Response to Referee 2:

Summary: The paper will be rewritten and extended in order better explain the model and results and to meet the requests and suggestions expressed by the referee. Since the referee requests many details about the model to be explained we suggest that we write a detailed Appendix with model descriptions. Below we respond in detail to the points raised by the referee:

Model description:

a) We will in the revised manuscript give a more in-depth description of the model assumptions and numerics. Specific expressions will be given for the elements in the mass and damping matrices. The matrix formulation is just convenient notation. The damping coefficient associated with drag will be given in the text. The damping associated with particles falling into the water can either be the so called “turbulent” or the “Newtonian” drag or a combination. Neither of them, nor the combination, are however very good approximations. The only realistic modeling of water drag would be to fully resolve the multi-phase (liquid and solid) flow. The damping coefficients is a constant only if similar size particle are used. Among the parameters that are important for the model behavior some are known to a high degree of accuracy (like density of water or ice). Others are not, like e.g. the stiffness of ice. Young’s modulus values range from almost 10^{11} for very pure crystalline ice to orders of magnitude lower values the more the ice is damaged, contain voids or cracks, is crushed into grains, etc. The fracture strain also varies significantly for various types of ice. The spatial fluctuations (i.e. disorder) is not known (this is the case for both distribution function of local strength and stiffness variations as well as their spatial correlations). It is therefore rather pointless to model some aspects to a very high degree of accuracy while others, equally or even more important aspects, are not even known within the order of magnitude. On the contrary, it is good scientific practice to use, as much as possible, a consistent degree of accuracy within a model. Therefore we use, for example, 10^3 for the mass density of ice, a 10% mass difference between ice and water to calculate buoyancy, etc. The general principle is that the accuracy of the model parameters are set to the correct order of magnitude. This is also the case for the moment of inertia. The moment of inertia of the particles is an input parameter and it can be changed from simulation to simulation. For the moment we use the simplest possible explicit time integration scheme. Others could be considered in the future, when the model develops. “Do simulation results scale with changing particle size and or time-step size?” What does scaling mean here? Time step is limited in practice by the collision times for particles. The model is scale invariant so particle size is really only a matter of the level of coarse-graining. Smallest fragment size is determined by the particle size, and the model behaves consistently for reasonable particle sizes. Buoyancy forces: see above. All these points will be explained in the revised manuscript.

b) As long as several, i.e. more than one, particles are elastically connected into blocks friction arises naturally through the interacting rough surfaces of the blocks. For single particles, i.e. particles that are elastically detached from their neighbors, transverse forces are transmitted via the viscous interactions. These interactions are to a large degree similar to frictional interaction (viscosity is simply internal frictional interactions on the molecular level of a material). The main difference arises from the lack of static interaction for viscous forces. If stress on a particle is non-zero, waiting long enough will mean that the contact will break. It is, however, unclear to us how both granular friction and viscous interactions could be combined in a consistent way within a single framework on the same length scale. This could in principle be done with a separation of length scale (i.e. viscous interaction on a small and granular friction on a larger scale, but this approach would be a disaster for computational efficiency). In other words there is no stable angle of repose in the model because viscous flow will always deform a heap over long times. This should also be the case for ice. These things will be explained in the model appendix of the revised manuscript.

c) We use both Timoshenko and Euler-Bernoulli beams (demands rotational and translational degrees of freedom for each particle). If, e.g., Young's modulus and Poisson ratio as well as dimensions of the beam are given (input parameters) they define tensile, shear and bending stiffness for the beams. Beams may break under tension, shear and bending with different thresholds (input parameters). We have tried to set the thresholds so that they mimic strength of ice (i.e. rather weak under tension as the referee also suggests). The 30m ice block is weak because both stiffness, i.e. Young's modulus, and tensile strain threshold were set to rather low values (10^8 and 10^{-4}). Increasing the parameters makes considerably larger ice blocks stable. Text will be improved to explain these things.

d) We are not aware of any literature were the viscous model used here is presented. We are, however, in the process of writing such a publication also for a physics journal. The notation is not sloppy. The derivation in the text is very general. In particular it is not limited to any specific type of stress or strain. Obviously the most relevant specific case for ice on a macroscopic level would be the deviatoric stress tensor, its second invariant, and the related strain rate tensor, but the theoretical consideration in the text is far more general than that. The theoretical considerations give an expression for the occurrence probability of the irreversible microscopic deformation event (in the text called the melting/refreezing probability) that reproduce the desired power-law relation between stress and strain rate. This probability expression is sufficient for the numerical implementation. The power-law relation was also tested explicitly by simulations and the result will be added to the revised manuscript to prove that the numerical model indeed reproduce the correct relation between viscosity and stress. It should, however, be kept in mind that this model necessarily display a granular flow, which is not quite Stokes flow. So far we have not paid any attention to the temperature. This is something that can be easily added to the model later on. These things will be explained in the revised manuscript, including a tensor notation derivation to avoid confusions.

e) Slow, quasi-static fracture, is significantly different from fast fracture. Fast cracks are unstable as a result of significant fracture taking place during relevant elastic relaxation times in contrast to slow fracture. That is, for fast fracture inertia is important. This instability is the origin of the typical fast-fracture fragment size distribution observed also here and for calving debris. It is also not so much the elastic waves that limits the time-step, it is rather the high-speed collisions between blocks that ultimately sets the limit for the time-step. The referee is right that it is not correct to speed up the viscous flow so that the time scales of fracture (~ secs) become equal to that of large viscous strains. It is, however, possible, to speed up viscous flow as long as there are no significant viscous deformations of fragments and their immediate surrounding during the fragmentation process. Still, it is possible to speed up viscous flow several orders of magnitude with very minor violations to this rule. As the referee points out this is all governed by the Deborah number. The referee suggests that we should test that our model remains stable across several orders of magnitude for Deborah number. This is actually trivial. As long as the viscosity in the model is high enough for no, or only an insignificant portion of, particles to undergo viscous events the model will behave as if it were a purely elastic model. Initially, we also had the same idea as the referee that we do not need viscous behavior to study calving. The problem seem to be that the stress field that create the typical crevasse patterns near the terminus of glaciers need viscous flow to be modeled properly. Also surging seems to be a very complex mixture of granular flow, fast viscous flow and fracture appearing on overlapping time scales. For this kind of modeling viscous flow is essential and we expect our model to behave really well.

Numerical experiments:

a) The referee is correct that in this 'experiment' there is no fracture, only viscous flow. In the revised manuscript we will show a graph displaying viscosity in the model as function of strain rate. This will replace the somewhat arbitrary statement 'to a high accuracy'. Analytical results for various rheologies are difficult to use for comparison since they are typically results for fluid mechanics which differs slightly from granular flow. It would be unrealistic to expect an exact match between e.g. Stokes flow ice-models and our particle model, therefore a numerical comparison with a well-known ice flow model like Elmer seems more appropriate to us.

b) There is no real symmetry since disorder will breaks it. The boundary is indeed different to the left and to the right. We will clarify this in the revised manuscript. There was also a small narrow crevasse at the top of the block to the left as initial condition. The block is not stable because rather low values for stiffness and failure strain were used in this case (10^8 and 10^{-4}). Using e.g. $5 \cdot 10^9$ and 10^{-3} easily makes a several hundred meter high block stable.

c) Size 's' is cubic meter. We do not have the data ourselves, but it is rather easy to compare the FSD (Fragment Size Distribution) function from the model calculations to corresponding FSDs from the cited paper: Savage et al. "Size distribution of small pieces calved from icebergs" (Notice that this is ice debris calved from ice bergs, not ice berg size distribution). The functional form for the FSD is robust and does not depend on the details of the model. Time lapse cameras from calving glaciers are typically used for determining the 'size' of calving events, which is different from FSD. A single calving event typically creates a large amount of fragments.

d) The question about friction was dealt with above. Viscosity is simply internal friction between the constituents of the material moving past each other. The particles collide and thereby transfer momentum between each other. There is no stable angle of repose, but the material relaxes only over times for which viscous deformations are large, which is a very long time and therefore there appear to be a stable angle of repose. The friction with the bed is a 'no-slip' condition. We are not aware of any useful observation data that could be used for a reasonable comparison with model results.

Minutia:

a) We agree with the referee on this point and will change the text to “marine terminating glaciers and ice shelves”.
b) This is fair enough, it’s true that it’s difficult not uninteresting. We will add proper citations, and the statement
"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"