute front, could effectively be reduced by exploiting the adhesive bonds between the particles. However, this is not addressed in depth in the paper.*

The presence of a dilute front would change depending on the value of k_1/k_2 which partially controls the activation of adhesive bonds. If the adhesive bonds are not activated, particles would detach from the flow and a dilute front would appear. The lack of deposition is addressed in the next comment.

- *With respect to the lack of deposit, it is not clear why simulations with a friction*

angle larger than the flume slope (40° vs 30°) do not produce a deposit. As a matter of fact, I would have expected to see little mass mobilization in this case. Adhesion would have further prevented the mass from mobilizing. In my opinion, this aspect should be clearly addressed in the paper.

In the DEM model, the friction angle value is meant as the value of friction angle between a sliding particle and the channel base. Since this value is calculated at a microscopic level, it does not correspond to the macroscopic value of friction angle which would have stopped the mass from sliding if it had exceeded the channel's inclination angle (30°). In order to avoid ambiguity, the friction angle in the paper would be referred to as the microscopic friction angle and would be stressed that it is the one calculated at the micro-scale between sliding particles and the channel base.

Furthermore, in the revised version, a clear difference will be highlighted between the particle-particle interaction and the particle-wall interaction, with the wall being either the sensor or the channel base. The authors will clearly state that the particle-particle interaction is governed by the new proposed contact law of Luding (2008). On the other side, the particle-wall interaction is governed by the classical visco-elastic contact law (Schwager and Pöschel, 2007) for which previous studies of Albaba et al. (2015) have been used as reference for calibrating its parameter values concerning the impact between a flowing mass and a rigid wall.

- Overall, the choice of the simulation parameters could be motivated more. The values of mean particle diameter, particle density, inter-particle friction, and Young modulus are given without a convincing explanation behind their choice.

The choice of the model parameters was based on previous simulations of Albaba et al. (2015). It would be clearly stated in the revised version.

- The analysis of Fig. 8-15 and 17-20 is mostly descriptive, and does not add much to the figures themselves. The authors possess a lot of information that they do not use for the interpretation of the results. For example, in section 3.3.2, the authors guess that the excessive dispersion is due to the decrease of plastic deformation. However, the authors do have the information to check if this is indeed the case. Here again, it would have been very interesting to see whether the adhesive bonds could have countered this unwanted effect. Another example is in section 3.3.3, where the authors suppose that the augmented flow shearing due to an increase of ϕ_b generates thicker flows. Once more, the authors could check whether this is the case, rather than leave the interpretation open. Here adhesion is one more not mentioned. If, instead of basal shear resistance, the adhesive bond would have been boosted, would the results have changed in a less intuitive fashion?

In addition to the investigated parameters, a detailed investigation will be carried out regarding the impact pressure signal and its relation to the filtering interval (more information in the next comment). Furthermore, an in-depth analysis will be carried out concerning the effect of the particle size (75, 100, 125 and 150 mm) by relating mean particle diameter, evolution of number of contacts and the duration of the contact to the observed flow behavior. Moreover, the evolution of the cohesive bonds with time

for different contacts within the flow will be analyzed and the possible relation to the model's parameters will be discussed.

- *In the comparison between measurement of basal pressure and impact load on the sensor, clearly the applied filter has a great influence on the results. The authors do a good job at describing the problems associated with this. However, the particle size chosen for the simulation does not correspond to the one used in the experiments. Therefore, applying the same smoothing window as in the experiments does not seem like an intuitive choice. Since pressure is one of the calibration parameters, this might lead to erroneous results, and maybe partially explaining the difficulties in obtaining a better calibration for both velocity and flow height.*

The authors thank the referee for raising this important point concerning the filtering of DEM signal which has a direct effect on the way the maximum impact pressure is calculated and also relates to the calibration process which is partially based on the comparison between maximum pressures of DEM and Exp, in addition to the flowing height and velocity.

The strong oscillations of DEM signals are usually linked to many factors including the number of particles, the area that is being impacted, the frequency of recording data, the mean particle diameter and the number of contacts.

One difficulty in the current study however is the fact that the experiment represents a full-scale hill slope debris flow with a volume of 50 m³. Such a large volume requires running simulation with particle sizes that are relatively large ($d_{50} = 75$ mm) in comparison with the range of sizes, in order to keep the total number of particles within feasible range as to the computation capabilities of the super computers (the average total number of particles is around 160,000). In addition, the sensor size is indeed small (200x200 mm) in comparison to the mean particle size considered for the simulations, which leads to having few contacts per impacting step and thus a discrete fluctuating signal in DEM.

Furthermore, the possible variation of the particles' initial spatial distribution in the released material might also have a small effect on the force signal, as reported in some DEM studies (e.g. Albaba et al 2015).

Because of all aforementioned reasons, there is a need to define a filtering interval based solely on an investigation of the DEM signal and independent of the experiment's filtering interval.

In the revised version, the authors propose an analysis of the DEM signal based on two points:

- I. The repeatability of the same tests to account for initial spatial variation.
- II. The signal of different simulations with different parameter values.

First, the same DEM simulation would be run 10 times with different initial spatial distribution and then the maximum pressure will be plotted against different filtering intervals (0.025 s up to 0.5 s). In addition, the relative error defined as the normalized difference between two successive values of maximum impact pressure would be plotted. An optimum filtering interval would be defined as that with a relative error of 5% or lower. The same would then be carried out for the different DEM simulations with different parameters of φ and ε .

The filtering interval to be used for filtering the DEM and deducing the maximum impact pressure would be the optimal one while considering the two points above. An example of the proposed analysis is presented below in Fig R1 for a simulation with $\varphi = 30^\circ$ and $\varepsilon = 0.3$

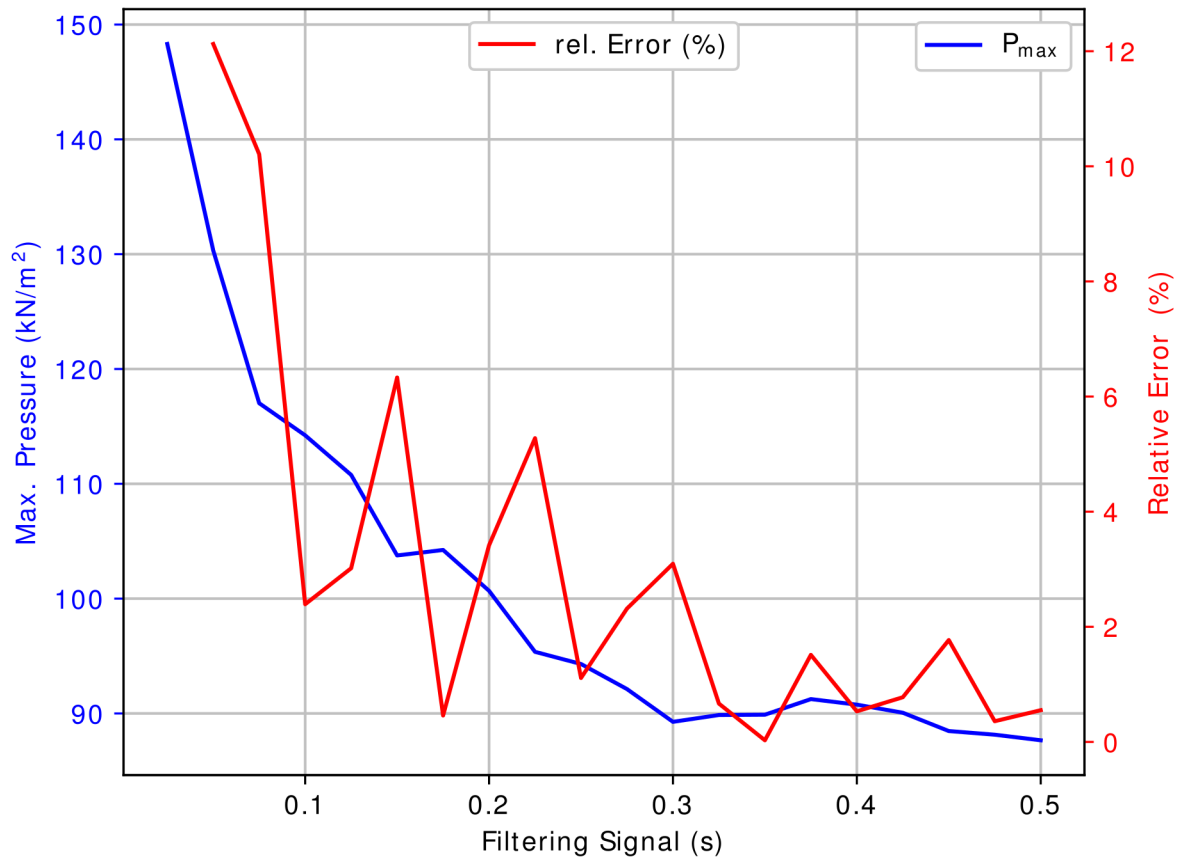


Figure R1 The maximum pressure and relative error for different filtering intervals for DEM simulation with $\varphi = 30^\circ$ and $k_1/k_2 = 0.3$.

- Finally, I think that the paper would benefit from a reorganization of section 3. The results of the sensitivity analysis (3.3) should be presented before the comparison with experimental values.

The revised version will be prepared in compliance with the proposition of the referee. After the introduction, a detailed sensitivity analysis of the model's parameters will be presented in addition to the analysis of the filtering interval. After optimizing the choice of filtering interval of DEM, a comparison between the model and experiment will be detailed. The discussion will then be based on the analysis of pressure signal in addition to further analysis of the comparison between DEM and Exp data. All pieces of information concerning the experiment will be introduced when introducing the experimental data.

Minors: Page 2, last line: "flowing velocity, flowing height" maybe better "flow velocity, flow height" Page 3, line 25: "and and" Page 4, line 21: "channelized channels" sounds weird Page 8, line 13 "chut's bottom" Fig.5 The picture bottoms are cropped before the end of the chute. Fig. 8: when printed in grayscale, the lines become very similar.

Fig. 22 Row -> Raw. Also: why negative values? Page 13, line 6: well -> good

All these minor points will be corrected in the revised version