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 1: model description**” by A. von Boetticher , J.M. Turowski, B.W. McArdell, D. Rickenmann, and J.W. Kirchner

SUMMARY REMARKS

As a preface to my comments I will disclose that I reviewed this paper because I was asked to review its companion paper (part 2: model validation). I felt that it was necessary to have a firm understanding of the first paper (part 1: model description) in order to understand the second. Consequently, I decided to review paper 1 as well as paper 2. However, after reading paper 1, I still lack a firm understanding of the authors’ model. The paper provides an unsatisfying “model description” because it presents neither derivations of the model equations nor much in the way of data to support them. Instead, it presents a brief summary of the equations and a qualitative description of the computational strategy used to solve the equations. It would be more satisfying to see a precise derivation of the model equations as well as illustrations of how they’re constrained by data. As it stands, the paper leaves much room for doubt about how the model actually works. My comments below elaborate this view.

COMMENTS ON MODEL EQUATIONS’ INCONSISTENCIES

I’m perplexed by several mathematical attributes of the model. Parts of it appear to be internally inconsistent.

The text characterizes the model as “multiphase,” and the forms of equations (13) through (16) do indeed imply that the concentrations of different mixture constituents can evolve during transport. (Evolution of constituents’ concentrations is a central feature of the continuum theory of multiphase mixtures.) Yet elsewhere in the text and equations, the velocities of all constituents are treated as identical, and dispersion of constituents by diffusion or other means is explicitly neglected. Thus, I can find no evidence of any physical process that would allow the concentrations of different constituents to evolve. As a result, it appears that the model is not really a multiphase mixture model but is instead a one-phase model that calculates the behavior of a fluid with an evolving free upper surface but with a fixed composition and complex rheology described by equations (1) through (9). The authors should either clarify or refute this key point. (I will also mention that a 50-year history exists of using complex, nonlinear rheological models to simulate the behavior of single-phase debris flows. Much of the research community has abandoned such models in favor of mixture models that simulate interactions of solid and fluid phases with evolving concentrations.)

If the authors' model somehow does allow for differential advection of constituents with different densities, then this advection prohibits the use of a single momentum-conservation equation for the mixture as a whole (i.e., the authors' equation (10)). (One cannot calculate the evolving momentum of a multiphase mixture by simply summing the momenta of the phases, because the nonlinear advective acceleration terms in the momentum-conservation equations for each phase do not sum to yield the advective acceleration of a mixture whose density is the concentration-weighted sum of the densities of the constituents. See, for example, Iverson, "The physics of debris flows," Reviews of Geophysics, 1997). Additionally, equation (10) includes no gravitational body force. Isn't such a force necessary to drive debris-flow motion?

COMMENTS ON THE RHEOLOCIAL MODEL

Assuming that the authors' model is, indeed., a one-phase model that calculates the behavior of a homogeneous, constant-density fluid with a complex rheology described by equations (1) through (9), then issues exist concerning how the rheological model is presented. First and foremost, the complete rheological model should be written in an explicit form that shows how all components of the amalgamated mixture stress tensor are related to those of the mixture rate-of-deformation tensor. At present the rheological model is presented piecemeal, and several of the pieces have issues.

For example, why is equation (1) presented as a scalar equation? Isn't a frame-invariant vector-tensor form of the equation required in order to apply it in 3-D computations? The information provided by the authors is insufficient for me to try to guess how they've implemented equation (1) in 3-D. Thirty years ago I addressed a similar 3-D rheology problem involving nonlinear, pressure-dependent viscoplasticity (Iverson, "A constitutive equation for mass-movement behavior", J. Geology, 1985). I subsequently abandoned that approach as suitable for describing the behavior of debris flows and landslides, but the approach highlighted some issues concerning material frame invariance, which the authors do not address.

Some equations that are presented in vector-tensor form by the authors also have issues. For example, consider equation (6),

$$\mathrm{T}_s = -p\,\mathrm{I} + 2\nu_s\,\mathrm{D} \ ,$$

in which T_s is defined as the Cauchy stress tensor, p as the normalized pressure, I as the identity tensor, ν_s as the kinematic viscosity, and D as the rate-of -deformation tensor. (To discover the definition of the "normalized" pressure, which is not provided by the authors, I had to consult the paper by Domnik and Pudasaini, 2012. That paper defines normalized pressure as pressure divided by density.) With these definitions in hand, equation (6) is dimensionally inhomogeneous and consequently invalid. (The inhomogeneity follows immediately from the fact that T_s has dimensions of M/LT^2, p has dimensions of L^2/T^2, ν_s has dimensions of L^2/T, and D has dimensions of 1/T.) It appears that what the authors intended was for T_s also to be "normalized" by dividing it by the density, but their paper mentions neither this definition nor the formal definition of p. Instead, as a reader, I've had to decipher the authors' intent through my own detective work.

Another issue with equation (6) is that T_s must be a stress deviator tensor, and not the full "normalized" Cauchy stress tensor. This distinction is evident from the fact that the isotropic stress component pI has been isolated from T_s. With this interpretation, equation (6) is precisely the standard constitutive equation for an incompressible Newtonian fluid with a rate- and state-dependent kinematic viscosity, which is defined in equation (9). It would be helpful for the authors to explain, in physical terms, why they believe that stresses within a deforming granular material can be accurately modeled using this approach. A comparison with data would be especially helpful. (Merely citing precedents of usage in other papers places the burden of seeking an explanation on the shoulders of readers, which is unfair. In scientific literature, the burden of explanation should be borne by authors, not by readers.)

Another mathematical issue appears in equations (7) and (9). Those equations employ the function $\exp(-m_y \|\mathrm{D}\|)$, where m_y is a pure number that the authors set equal to 2, and $\|\mathrm{D}\| = [2\,\mathrm{tr}(\mathrm{D}^2)]^{1/2}$ is a norm of the deformation-rate tensor. The authors fail to clarify why this particular norm provides an appropriate gauge of the magnitude of the tensor (as other scalar norms and tensor invariants of D also exist), but in any case the physical dimensions of $\|\mathrm{D}\|$ are the same as those of D, and are equal to 1/T. This constraint indicates that $\exp(-m_y \|\mathrm{D}\|)$ is an invalid mathematical operation, because mathematical functions can operate only on pure numbers, and not on quantities with physical dimensions. (As an aside, a rate-of-deformation tensor is not the same as a "strain rate" tensor, yet the authors use the terms interchangeably when referring to D. See the classic continuum mechanics text by L.E. Malvern for a detailed clarification of this point.)

OTHER COMMENTS KEYED TO PAGE NUMBERS

On p. 6352 the authors note that they employ linear averaging of concentration-weighted phase viscosities in order to obtain an effective viscosity for the mixture. It would be helpful to see a formal mathematical demonstration of this averaging procedure that includes all components of the 3-D stress tensor.

On p. 6359 the authors advocate use of 3-D rather than depth-averaged models on the basis of improved fits to data from dam-break experiments. However, few if any natural debris flows begin with dam breaks that impose large instantaneous force imbalances. Instead, debris flows generally arise from small perturbations to statically balanced initial states. This observation motivates a key question: how does the authors' model compute the initial stages of motion of a debris flow triggered by a small stress-state perturbation such as a pore-water pressure perturbation? Because their model takes no account of solid-fluid drag, it may be incapable of representing this effect. Yet this type of scenario is far more prevalent in nature than is a sudden dam break.

On pages 6365-6367 the Discussion section begins with a literature review rather than a discussion of the authors' results. It then transitions to a brief description of findings from some test computations. Neither of these topics is addressed thoroughly, and neither really constitutes "discussion" material, in my view. Generally a discussion section follows a presentation of results, but the authors' paper lacks a "results" section.

On p. 6368 the Conclusions section states that, "… we have developed a debris flow model whose parameters can be estimated directly from the site geometry and material composition, rather than from extensive calibration." This is a strong statement that is not supported by the evidence presented in the paper.