Geosci. Model Dev. Discuss.,
doi:10.5194/gmd-2017-25-RC1, 2017

Geoscientific Model Development Discussions

Interactive comment on "DebrisInterMixing-2.3: a finite volume solver for three-dimensional debris-flow simulations with two calibration parameters – Part 2: Model validation" *by* Albrecht v. Boetticher et al.

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Received and published: 21 April 2017

This paper discusses the capabilities of a new 3D finite-volume model developed for simulating the propagation of debris flows that are rich in fine particles. The model relies on a monophasic approach for describing the mechanical behavior of the material. The originality, however, is that the contributions of the interstitial slurry (water and fine particles) and of the large particles are handled independently, using distinct constitutive models, and then lumped into a global apparent viscosity through a concentration average. Owing to this approach, the authors claim that, once the model is calibrated

for a given composition of the solid material, it should be capable of accounting for changes in water content without further recalibration. The model is presented in detail in a companion paper recently published in the same journal. The present paper focuses on comparisons with experimental results of the literature at different scales.

Overall, the agreement with the experiments appears reasonably convincing, and shows the potential of the model. Since the code is provided as supplementary material (I did not actually test it), the readers will have the possibility to further explore the capabilities of the tool. The choice of the case studies appears a bit questionable, since none of them really challenges the 3D character of the model. A case of impact against an obstacle would have probably been better suited for that purpose. Yet, the presented case studies are interesting and allow the authors to evaluate the rheological module, which is the real focus of the paper. The comparisons with experiments are discussed in a balanced manner, highlighting the assets as well as the limits of the lumped approach. However, a more thorough discussion of the respective roles played by the slurry and granular contributions would have been interesting (see comments below). The paper is well-written and the presentation is already well-advanced, although the number of figures is probably in excess (see suggestions for removals below). Globally, I thus support the future publication of this paper in GMD, but I have first a number of specific and technical comments that I would like the authors to address in a revision of their manuscript.

Specific comments

1/ Nowhere do the authors properly specify how the distinction between particles belonging to the interstitial slurry (characterized by a Herschel-Bulkley rheology) and particles belonging to the granular phase (characterized by a pressure-dependent rheology), is made. I assume that this distinction is based on a grain-size threshold? What is then the value of this threshold? Typically, is the sand fraction considered to be part of the slurry or the granular phase? Since colloidal interactions are generally negligible for particles larger than a few microns, I would personally consider sand as belonging to the granular phase. However, as the authors systematically refer to this granular phase as "gravel", one is led to think that sand is instead accounted for in the slurry. This issue would need to be better explained and discussed.

2/ Related to the previous comment, it would be interesting to fully explicit the computation of the lumped rheology in at least some of the examples treated: i.e., give values of the "full" yield stress of the slurry \tau_y (and not only of \tau_00); give values of the effective viscosities of the slurry and granular phases, and of the lumped material (concentration average), for representative shear rates and pressures. More generally, a discussion on the interest of considering this composite constitutive law in the examples shown would be interesting. Is the contribution of the granular phase significant? Would have it been possible to obtain equivalent results with only the viscoplastic part?

3/ I did not really understand the rationale behind equation (1) used to evolve the parameter \tau_00 with water content. In principle, one would expect this parameter to remain constant for a given composition of the solid material, and thus independent of water content. If I understood well, the authors do nevertheless consider a variation of \tau_00 with water content due to a sensitivity of their computations, in particular shear rate, to grid size. From my point of view, this issue should be discussed in more details (see also comment 4 below), and equation (1) should be better justified. In some sense, it can seem disappointing to develop a full 3D model relying on supposedly physically-based constitutive models and, in the end, to use such a trick to resolve what seems to be a purely numerical issue. In particular, the grid-size-sensitivity probably implies that the vertical structure of the flow is relatively poorly captured in the presented simulations. What is then the benefit of the 3D model compared to a depth-averaged approach?

4/ Nothing is said concerning the mesh characteristics used in the different examples presented: grid size, number of elements in the horizontal and vertical directions, etc. This is essential information for any reader interested in performing similar studies with the provided code. Furthermore, the influence of grid size on the presented compar-

isons with experimental results would also need to be discussed. I wonder, in particular, whether the results presented in Fig. 5 for the reduced and increased water contents (compared to the calibration case) could be improved with a finer mesh. Same question for the results presented in Fig. 15, notably the strong unphysical oscillations displayed by the pressure signal.

Technical comments

P. 2, l. 7: "Material properties are related to the fractions of different minerals...". Maybe specify "clay" minerals?

P.3, l. 25: "Therefore, for each material composition there should be a critical range where a minor variation in water content causes a strong change in flow depths and run-out distance." I do not really understand this statement. How is this "critical range" related to the exponential variation of the yield stress with water content?

P.5, l.1-9. The relative error (in %) between the simulated and experimental deposit length is indicated for the case of increased water content, but not for the case of decreased water content (in which case only absolute values are given).

P.6, l.11-12. Please also indicate the experimental values of tan(\beta_min) and of the corresponding correction factor.

P.7, l.5-7. Why were simulations of the SG mixture based on the same calibration parameters as for the SGM mixture? Since the composition of these two materials is different, it seems that a recalibration would be necessary? Furthermore, results obtained with SG mixture are not really described in the following (and Fig. 8 is never properly discussed). What is then the point of introducing this additional case?

P.10, l.21-23: "The measured and simulated values do not agree with the mean arrival times implied by the laser signal at position 66 m (Fig. 15 b), however, they do by means of basal pressures for the lower gravel friction angle simulation (Fig. 15 d)." Unclear sentence.

P.11, l.1. I do not fully understand what the authors mean by “Our approach allows the model parameters to be linked to (. . .) local topography”.

P.12, l.1: “The model can account for the sensitivity of the rheology to channel geometry . . .”. This is a strange statement: one does not expect the rheology (a material property) to be sensitive to channel geometry.

P.12, l.8-10: “Because such changes in model setup are translated into consequences for the flow physics by the model, the ensemble of such simulations may mirror how the modeled site would respond to similar changes.” Unclear sentence.

Fig. 1: This figure may not be necessary.

Fig. 2: What are we supposed to see in this figure? What conclusion can be drawn from it concerning the influence of the gate on flow dynamics? To highlight the role of the gate, it would certainly be more demonstrative to show, e.g., front position versus time for cases with and without gate.

Fig. 7 top: This figure may not be necessary.

Fig. 10: This figure, on which details are hard to see anyway, may not be necessary.

Fig. 11. It appears that this Figure is never called, nor discussed, in the text. Is it then necessary? If the authors decide to keep it, axes and scale should be indicated. What is the thin vertical line that can be seen behind the thick red mark?

Fig. 13. This figure may not be necessary.

Fig. 15. It appears that plots e and f, corresponding to the SG mixture, are never called nor discussed in the text.

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