Author reply to reviewer comments

“A new methodology to simulate subglacial deformation of water saturated granular material” by A. Damsgaard et al.
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We appreciate the support and constructive reviews of both referees. Our primary intention with this manuscript was to introduce the numerical method and its application. We confirm prior laboratory studies of dilatant hardening while providing new insight into granular kinematics and pore-water fluid dynamics during deformation. In the revised manuscript, we have made a clearer distinction between results from previous studies and our own novel findings. The reviewer comments are included below and we address all points in turn.

N. R. Iverson (Referee)

The review text unfortunately contained what seemed to be numerous text encoding errors, which manifested themselves as random characters (e.g. âĂŤ). We’re certain that these did not influence our understanding of the referee, however.

General comments

C1	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 method âĂŤ [sic] the power of which will continue to grow as computer power increases âĂŤ [sic] is 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 method âĂŤ [sic] for example, large equidimensional grains are used âĂŤ [sic] but 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.
R1	We are glad that the referee acknowledges the potential of the computational method and recognizes our intentions with this manuscript. We aimed at 1) comparing our results to those of previous studies in order to increase confidence in the numerical method, while 2) using the vast amount of generated data to present new insight into the dynamics of pore-water pressure and the small-scale interactions between grain and pore-water during deformation. The shortcomings of the method with regard to grain size and grain size distribution width are immediately clear; few tills are as simple in grain shape and grain size distribution. These limitations have been previously discussed, and we retain the opinion that despite the simplifications we can obtain insight into fundamental granular mechanics (Damsgaard et al. 2013). We believe that the strength in the computational method lies within the reproducibility of experiments, the flexibility of experimental setups, and the possibility to analyze micro-mechanical deformation patterns during progressive shear strain.
C2	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.
R2	Thank you for the suggested improvements. We have revised the manuscript accordingly.
C3	1) Better articulation of some concepts in the abstract.
R3	We have rewritten parts of the abstract according to your specific comments, as well as condensed and reworked the wording in order to improve readability.
C4	2) Clarifications to some of the methodology, particularly with respect to water-grain forces.
R4	In hindsight we agree that simply mentioning the other grain-fluid interaction terms is not sufficient. We have therefore strengthened the general description of interaction forces, with the intention of clarifying their physical basis and their relevance for this study.
C5	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).

R5	According to your specific comments (e.g. C9, C12, C24, C25, C26) we have adjusted the manuscript to better communicate the transient nature of the dilatant hardening mechanism. Under the imposed boundary conditions, the dilatant hardening only contributed to the bulk shear strength at low strains until an early and narrow shear zone was developed. The shear zone continued to expand as the low pore-pressures at greater depths readjusted towards the hydrostatic pressure, and the shear zone slowly deepened until it corresponded to the deformational pattern in the dry control experiment. This shear-zone widening caused the slow trend of continued dilation, but did not influence shear strength significantly. This has been clarified in the revised manuscript, see R24 to R26.
C6	4) Related to #3 [C5] 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.
R6	We agree and have refrained from the term "rheology" unless referring to steady (critical) state.
C7	5) Support for compaction-driven weakening during shear âA˘T˘ [sic] advocated in the paper but not demonstrated in the modelingâA˘T˘ [sic] could be supported more effectively by citing physical experiments that have demonstrated this effect.
R7	Thanks for the references, we have added them at the appropriate places in the text.
C8	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.
R8	The relevant parts in the conclusion have been rewritten to clearly distinct between the two.

Specific comments

C9	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.
R9	Thank you for the suggestions, we have worked on the abstract and hope that the central role of porosity change comes across better. Also we have highlighted the transient nature of the strengthening. We added that the dilatant strengthening

	observed here is consistent with previous studies, and we have highlighted the new findings associated to distribution of strain.
C10	3619, 7. Odd wording. Ice streams are constituents of the ice sheet, not its mass balance.
R10	Corrected.
C11	3619, 9. "Majority" is meant to be applied to a population of discrete items rather than to continua. How about "Although most flow-limiting . . ."?
R11	Thanks, corrected.
C12	3620, 10-30. See my comment #4 [C6] above.
R12	The first paragraph is intended to introduce previously suggested relationships for (steady-state) rheologies. In the second paragraph we attempted to present a much more generalized view of grain-fluid mixtures beyond subglacial till. In some cases, the fluid phase will contribute more to the strength than the solid phase. We acknowledge that this realization is not all that relevant in this setting and have removed the opening sentence of the second paragraph. We have also noted that the influence of pore-water on material strength is transient and most relevant to the earliest stages of shear deformation.
C13	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.
R13	Absolutely, this has been added. We did note that local porosity changes in the critical state may initiate liquefaction events (Goren et al. 2011).
C14	3622, 6. Probably not a good idea to start sentences with symbols, particularly lower case ones.
R14	Several sentences have been restructured.
C15	3622, 9. To the uninitiated this inter-particle overlap will seem non-physical, so this needs little more explanation.
R15	We have elaborated on the numerical principle behind the DEM.
C16	3699 [3622], 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?
R16	Good point, we now underline that the macroscopic frictional strength depends on inter-particle friction, inter-particle elastic stiffness, as well as particle packing.
C17	3622, equation 3. ksubt, which I assume is an elastic modulus, does not seem to be defined.
R17	Corrected, see also R40-41.
C18	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.

R18	We do not believe water compressibility is a major contribution to the fluid dynamics presented here. However, we do not exclude it because the numerical method does not require us to impose this simplification. A clarifying sentence has been added to the text.
C19	3625, 4. Spelling: exert.
R19	Thanks, corrected.
C20	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.
R20	This paragraph has been expanded by more thoroughly describing the other water-grain interaction forces, and adding several references. If we were to include the equations for these other interaction forces they add a page of text and introduce many new symbols.
C21	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?
R21	Good point, we have elaborated on the variability in the text, and we now include a specific reference to linked cavity systems.
C22	3628, 13. This phasing suggests there in a range of particle sizes. As I understand it, there was a single particle size used.
R22	We have rewritten the sentence to note the uniform particle size.
C23	3628, 25. Table 1 should probably be cited here, so readers can access the actual particle size.
R23	Thanks, reference to table 1 was added to the text.
C24	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.
R24	This is an interesting observation. The shear strength seems to have reached the critical state, while the slow continuous dilation suggests otherwise. Dilation is caused by changes in porosity, which in the normal-consolidated material is increased by shear deformation. We have looked into the spatial and temporal porosity evolution for the simulations presented in Figure 4 and 7, and have added the following observations to the results section: We observe that the porosity and grain velocity distribution in the experiments at higher strains evolve towards the distributions in the dry experiment. The dilatant strengthening keeps the shear zone shallow during relatively low shear-strain values (< ~2), but vanishes as the low pore-water pressures in the deeper sediment

	volumes goes towards the hydrostatic pressure distribution. When the hydrostatic pressure distribution is recovered, the dilation and displacement profile is identical to the dry experiment. The dilation slowly increases while shear strength only displays very slight decrease during this asymptotic evolution. This has been stated in the revised paragraph.
C25	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.
R25	Good point. The deformation profile in subglacial beds is in many cases likely the product of large displacements in the critical state, unless frequent consolidation episodes take place. The question remains if diurnal or annual changes in basal stresses and movement are sufficient to cause this consolidation, as discussed by Iverson (2010). We also note that, according to C24, the dilatant hardening only influences strain distribution during low strains.
C26	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.
R26	Continuing from the insight obtained from C+R 24-25, we were surprised to see that the critical state in terms of friction seemed to be established much earlier than the critical state in terms of dilation and porosity. It can thus be concluded that the sediment strength does not seem to be greatly increased if the shear zone thickness is limited by pore-pressure gradient hardening. We have changed the wording to "Dilation ceases when a sediment reaches the critical state and the hydrostatic pressure distribution is recovered".
C27	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 stateâA˘T˘ [sic] as a result of, say, decoupling with iceâA˘T˘ [sic] will compact the next time it shears if effective stress is higher when shearing is renewed.
R27	Thanks, the references have been added. We strongly agree on the role of initial porosity, and it is important to make this clear in the paper. We have elaborated on that critical state shear takes place at a certain constant porosity. The shear zone porosity will evolve towards this value regardless of whether it initially was higher or

	lower. The accompanying porosity changes will be able to provide transient softening or hardening.
C28	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.
R28	We agree, and the caveat has been stated.
C29	3634, 19-21. These observations of very shallow deformation are also, however, consistent with rate-weakening associated with plowing at the ice-till interface.
R29	We agree and added a reference to Thomason & Iverson 2008. We also referred our earlier DEM study, which argued that low deformational depths are associated with small effective normal stresses and small grain sizes.
C30	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.
R30	Absolutely. We have noted the important role of inter-slip consolidation. Thanks, we have clarified this paragraph accordingly with references and more careful wording. We also now state that this *confirms* previous studies, and we added the suggested references.
C31	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.
R31	We have updated the phrasing of the first sentence to note that our results confirm results from previous studies.
C32	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.
R32	See R24 to R26.
C33	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 deformationâA˘T˘ [sic] the conditions under which the rheology of a creeping material is usually defined. See my comment #4 above.
R33	We agree and have rewritten the sentence to better reflect this sentiment.

Anonymous Referee #2

General comments

C34	The authors describe an elegant numerical model of a deforming granular subglacial material. (I am not qualified to evaluate the numerical model.) It is satisfying that the results are consistent with laboratory experiments and well-understood physics. However, the conclusions are, I think, already pretty well-known, and in that respect the paper does not seem to provide much insight into subglacial processes. Perhaps the paper should be written as a model description and validation paper, as the title actually suggests. This would require minimal change. It would simply involve statements along the lines of, “See, the model does what we know it should do, so despite its short-comings (large grain size, no clay,…) it is reproducing nature.” Alternatively, perhaps the authors can make some predictions using the model that are not already understood, but are supported by field evidence.
R34	We thank the reviewer for the many constructive comments. We have reworded many statements to reflect that this study confirms prior theory and experimental results of dilatant hardening, see e.g. R30 and R31.
C35	Under the best conditions modelled, deformation extended only two to three decimeters into the bed, yet it is known that deformation extends to greater depths in nature. Can the authors explain what is necessary to get deformation at greater depths? I don’t think this is really understood, and it would be a nice contribution.
R35	We think that this is a very relevant question in the discussion of subglacial sediment deformation. Studies of simple granular materials, like this one, have provided a few clues. Due to the boundary conditions imposed for the fluid phase, we are not able to fully address the issue here, but would like to elaborate in the following. Based on granular material literature and our own observations, there are strong indications that shearing of sediments always results in a minimum shear zone thickness, dictated by grain size (Tulaczyk 1999, Damsgaard et al. 2013). In the absence of strong cohesion or softening mechanisms, sediments do not fail along infinitely thin planes, which critics of the plastic rheology previously claimed. Distributed strain is alone not enough to discard the plastic rheology, which dry granular materials deforming in the pseudo-static regime accord to. We have observed that shear zones tend to be wider with increasing effective normal stress magnitude (Damsgaard et al. 2013), which in turn results in a stress-dependent sediment transport. Dilatant hardening has previously been considered a mechanism for distributing shear with depth in the subglacial bed. In this manuscript we hope to show that the dilatant hardening does not necessarily distribute strain. As in many other aspects of subglacial mechanics it comes down to the nature of subglacial hydrology. The deformation will occur at the ice-bed interface here if there is sufficient water available to keep water pressure at a high level.
C36	The “Results” section contains a lot of unsubstantiated statements and

	interpretation. Interpretation should be clearly distinguished from the “facts” that are evident in graphical (or numerical) results.
R36	We have removed several sentences that contained preliminary discussion of the results. Some of the content has been moved to the relevant subsections in the discussion.
C37	The changes in peak stress and mean fluid pressure from one experiment to another are very small and if they were based on physical experiments, most readers would consider them to be within the limits of uncertainty of the experiments. What happens if you repeat an experiment from the beginning, numerically dumping a new assemblage of particles (with the same particle size distribution) into your “dry, tall volume”? Are the results in Figures 4 and 7 reproducible to the degree that you can argue that the differences among panels of those figures are real?
R37	Good point. We do believe that the observed strengthening is reflecting real processes, since the starting material for the experiments with different shear velocities is completely identical, all the way down to individual grain arrangement. This statement is now included in the text. We are certain that the observed weak strengthening is not purely stochastic. Unfortunately due to the heavy computational cost it takes 3 to 4 months for each reiteration of the experiments on different starting material, so performing many “control experiments” in order to investigate the effect of variability in the granular material is not viable.

Specific comments

C38	(p. 3, Line 23) “…is, over time, carried…” Add commas
R38	Corrected.
C39	(p. 5, Line 5) “In this study, we explore the…” The study is not a person and thus can’t, itself, do anything.
R39	Good point, corrected.
C40	(p. 6, Lines 5-14 and elsewhere later) Please define all symbols used in your equations. Here I don’t see definitions of $\boldsymbol{g}$, t, r, and k. Two of these will be fairly obvious to most readers, but don’t leave the reader guessing. In equation (6), V_g is not defined and in equation (8) $\boldsymbol{v}$ is not defined.
R40	Apologies for these mistakes. We have made sure to adequately refer all parameters in the text.
C41	(p. 6, Line 17) How is linear elasticity involved? I don’t see any elastic constants in any of the equations.
R41	k_n and k_t are the elastic stiffnesses in the grain-to-grain contact model. We hope that the clarification from R40 has resolved this issue.
C42	(p. 7, Line 6) “…2012) because it allows convenient…” “Since” involves time.

R42	Thanks, corrected.
C43	(p. 7, Line 14-15) Symbol *k* now appears to have a different meaning than in equation (3).
R43	Yes, k without subscripts is in this context permeability. This is explained a bit later when the Kozeny-Carman relationship is introduced. Here we have added a sentence explaining the meaning explicitly.
C44	(p. 9, Line 13) "…and is of…" Add "is"
R44	Thanks, corrected.
C45	(p. 9, Line 16) "…constraints…" Second 't' is missing.
R45	Corrected.
C46	(p. 9, Line 17-18) If I understand this, you mean, "…we are unable to give fine grain sizes ~~with~~ realistic elastic properties ~~within a reasonable time frame~~."
R46	The length of the computational time step is proportional to grain mass and inverse proportional to elastic stiffness. We aim for realistic stiffness as porosity depends on the grain bulk modulus. Smaller grain sizes would require substantial softening of the grains, but the discrete element method assumes that compressive strains of the grains are negligible.
C47	(p. 13, Lines 9-17) (*i*) You write this as if it were a physical experiment in a soil mechanics laboratory, not a numerical experiment on a computer. (ii) Also, perhaps here, you should say something about the physical size of the modelled domain (0.4 m according to Figure 5)
R47	(i) These paragraphs describe the actual procedure followed, including filling the bounding box, consolidating the loose material, and applying shear deformation. We substituted placed with positioned, and added noted the bounding box in order to refer to the artificial nature of the experiments. (ii) The physical dimensions have been added to the text. They are also accounted for in table 1, referenced at the end of the second paragraph.
C48	(p. 14, Line 9) "…viscosity, the…" Add comma
R48	Comma inserted.
C49	(p. 14, Lines 21, 23) Neither Figure 4 nor Figure 5 show rates of dilation. The middle diagrams in Figure 4 show that dilation increases roughly linearly with shear strain, but to get a rate out of that one has to – what(?) – also divide by the shear velocity?
R49	Yes, divide by shear velocity. Due to constant imposed shear velocity the first derivative of the dilation curve will be the dilation rate. The referred sentences have been removed, however. See R36.
C50	(Figure 4) You seem to be equating "peak strength" in the caption with "shear friction" on the y-axes in the top row. Why use different terminology?

R50	We consider the two to be different expressions of the same mechanism as effective normal stress is defined and constant. We have rewritten the text favoring friction over strength. See also R55.
C51	(Figure 5) (*i*) Lettering is too small on axes. (*ii*) "…fluid pressures (y axis)…" If I am interpreting the graphs correctly, the fluid pressures are not shown on the y-axis, they are shown by the color scale. The y-axis appears to me to be the vertical height above the bed.
R51	(i) We intended this figure to be printed as an entire column of an a4/letter type paper. The figure text size did not respond well to the shorter review layout in which this manuscript was typeset. We hope that the figure reads better in the final publication. (ii) You are reading the figures correctly. Apologies for the confusing text in the figure caption, which has been improved.
C52	(p. 14, Lines 25-27) I read the peak values in Figure 4 at 0.65 and 0.61 respectively.
R52	The smoothed shear friction values presented in Figure 4 and 7 were produced using a 5x stronger smoothing than for Figure 6. We have included new versions of Figure 4 and 7 using the weaker smoothing, which more precisely captures the early peak values while discarding the high-frequency fluctuations due to the material granularity.
C53	(p. 15, Lines 7-8) What is the "pure granular strength"?
R53	It is the strength of the granular material alone without any fluid interaction, as for dry granular materials. This information has been added to the text.
C54	(Figure 6) (*i*) Why are there two points at a shear velocity of 10^2? (*ii*) The "constant frictional strength" at low shear velocities is not shown in the graph. In fact, if I draw a curve through the points shown, peak shear friction continues to decrease at shear velocities less than 10^1 m a^{-1}.
R54	(i) Scaling fluid viscosity and sediment permeability are two different approaches, which ultimately result in the same effect on the hydraulic diffusivity. For resolving the peak strength at 10^2 m a^{-1} we both use the shear experiment with a permeability prefactor 1/10 the value at 10^3 m a^{-1}, and a fluid viscosity 1/10 the value at 10^3 m a^{-1}. The results are not exactly similar as fluid viscosity not only influences the diffusion term in the pore-pressure equation, but also the forcing term. We used two sets of permeability and viscosity to obtain the results at 10^2 m a^{-1}. (ii) We attempted to account for the rate-independence in the figure caption, as the data presented in the figure alone does not communicate this conditional relationship. We have added an annotation to the figure itself to mark the rate-independent domain.
C55	(p. 15, Line 12) In this line you use shear stress but elsewhere you use shear friction. What's the difference? If they are fundamentally interchangeable, I suggest using shear stress (or better yet, "shear traction") throughout (including in figures).

R55	We rewrote this sentence in order to eliminate this ambiguity. In our experiments the two parameters are interchangeable as the effective normal stress is held constant. We chose to maintain the use of friction in this manuscript as the reader will be able to understand the relative strength without referring to the effective normal stress value in the text or in Table 1. Friction is commonly used to characterize shear strength in similar studies of granular materials.
C56	(p. 15, Line 17) "…low-permeability…"
R56	Thanks, corrected.
C57	(p. 15, Line 18) "…largest…" The difference is pretty subtle.
R57	After applying the weaker smoothing to the frictional values (R52) the new Figures 4 and 7 show stronger hardening. Arguably the 15% increase in frictional strength from 0.62 to 0.71 is small, but it may still be significant when glacier surface slope and resultant driving stresses are low, and the ice flow displays highly variable velocities during stick-slip.
C58	(p. 15, Line 20) In both Figures 4 and 7, the deviation of fluid pressure from 0 is very subtle. You need to find a way to make it more obvious so readers can easily see what you describe in the text. You also need to find a way to convince the reader that the very small changes from one experiment to another are physically meaningful. Certainly, if you were reporting a physical experiment, most readers would consider the differences to be within limits of experimental error.
R58	We have annotated the figures to make it clear that we are referring to the earliest states of shear. Along the lines of R37 we note that the perfect reproducibility of experiments allows us to study the effects of variation in single parameters without introducing variability due to slightly different starting material, granular packing, etc.
C59	(p. 15, Line 25) "…~~more~~ shallower deformation…"
R59	Thanks, corrected.
C60	(Figure 9) (*i*) In two places in the caption you mention shearing velocities. These are not shown in the figure and should not be mentioned. (*ii*) "…porous flow…" Do you mean Darcian flow?
R60	(i) References to the shear velocity have been removed. (ii) Yes, this is now specified.
C61	(p. 16, Line 1) "values (Fig. 9 right, red)…."
R61	Statement removed, we show pressure-gradient forces instead of pore pressures.
C62	(p. 16, Line 2) "…this experiment…" (singular). "…deformation ~~is~~ in impermeable…"
R62	Thanks, corrected.
C63	(p. 16, Line 3) "…experiment is primarily…" "…top wall ~~and~~ from the…"

R63	Corrected accordingly.
C64	(Figure 10) Call the reader's attention to the fact that scales on axes differ from one plot to the next in a column.
R64	Good point, a statement about the color bar differences has been added to the legend.
C65	(Figure 11) "…low-permeability material…"
R65	Thanks, corrected.
C66	(p. 17, Line 12) "…~~reduction~~…"
R66	Corrected.
C67	(Figure 12) "…The ~~established~~ gradient in fluid pressure thus established pulls forces…"
R67	We think this comment is meant for Figure 13, and the wording has been corrected.
C68	(p. 17, Line 15) "…contacts ~~is~~ in the DEM is determined…"
R68	Corrected.
C69	(p. 17, Lines 4-20) This seems like a long paragraph to explain something that is already pretty clear and also well understood by most of your readers. It could probably be reduced to about 5 lines.
R69	We appreciate the input and have condensed the paragraph.
C70	(p. 17, Line 26) "…which alternately slightly weaken…"
R70	Added.
C71	(p. 18, Line 6) "…zone in cases…" Delete comma
R71	Comma deleted.
C72	(p. 18, Line 19) "…deformation ~~of~~ in subglacial…"
R72	Corrected.
C73	(Figures 14 and 15) These figures can be deleted. The description in the text is adequate.
R73	Figures removed.
C74	(p. 19, Line 8) "…stagnation ~~ice flow~~…" "stagnant ice flow" is a redundant. If ice is stagnant there is no flow.
R74	Got it, wording changed.
C75	(p. 19, Line 16) If clay particles are added to the water, it will be more viscous. This brings up the old debris-flow problem: what is the fluid and what is the matrix?
R75	Interesting observation. The same increase in viscosity may also takes place if ice

	crystals start to form. We are not sure of the exact rheology of such mixtures, which may very well be non-Newtonian, and have therefore not included such effects in this study. We added these considerations to the end of section 4.1.
C76	(p. 19, Line 19) "…deformed at a constant rate. Changes…"
R76	We instead chose to change the wording to "towards a critical-state value with increasing shear strain".
C77	(p. 19, Line 22-23) "…sediment dilation cause a volumetric contraction in the granular phase…" Confusing. Do you mean the grains are getting compressed elastically? If a sediment dilates, it does not occupy less space.
R77	Apologies for the confusing wording. We have reworded the sentence to "Low fluid pressures developing due to sediment dilation increase the frictional strength of inter-grain contacts".
C78	(p. 19, Line 26) "…perfectly plastic…"
R78	This sentence has been reworded according to R33.
C79	(p. 20, Line 5) I don't remember 732 m/a being mentioned previously.
R79	We have removed this statement.
C80	(p. 20, Line 11) "…millimeter-to-centimeter…" Isn't it more common to go from smaller to larger?
R80	Order changed.
C81	(p. 20, Line 15) "…These temporal…"
R81	Thanks, corrected.