

Interactive comment on “Laboratory and Field Test and Distinct Element Analysis of Debris Flow” by Yung Ming Cheng et al.

Dr. Scaringi

g.scaringi@qq.com

Received and published: 17 May 2018

The authors present a set of flume experiments performed with dry mixtures of glass beads of various sizes, which were then modelled using a two-dimensional discrete approach. Moreover, the experiments explore the characteristics of the flow and deposition, but not of the movement initiation (as the material is simply released, rather than being destabilised through external forcing). So, I was wondering if the authors could modify the title, which at this stage is generic and too ambitious, to a more matching one such as "(qualitative comparison of) experiments and distinct element modelling of dry flows of granular mixtures" or similar.

Besides, the introduction from line 30 to line 73 seems unnecessary. Even though

C1

it provides some contextualization and motivation for the authors' research work, it seems to actually constrain and limit it to a specific location. The issue of the correct modelling of the geo-hazards is universal, and the authors' study may be potentially applicable to other contexts. Hence, I suggest that the authors shift the focus in their introduction to the state of the art of their modelling approach (dry particle flow as opposite to continuum-based modelling) and provide the reader with an overview to understand the limitations of the existing modelling techniques, the motivation of the present work and how this work represents a step forward in our understanding and modelling capabilities. This is partly done from line 74 on. However, the motivation of the work does not stand out enough.

Lines 82-84 are just a list of citations. I suggest that the authors describe one of these classifications (if useful for the understanding of the manuscript) or leave this part out.

From line 89 to line 131: I think the authors could focus their literature review to experimental and theoretical experiences on dry flows first. Then, they could explain briefly how the presence of water changes the flow behaviour, if and how studies on dry flows are still applicable to wet flows, and why wet flows have not been studied in this work.

Lines from 132 to 143: the authors should motivate the choice of a 2D approach better. Even though flume test geometries attempt a 2D simplification, modelling them using a 2D granular approach may be not sufficient anyway, unless the modelling takes into account the changes of porosity (and not only) deriving from the use of discs that cannot move in the out of plane direction instead of balls that can do so in a 3D simulation. Moreover, current software and hardware allow for 3D computations of large assemblies of particles, so it is a pity that the authors did not explore this more realistic approach.

Lines 144 to 148: the authors could think of including the visual material (photos, videos) as supplementary material accompanying the paper, if they or the editor think it can be useful.

C2

Lines 160-162: the authors could explain the advantages and limitations of using spherical glass beads instead of sand or gravel mixtures with generic shapes more in detail. For example, glass beads can rotate easily rather than simply sliding (which makes their apparent friction very small), while rotation is partly hindered when particles with irregular shapes are involved. Additionally, in real-size cases, particle crushing may occur during the movement, so grading will change during the flow and this may exert a feedback on its characteristics. Furthermore, I would be careful when interpreting the results of experiments in which the deposit is just one ball thick. Is this still representative of dry flows in nature or it is an oversimplification? Perhaps the authors could discuss on this.

Lines 221-222: it is anyway a pity that wet flows are not considered in the same work, as they would provide a much interesting insight and the article would become much more valuable. Moreover, it's generally the wet flows that are the most concerning ones in terms of hazard.

Lines 224-230: it would be good if the authors could explain their statement better and support it with data and/or references.

Line 285: model generation. A calibration of some parameters is involved here. The authors should specify which of the parameters have been calibrated by back analysis and show, for instance as a supplementary information, sensitivity analyses to the variation of these parameters.

Lines 333-335: I suggest that the authors provide quantitative metrics rather than a qualitative judgement of the agreement of the numerical simulations with the physical tests. These metrics could be geometric (e.g. runout distance, shape of the deposit), kinematic (e.g. flow velocity), dynamic (e.g. impact forces), energetic (e.g. energy dissipation, heat, acoustic emissions).

Line 448 onwards: the study of segregation in large flume tests seems very interesting. However, the results are shown and discussed only qualitatively (through photographs).

C3

Again, I think the authors could discuss their observations in a more quantitative way (e.g. by studying the spatial changes of particle grading along the deposit and with depth, compared to the initial grading).

Lines 545 onward: this is of main concern. I think that providing only a "reasonable qualitative simulation of dry granular flow" without a quantitative insight is insufficient "for the consideration of the engineers". Also in the conclusions the authors provide only qualitative considerations. Therefore, I warmly encourage the authors to rethink their manuscript in a way that can provide quantitative results on which a solid scientific discussion can be based.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2018-36>, 2018.

C4