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Discussions

Interactive comment on "A particle based simulation model for glacier dynamics" *by* J. A. Åström et al.

J. Bassis (Referee)

jbassis@umich.edu

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Summary:

This discussion paper describes a discrete, particle based model that can be used to simulate the dynamics of glaciers. The model that the authors present is novel in that the authors do away with the usual continuum approximations that are conventionally used to simulate the viscous flow of glaciers and simulate glacier dynamics as a discrete process involving particle-particle interactions. This choice of discretization has the advantage that it naturally leads to a formulation where bonds between particles can be broken thus allowing the ice to fracture. There is a long history of applying particle methods to study the failure of geophysical materials [Cundall and Strack, 1979; Potyondy and Cundall, 2004]. These techniques, however, have only recently been

applied to study the fracture of glacier ice. I think this type of discretization has great potential as a means of gaining insight into the fracture behavior of glaciers since it avoids the cumbersome overhead of removing nodes in continuum models. I have not seen many discrete element models that simulate both the elastic and viscous regimes so this aspect of the model is novel compared to the purely elastic models we have used to simulate different calving regimes over the past several years.

Overall, this submission verges on exciting and the model presented here appears very promising, but the manuscript seems rushed and the model description, along with experiments performed need to be better described and justified prior to publication. There are a number of hasty claims that are made with little justification and sloppy or confusing (at least to me) exposition left me thoroughly confused about what physics is actually used in the model. I had similar questions about the numerical experiments and the relationships between simulations and observations. I would encourage the authors to take the time to rework the text and perhaps even re-conceive some of the numerical experiments so that the manuscript is better able to support the claims expressed and so that the work presented can make a more coherent mark in the field.

I provide below a (very long) list of detailed comments about the model and numerical experiments. I base many of my comments on my experience with the discrete element model that I wrote. I have less experience with the melting/freezing portion of the algorithm so my comments about this aspect are less informed.

Detailed technical comments

1. Model description

a. Numerical model description and parameters: The model would benefit from a more in-depth description of the model assumptions and numerics. For example, can specific expressions be given for the elements in the mass and damping matrices? The collision damping is given in the text, but as a scalar so it is unclear why a matrix is needed nor how to form such a matrix. The damping coefficient associated with drag does not appear to be given at all in the text. Moreover, damping associated with particles falling into the water should be turbulent instead of Newtonian so that the linear dependence on velocity may not be correct. The scaling of the damping coefficients with particle size are also unclear as one normally specifies damping in terms of a force per unit particle area to give a drag stress. A consequence is that experiments with different particle sizes may not scale correctly. More significantly, without knowing how the mass matrix is formed it is unclear how the moment of inertia is defined. The moment inertial can be specified as appropriate for a sphere, disk or cylinder depending on the symmetry of the (un-modeled) third dimension [Potyondy and Cundall, 2004]. We are also left in the dark about how the equations integrated? Is the integrator simplectic, Runge-Kutta, adaptive? Do simulation results scale with changing particle size and or time-step size? How were buoyancy forces calculated? Buoyancy requires knowing the density of water, if freshwater is assumed then with the 1000 kg/mˆ3 density of ice assumed, won't this result in no buoyancy? What density is used for the water?

b. Frictional behavior of the model: From equation 1, it looks like the model includes dissipation in a normal direction to particle-particle collisions, but not dissipation in the tangential direction. Hence the model does not include friction between particles. This is an odd omission since friction is a crucial component of granular flows and brittle failure of ice under compression [Cundall and Strack, 1979; Beeman et al., 1988; Kennedy et al., 2000; Schulson, 2001]. A consequence of omitting friction from the model is that there is no angle of stable repose. This makes it very difficult to interpret the surging glacier model where it appears as though an angle of stable repose does develop.

c. Yield strength asymmetry of the fracture model: It looks like the authors are using a yield strength of ice of 100-500 kPa (based on the combination of Young's modulus and critical strain rate). This is plausible for the tensile strength of ice (although the authors should clarify which yield strength is used in each experiment). However, ice and most

other materials exhibit asymmetric failure regimes. Most particle models handle this by introducing separate tensile yield stress and a shear yield stress. (Incidentally, most particle models use both the Young's modulus and the Shear Modulus to define how the springs deform in tension and in shear. Using only Young's modulus for both shear and tension implies that the elastic behavior is that of a Poisson solid and unless I am missing something, Poisson' ratio for the model will not be 0.3 in this case.) Some models also introduce a compressive yield stress, but this is more tenuous and less obviously needed to simulate failure of ice under geophysically relevant loading. This model appears to only allow bonds to fail in tension or the yield stress in tension and shear is assume to be equal? It is unclear from the model description. Typically ice is much stronger in shear than in tension so if the latter approach is used ice will be much weaker than predicted by laboratory experiments. This might explain why the 30 m high block disintegrates. Moreover, most models attempt to simulate the elastic bonds between particles using massless beams that deform in tension and shear as is done here, but also allowing the bonds to flex. The flexure of the beams requires an additional variable related to the width/geometry and the bending moment of the bond. The flexed bond model often gives much more realistic results than models that only include tension and shear.

d. Viscous relaxation of ice: The melting/refreezing algorithm to simulate viscous flow of the ice is clever and I have not seen this before. It would be nice to see a reference to such a model in the literature for more details about how this model is able to simulate different rheologies. My crude understanding of this approach is that the Boltzman probability distribution is necessary to enforce the principal of detailed balance necessary for the statistical equilibrium desired? I'm a little bit concerned that the equations on page 926 are appropriate only for a uniaxial loading. The multi-axial generalization of Glen's flow law frequently used by glaciologists is more sophisticated than that expressed on page 926 and provides a relationship between the deviatoric stress and strain rate *invariants*. I assume that the authors are using the tensor equivalent and this is just sloppy notation. I would encourage the authors to clear this up so that readers can see (if?) the correct rheology and invariants are used in these computations. I'm also uncertain how the authors handle the temperature dependence of the viscosity factor "A". Is each particle assigned a temperature? How is this evolved? I can't find any equations relating to temperature.

e. Visco-elasticity and scaling of time: Up until here, most of my questions are related to digging under the hood of the model to figure out what is going on. But I'm not sure that it is permissible to simply rescale time since this forces brittle failure to occur over a comparable time scale to viscous deformation whereas in reality there is a strong separation of timescales. As the authors allude to earlier, most fracture and calving events occur over a time scale that is sufficiently short that viscous flow can be neglected. By slowing down the brittle-failure (or alternatively speeding up the viscous failure) failure and flow are forced to occur on comparable time scales and this can lead to unphysical feedbacks. The argument that I think the authors are relying on (although this is unstated) is that the elastic portion of the model needs to resolve elastic waves which forces them to use a small time step. However, if elastic waves are not crucial to the problem then it is tempting to slow elastic waves down to something more comparable to the viscous time scales. The appropriateness of the "slow fracture" approximation may be clarified by non-dimensionalizing the governing equations (i.e., equation 1) and defining a set of dimensionless numbers that describe the behavior. The visco-elasticity will be described by the Deborah number, the yield stress by the Bingham number and so on. As far as I understand the argument, the authors are hoping that the behavior of the model as a function of the Deborah number becomes sufficiently constant for large Deborah numbers and hence the behavior is constant across several orders of magnitude of the Deborah number. This assumption should be demonstrated numerically by conducting a sequence of experiments with a range of Deborah numbers and showing that the same behavior is obtained. However, the fundamental question that remains in my mind is: Do you need viscous creep to explain ice fracture? It isn't clear to me that you do.

Numerical experiments

a. Validation with viscous ice flow model: The fact that the model is able to reproduce the results from continuum viscous flow models is very encouraging. However, the description of this experiment is confusing. Typically viscous ice flow models do not permit fracturing so the comparison is only valid in the limit that brittle failure is negligible. However, the size of the block is identical to that of the marine ice experiment which does fail. Why does failure not occur here? Has the yield strength been increased to avoid failure? Why use a different geometry and setup from the earlier ice cube in water experiment? Also, what does it mean to reproduce Glen's flow law to "high accuracy". Incidentally, this problem is a traditional dam break problem and there are analytic solutions for this problem using a variety of rheologies [Barenblatt, 1996; Balmforth et al., 2006]. I encourage the authors to validate their model using various analytic solutions where possible to avoid any issue with numerical artifacts associated with numerical intercomparisons. Logically, I would prefer to see the validation done before I see the other experiments so I suggest swapping the order between this experiment and the ice cube in water experiment, but that is a personal preference.

b. Ice cube in water experiment: This experiment is thoroughly perplexing. First, given the symmetry of the problem, it is surprising that the cube breaks asymmetrically with the right side intact and all failure occurring on the left side. Is a boundary condition used on the right side to prevent failure that we are not told about? If not this looks like a bug in the code . . . . If a boundary condition is used on the right side, one needs to be careful about edge effects since the behavior may be dominated by the boundary conditions. Second, given the small size of the block and the fact that gravitational stresses are partially supported by buoyancy stresses, the stress within the block should be small [Bassis and Walker, 2012]. Why does a block this small fail at all? We definitely see much larger icebergs and intact ice cliffs can be much larger than 30 m. Once the block starts to fail, why doesn't the entire block disintegrate? These issues need to be explored since a 30 m block disintegrates, one wonders what happens to a tidewater glacier nearly 1000 m thick? Given these issues, comparisons to the behavior of marine terminating glacier would appear to be premature at this stage.

c. Comparison of ice cube in water with observed size distribution of icebergs: The graphs are confusing since there are no units. Is “size” the mass of blocks or volume? Also, *how* is the model consistent with observed data? Surely iceberg debris exceeds 30 m in radius? The authors need to show the observed data on the figure so that readers can assess how well the model matches observations for themselves. Just telling us that the model is consistent the observations that are not described is not satisfying. Also, you have to be very careful comparing simulated iceberg size distributions with observed size distributions. Icebergs interact strongly with the ocean as they drift, melt, collide and break apart in ocean swell [Wadhams, 2000]. Most studies assume that the size distribution of icebergs away from their source (i.e., Newfoundland) reflects ice-ocean interaction and not the size-frequency at the point of production. Furthermore, the size-distribution of icebergs reported may also result from a large sample of glaciers of different sizes. The mixing of iceberg debris from different glaciers will also create a size-frequency distribution that needs to be taken into account. It would be far better to compare with size distributions observed within fjords, but even this is tricky since icebergs can have a long lifetime in fjords. Is it possible to compare with high resolution imagery or some of the field observations reported from time lapse photography? Is the size-frequency distribution observed independent of particle size? Does it depend on the height and length of the block? Does it depend on water depth? The authors deduce quite a bit from a small block of ice. These speculations are interesting if true, but appear premature.

d. Surging glacier: This is a neat experiment. However, I’m a little bit suspicious about the lack of friction between particles. It looks like the simulation is evolving to a state where there is a linear slope of stable repose. This would imply that there is friction between particles and that the model description is not representative of the model used for this simulation? This is a problem in the exposition and I’m now thoroughly

confused about the ingredients in the models. Since friction is included between particles and the bed (at least), how is this friction parameterized? This experiment doesn’t have a lot of context and the authors would be wise to cite previous work and give readers a sense of how different or similar these results are relative to previous work. I would also find this simulation much more convincing if the authors had some data to show that the shape of the profile, size-distribution of fragments, etc. is similar to that observed in a surging glacier somewhere. Is there a benchmark glacier that can be used where data is available?

Minutia:

1. The statement that marine terminating glaciers account for all of the mass lost through calving from Antarctica can’t be right since virtually of the mass lost from Antarctica occurs through the ice shelves that fringe the Antarctic ice sheet and calving is about 50% of the mass loss budget from these features. I think the authors mean that the mass is lost by calving from ice shelves.

2. I have quibbles about the statement that “glacier fracture and iceberg calving have been little studied.” The fracture of ice and its connection to iceberg calving has a long history of study in glaciology dating back to early papers by Weertman and Smith [Weertman, 1974; 1980]. Since then there has been a steady dribble of papers that has become a steady flow of papers over the past decade. The fracture of ice has been intensely studied in the lab, although the emphasize has been on compressive failure of ice [Schulson, 2001]. Despite this interest, iceberg calving and fracture of ice remain topics of current interest and this is a testament to the difficulty of the problem, not an absence of attention to it. As one of the commenters noted, damage mechanics provides another venue to study fracture of ice in the continuum limit. While there is no need for a detailed literature review, this previous history needs to be acknowledged.

3. Densely packed. Does this mean closely packed? What happens if the particles are randomly packed instead?

4. p. 928, line 10: Define high accuracy.

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