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Interactive comment on "DebrisInterMixing-2.3: a Finite Volume solver for three dimensional debris flow simulations based on a single calibration parameter – Part 2: Model validation" *by* A. von Boetticher et al.

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R.M. Iverson review comments on "DebrisInterMixing-2.3: a finite volume solver for three dimensional debris flow simulations based on a single calibration parameter – part 2: model validation" by A. von Boetticher , J.M. Turowski, B.W. McArdell, D. Rickenmann, H. Hürlimann, C. Scheidl, and J.W. Kirchner

SUMMARY REMARKS

As a preface to my comments, I will disclose that some of the test cases used by

the authors of this paper involved experiments that I performed myself at the USGS debris-flow flume (with much assistance from colleagues). Thus, I'm very familiar with these experiments. However, the authors did not contact me about their intent to use the results of these experiments in the manner that they did. If they had, I could have provided information that may have helped them avoid misunderstandings that seem evident in this paper. Below, I elaborate these matters in my detailed comments regarding section 2.3 of the paper.

In a broader context, it's important to note that each of the three classes of test-case experiments considered in the authors' paper involved abrupt releases of debris that was impounded behind a vertical headgate. Such "dam-break" releases almost instantaneously impose a large downslope force imbalance. However, this type of large initial force imbalance rarely, if ever, occurs in nature. Instead, natural debris flows generally develop from small perturbations to balanced static states. The significant difference between natural and test-case initial conditions prompts questions about the performance of the authors' model for realistic scenarios in which the force imbalance during the onset of motion is infinitesimal. If the authors' model is like most models that use stipulated debris rheologies, then it would be unable to simulate the behavior observed in such cases. This inability results from the model's lack of representation of state-dependent transient weakening of debris that begins after motion commences. Such state-dependent weakening can be caused by soil liquefaction, for example. Generally, models can realistically simulate such state-dependent behavior only if they include feedback mechanisms such as evolving pore-pressure feedback. The ramifications of this issue are not merely academic. Indeed, the mathematical structure of a model holds large implications for how a modeler initiates a simulated debris flow, which in turn influences the predicted flow behavior downslope.

COMMENTS KEYED BY PAGE NUMBER

On p. 6383 there is the perplexing sentence, "The smooth channel experiment included a sediment mixture with 2.5 times more loam than the so-called SGM mixture (Iverson et al., 2010) that was applied on the rough channel." When taken in its context, the sentence's meaning is entirely opaque. When I reached page 6389 I finally was able to decipher the meaning of the sentence. To decipher the meaning one also needs to know some pertinent facts about the large-scale experiments performed by Iverson et al. (2010): A total of 28 experiments were conducted. Eleven involved flow of wet sand-gravel mixtures (with less than 1% finer sediment by dry weight) flowing across a smooth concrete bed. Nine involved the same wet sand-gravel mixtures flowing across a concrete bed surfaced with regularly spaced rounded conical bumps 1.6 cm high. And eight other experiments utilized the same bumpy bed but involved wet sand-gravel-mud ("SGM") mixtures containing 7% silt- and clay-sized grains by dry weight. What the authors aim to convey is that they utilized results from a smooth-bed experiment that was not included among the 28 experiments analyzed by Iverson et al. (2010). Instead they utilized results from an experiment performed in 1997 that we never fully analyzed because it was a flawed experiment. The flawed character of the experiment calls to question its usage as a test case by the authors. Below I elaborate this point in my comments on section 2.3.

In section 2.1, pages 6384-6386, model results are compared with the results of small-scale experiments involving debris with differing water contents that was released from behind a vertical gate and allowed to descend a flume. The comparisons look impressive, but too few details are provided for readers to know what was really done. For example, how were the dynamics of the opening gate simulated by the model? Evolving, finite vertical shear tractions must have existed where the gate contacted the debris, and a 3-D model must somehow account for the effects of these tractions. Moreover, was a no-slip basal boundary condition used in the computations? If there truly was no boundary slip, then the full frictional resistance of the gravel may not have been engaged, invalidating the use of the gravel friction angle to calculate its effect on basal shear tractions. On the other hand, if boundary slip occurred, then how was it represented by the mathematical boundary conditions? And why are calibrated values of reported to four significant digits? Do the data really justify this level of precision?

Are model predictions sensitive to variations of as small as 0.01 Pa? If so, then that is a great liability, because 0.01 Pa is smaller than the basal stress exerted by a gravitationally loaded "layer" of typical debris less than 1 micron thick! In other words, it's physically almost meaningless.

On p. 6387 I don't understand the basis of the statement, "a correct representation of super-elevation of deflected material by the model would suggest that the interplay between pressure- and shear-rate-dependent viscosity is handled correctly." Any assertion of this sort needs to be supported by an analysis that explicitly shows how super-elevation is related to pressure- and shear-rate-dependent viscosity. The relationship seems completely opaque to me. I examined the paper by Scheidl et al. (2015), which the authors cite in this regard, but I found no derivation that accounts for the effects of pressure- and shear-rate-dependent viscosity on super-elevation.

On p. 6388 the presentation of super-elevation comparisons concludes with disclaimers explaining why the model predictions and experimental results aren't entirely comparable, and it ends with the statement, "Nevertheless, this example is included to illustrate that the model can predict plausible superelevation." This statement seems honest, but it also seems at odds with the statement on page 6387 which, as noted above, indicates that "a correct representation of super-elevation of deflected material by the model would suggest that the interplay between pressure- and shear-rate-dependent viscosity is handled correctly." The paper contains no real evidence in support of this statement.

On p. 6391 and in a few other places in the text, the paper states that the bumpy bed in the USGS debris-flow flume was surfaced with hemispherical bumps. In fact, the bumps were round-nosed cones. I could have provided a mathematical description of these cones and even a high-resolution DEM of the entire flume bed.

On p. 6392 the paper briefly states that the model neglected opening of the flume headgate. This is the first place in which I encountered any mention of initial conditions used in the modeling. I take this statement to mean that the model assumes that, at time zero, a traction-free vertical face of debris exists at the location of the headgate (i.e., an ideal, instantaneous "dam break"). This assumption is incompatible with the proper representation of boundary tractions in a full 3-D model.

On p. 6393 I do not understand the meaning of sentence fragment, "The model could to predict flow depth developments over time."

On p. 6394 the paper mentions "material losses to the walls in the model by the no-slip boundary condition." I'm not sure of the exact meaning of this statement, but it appears to mean that the model does not conserve mass when a no-slip boundary condition is imposed. That could be a very serious problem, indeed. It may call to question the veracity of all of the computational results.

On p. 6395 the paper states that "Ensemble-averaged time evolutions of flow depths and basal pressures of twelve such experiments were compared to the model output." This statement is inaccurate, because only eight such experiments were conducted.

On p. 6396 and continuing on the next page, the paper digresses into a brief and not very useful discussion of the debris-flow model of Iverson and George. The paper's characterization of the Iverson-George dilatancy angle is particularly confusing. In fact, in that model, the tangent of the dilatancy angle is simply the difference between the ambient solid volume fraction and a solid volume fraction that is equilibrated to the ambient shear rate and stress state. As explained by Iverson and George, calculation of the equilibrium solid volume fraction is based on experimental results reported by Boyer et al. (Phys. Rev. Lett., 2011). However, I'm not sure that any of this has any real bearing on the authors' model or their results.

On p. 6397 the paper states that simulations of the gate-opening process were performed. This statement seems to be at odds with previous statements concerning how the effect of the gate was modeled. To avert great confusion, it would be helpful if the authors would state clearly the initial conditions they used in all of the simulations they

report.

On p. 6397 the paper surmises that "the observed bounce-back of the gate might play a role as well [in producing a secondary surge]." I have personally watched the gate-opening process in more than 100 of these experiments, and I can report that the effects of the gate bounce-back are minimal in comparison to those of a two-stage failure of material behind the headgate, which occurs very commonly. Development of a large secondary surge is also influenced by the presence of a small pond of water behind the impounded sediment and by grain-size segregation that develops as flows travel downslope.

Near the bottom of p. 6397 the paper makes some sweeping statements about the shortcomings of other debris-flow models, without mentioning any specific models. Not all debris-flow models have the shortcomings described here.

On p. 6398 the authors report that their simulations required as much as 10 hours of computation time per 1 second of real time on a Linux cluster with 32 processors. This is a very slow computational speed in comparison to that of simulations using the depth-integrated model D-Claw of Iverson and George, which runs about 300 times faster than this when executed on an ordinary laptop computer. The difference in computation speeds raises a question concerning the practical utility of the authors' 3-D model versus depth-integrated models.

On p. 6398 I don't understand Conclusion 3: "The model can account for the sensitivity of the rheology to channel geometry…" Rheology ought to be 100% independent of the flow-path geometry. If the rheology is sensitive to the flow-path geometry, then the rheology is wrong.

FURTHER COMMENTS ON SECTION 2.3: "Large-scale experiments: effects of bed roughness and share of fine material"

I have more to say about his section of the paper, because it utilizes results from debris-flow flume experiments I conducted myself. A general question I have is why the authors did not model the mean behavior of the three well-documented sets of 8 to 11 replicate experiments reported by Iverson et al. (2010). Instead they chose to model a few individual experiments, one of which was poorly documented and rather inappropriate as a model test. Moreover, the authors report no results in which they modeled experimental flows that had minimal (<1%) silt and clay contents. I would expect that such experiments would provide a nearly ideal type of test of the authors' model, because the model purportedly is able to distinguish the rheological effects of fine-grained sediment from those of coarse-grained sediment. With an absence of fine-grained sediment, the model should reduce to a simpler and more unambiguously testable form.

COMMENTS ON SECTION 2.3.1

An unfortunate choice for model-testing purposes was debris-flow flume experiment 970722 (conducted July 22, 1997). We posted video footage of this experiment in our on-line video archive, as we have done for every experiment we've conducted, but we never bothered to fully process the sensor data from this experiment or of its replicate on July 24, 1997. The reason is that we considered these to be flawed experiments. The problem with the experiments was that we were unable to thoroughly saturate the debris with water owing to its high content of silt- and clay-sized material. Indeed, our best estimate was that the material was only about 2/3 saturated at the time it was released from the flume headgate. Nevertheless, the authors state (p. 6389) that they assumed the material was fully saturated. I also wonder what the authors assumed about the volume of material loaded behind the headgate. In fact, only 8.4 cubic meters of sediment was used in this experiment. The authors also made assumptions about the clay mineralogy of the fine sediment that was used. I personally have no knowledge of the clay mineralogy, but I could have provided the authors with a sample of the material for x-ray diffraction or other analysis if they had asked.

Another unfortunate aspect of the authors' choice of the 970722 debris-flow flume ex-

periment is the lack of sensor data that would provide a detailed basis for comparison with model predictions. As a result, the authors based their comparisons exclusively on information extracted from relatively low-quality video recordings. Far better choices for comparisons are provided by the 28 experiments analyzed by Iverson et al. (2010).

COMMENTS ON SECTION 2.3.2

I am curious as to why the paper compares model predictions of evolving flow depths and basal normal stresses with those measured only at the location 32 m downslope from the headgate in the set of "SGM rough bed" flume experiments reported by Iverson et al. (2010). Similar sets of measurements were reported for locations 66 m and 90 m downslope from the headgate, affording an opportunity for more thorough model testing. All of the data are freely accessible at the AGU ftp site listed in the paper of Iverson et al. (2010).

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