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Discussions

Interactive comment on "Elasto-plastic-adhesive DEM model for simulating hillslope debris flows: cross comparison with field experiments" *by* Adel Albaba et al.

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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.

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.

- 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.

- 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.

- 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 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 phi_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 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 smoothening 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.

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

Overall, the paper is set out to offer a new and very interesting approach for the simulation of debris flow. However, the capabilities of the most innovative part of the proposed model are not really explored. The paper offers an analysis that reiterates some common findings in the literature. In fact, difficulties in reproducing flow mobility and in obtaining a correct estimation of impact forces are very common. It would have been very interesting to see if adhesion helps in addressing these long-standing problems. However, the paper does not offer much insight in this respect.

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

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