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Interactive comment on "Combined effects of grain size, flow volume and channel width on geophysical flow mobility: 3-D discrete element modeling of dry and dense flows of angular rock fragments" *by* Bruno Cagnoli and Antonio Piersanti

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The manuscript presented by Cagnoli and Piersanti describes an interesting application of the DEM method to a granular flow problem. The estimation of the expected runout distance of granular flows is indeed an important problem in a number of contexts, although the authors might overstate that importance slightly ("...at the top of the list of the most hazardous natural phenomena.", page 1, line 26). There are, however, a number of issues in the manuscript which are not quite clear to me:

Specific Issues:

(1) p. 2, l. 29ff : I can only partially agree with the sentence that DEM is ".. able to estimate correctly the relative energy dissipation of the flows without calibration." . This of course only applies if the interactions implemented in the DEM correctly model the interactions happening in a real granular flow and, importantly, the interaction parameters (friction coefficients, contact stiffness ...) are correct. And getting the parameters right might, at least in some cases, require calibration. In this context, i.e. whether the chosen interactions are correctly modelling the true mechanics of the granular flow, I would have one fundamental question: Does the lack of dissipation by grain fracturing, which might be relevant in the higher stress regimes of large granular flows, have an influence on the results?

(2) p. 3, l. 10ff. It would be good if the assumption that the behavior of the granular flow is independent of its actual size could be validated by some models.

(3) p. 4, l. 19: What is the justification of using an essentially monodisperse grain size distribution? Does this choice influence the results?

(4) p. 4, l. 31/31: The connection between particle shape and friction angle should be clarified. It is well known that there is such a connection (Abe & Mair, GRL 2009, Kim et al., Geosci. J. 2016) but quantifying it is not trivial.

(5) p. 5, l. 9: Using the density mentioned here, is the grain mass calculated from the volume of the actual grain (i.e. the box shape) or the volume of the spheres forming the contained cluster? How much does this decision influence the aggregate density of the granular material given the difference in the outer dimensions between the cluster and the box-shaped grain?

(6) p. 5, l. 18ff. I fully agree with the authors that the clusters of spheres are an efficient way to model the dynamics of non-spherical particles, in particular regarding the friction in granular materials (Abe & Mair, GRL 2009). However, I would be reluctant to call the

grains used here “angular”. At least it should be made clear somewhere early in the manuscript (the introduction maybe) that the authors are approximating angular grains by using aggregates and that they are not using a DEM with true polyhedral particles such as in (Nassauer & Kuna, Granular Matter 2013).

(7) p. 5, l. 23ff: The choice of a Hertzian contact law is an interesting one in this context because it assumes a specific contact shape (i.e. a sphere-sphere contact) whereas the authors are using the sphere clusters to approximate angular grains, which would supposedly lead to quite a different contact shape. Is there any indication if this choice of contact law has a significant influence on the presented results? More technically, is it known if the Hertzian contact law is a better approximation here than a computationally cheaper linear contact law (cf. Donze et al., GJI 1994)?

(8) p. 10, l. 7-9: What about uncertainty due to different initial particle arrangements? Could this be quantified by running a set of models with the same parameters but supplying a different random seed to the particle setup described on top of p. 5?

(9) p. 11, l. 28ff: If I’m reading this paragraph correctly, it seems be to strictly correct only if the curvature of the described “change of slope” is constant for a section of the surface at least as long as the traveling granular avalanche.

(10) p. 13, l. 21-25: Wouldn’t the grain size distribution also be a factor influencing the behavior of the flow?

(11) Fig. 4: Looking at the smallest type of grains the authors use, i.e. the ones on the right in Fig. 4, and considering that the contact forces on the grains appear to be calculated based on the inscribed sphere clusters, isn’t there a risk that these particles will move by rolling, rotating around their long axis, and therefore produce much lower friction angles than real angular grains?

Technical issues:

(1) p. 2, l. 3/4: I’m not sure that I understand why the fact that some quantities vary

significantly (line 4) automatically causes them to influence the mobility of the flow (line 3).

(2) p. 2, l. 8. It would be good if “mobility” would be defined before being used here.

(3) p. 7, l. 10ff: What is the internal dynamics of the sphere clusters? Are the spheres “glued” together by elastic interactions as in (Abe & Mair, GRL 2009) or are they rigid bodies like the “clumps” in (Cho et al., IJRMMS 2007)?

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