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Interactive comment on "A new methodology to simulate subglacial deformation of water saturated granular material" *by* A. Damsgaard et al.

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General comments

With this paper the authors apply the discrete element method (DEM) to subglacial till deformation. This method was developed in the 1970s by civil engineers and has been applied with success subsequently by geoscientists to study deformation of frictional materials over a wide range of environments and scales (e.g., accretionary wedges, fault gouge). This paper builds on an earlier application of this method to bed deformation by this group (Damsgaard et al., 2013, JGR), but unlike that paper this one focuses on simulating effects of pore-water flow caused by porosity changes during shear. These effects can potentially have a significant transient influence on till shearing resistance that needs to be considered in efforts to understand the unsteady flow

of soft-bedded glaciers, particularly ice streams. Thus, this study is topical and fully appropriate for this journal. Although many of the results of the paper confirm those of previous physical experiments and calculations, the methodâĂŤthe power of which will continue to grow as computer power increasesâĂŤis novel as applied to this problem and also brings to light new results, particularly with respect to the distribution of strain in a deformable bed. Computational limitations require that till and deformation be idealized using this methodâĂŤfor example, large equidimensional grains are usedâĂŤbut the authors use clever scaling of parameters (e.g., water viscosity) in simulating water-grain interactions to account for such idealizations. The authors deserve a great deal of credit for achieving the difficult task of applying this methodology to shearing of subglacial till. I think there is little doubt that this paper should be published.

Prior to that, however, the paper can be improved in significant ways. In my rating I characterize these improvements, as "minor" because I do not see a need for this paper to be re-reviewed. I elaborate on these improvements below in specific comments keyed to page and line numbers.

1) Better articulation of some concepts in the abstract.

2) Clarifications to some of the methodology, particularly with respect to water-grain forces.

3) More emphasis earlier in the paper that dilatant hardening is fundamentally a transient process because porosity increase occurs only in the early stages of shearing. Also, this fact needs to be reconciled with dilation continuing throughout these numerical experiments, with dilation rate in some cases not decreasing as strain approaches the largest values considered. This unexpected aspect of the numerical results needs to be explained. Also requiring clarification is that as dilation proceeds in the experiments and after pore-water pressure becomes essentially steady, shear resistance is steady, even though the bulk friction angle and hence shearing resistance should decrease as dilation proceeds (as porosity increases and dilation angle decreases).

4) Related to #3 is the authors' use of the term "rheology." In the most technical use of this term, which I think is appropriate here, it refers to a steady-state relationship between stress and strain rate (e.g., the rheology of ice). The authors should make it clearer to readers that the fluid-grain interactions that they simulate do not actually bear on the "rheology" of till. Rather they bear on the transient shearing resistance of till and its attendant effects.

5) Support for compaction-driven weakening during shearâĂŤadvocated in the paper but not demonstrated in the modelingâĂŤcould be supported more effectively by citing physical experiments that have demonstrated this effect.

6) In the conclusions section, results that are new need to be distinguished from those that confirm the results of physical experiments and calculations conducted previously.

Specific comments

page 3618, line 12-26. The physics of the dilatant hardening and compaction weakening could be better described in the abstract, which right now does not make clear the central role of porosity change: for example, the idea that porosity increase, if driven at a sufficient rate by rapid enough shearing, causes pore pressure decline that can strengthen till by an amount that depends inversely on the till permeability, is not brought out well. Also the abstract would benefit from making it clearer that hardening or weakening are transient phenomena because porosity change occurs during only the early stages of shear.

3619, 7. Odd wording. Ice streams are constituents of the ice sheet, not its mass balance.

3619, 9. "Majority" is meant to be applied to a population of discrete items rather than to continua. How about "Although most flow-limiting . . ."?

3620, 10-30. See my comment #4 above.

3621, 6-8. It perhaps also needs to be communicated here that any water-saturated

granular material also flows rate independently during slow, steady (critical-state) shear.

3622, 6. Probably not a good idea to start sentences with symbols, particularly lower case ones.

3622, 9. To the uninitiated this inter-particle overlap will seem non-physical, so this needs little more explanation.

3699, 11. Can it be clarified here for readers whether this friction coefficient is equivalent to the bulk Coulomb friction coefficient like that determined in a soils test, where the coefficient depends on both surface friction and dilation angle?

3622, equation 3. ksubt, which I assume is an elastic modulus, does not seem to be defined.

3623, 2. It might help readers here if they could be informed why the water, which typically is viewed as incompressible, must be considered to be compressible for this kind of computation.

3625, 4. Spelling: exert.

3626, 15-25. Please make it clear here how the drag forces, pressure gradient forces, and viscous forces are different. If the effects of inertia are discounted, I (and likely others) would expect these all to be manifestations of the same thing (consider their equivalence in the familiar Stokes Law of particle settling). Readers need a lot more help here.

3628, 8-9. The authors should better justify the statement that in coarse-grained tills their hydraulic diffusivity will exceed the hydraulic diffusivity of the ice-till interface. It is not obvious that this is (or should be) true. Why would, for example, an interface consisting of linked macroscopic cavities behind clasts have a lower diffusivity than a coarse grained till?

3628, 13. This phasing suggests there in a range of particle sizes. As I understand it, there was a single particle size used.

3628, 25. Table 1 should probably be cited here, so readers can access the actual particle size.

3631, 2. This sentence could be taken to imply that the critical state was reached in these experiments, but Figure 4 indicates that dilation was both occurring and not slowing down at the highest strains attained. Porosity, of course, should be steady during critical-state deformation.

3631, 22-26. The results of Figure 9 are quite interesting. However, the caveat should probably be added that these profiles reflect small total strains. In a glacier bed the total strain may greatly exceed the strain that accrues during dilation, such that the cumulative deformation profile will be insensitive to the short period early during deformation when the till was dilating.

3633, 23. This statement that dilation ceases in the critical state begs the question of why dilation has not stopped by the end of the experiments (Figs. 4, 7), even though friction is steady (disregarding high frequencies) at higher strains in the experiments and pore pressure is steady. Dilation under a constant effective normal stress should be accompanied by a decrease in shearing resistance as the porosity and hence friction angle decrease. This needs to be explained. My apologies if I am missing something here.

3634, 5. Here the authors can do better than to “speculate” about the mechanical consequences of compaction-induced weakening. Compaction-induced weakening is a leading hypothesis for debris-flow mobilization from landslides and a process that has been demonstrated experimentally at small scales (Iverson et al., 2010, Eng. Geol., 114, 84-92) and field scales (Iverson et al., 2000, Science, 290, 513-516). The mobilization occurs during the early stages of landslide motion when soil is shearing slowly with negligible inertia, so these experiments are relevant here. Some clay was present

in these experiments but more important than clay content was the initial soil porosity relative to the critical state value. This is a factor that is not brought out well in the paper: the important role of initial porosity relative to the critical state value at a particular effective stress. For example, a subglacial till (regardless of its clay content) that has stopped shearing in its critical stateâĂŤas a result of, say, decoupling with iceâĂŤwill compact the next time it shears if effective stress is higher when shearing is renewed.

3634, 11-12. See comment 3631, 22-26. High shear strains that accrue during critical-state shearing may result in strain distributions that swamp the strain distribution acquired during dilation.

3634, 19-21. These observations of very shallow deformation are also, however, consistent with rate-weakening associated with plowing at the ice-till interface.

3634, 26-27. Importantly, this new deformation will not be accompanied by dilatant strengthening unless some mechanism of till-density recovery is invoked. The authors might want to make it clear that this will be a one-off process unless tills compact once shearing stops. I think this process (Iverson, 2010) merits further study and is one that could perhaps be addressed with DEMs. It might be a factor in stick-slip basal motion.

3635, first paragraph of conclusion section. This is a bit misleading. It should be made clear in this paragraph that many of these conclusions are not new but confirm the results of previous physical experiments on less idealized materials (e.g. Moore and Iverson, 2002) and of previous calculations (e.g. Iverson et al, 1998). See Iverson (2010) for a review.

3635, 18-19. “The porosity of a granular packing evolves asymptotically towards a constant value when deformed.” This is true, but as noted in my earlier comments, porosity was still steadily increasing at the ends of these experiments, so the experiments seemingly do not demonstrate this effect.

3623, 26-27. This statement that a plastic “rheology” applies for permeable or slowly deforming till suggests that it does not apply otherwise. I would argue that it always apples during steady-state non-inertial deformationâĂŤthe conditions under which the rheology of a creeping material is usually defined. See my comment #4 above.

Interactive comment on The Cryosphere Discuss., 9, 3617, 2015.