Overall, I think the authors have done a commendable job of addressing reviewer comments. In addition to a few technical corrections, I still have two questions/comments, but these may reflect my own misunderstanding of the methods used by the authors and I leave it at the discretion of the editors and authors whether these comments need to be addressed prior to publication. I also strongly suggest that the authors provide inputs to their model as supplementary data tables or figures. I think most of the information is included as figures, but I don't think the authors provide the mass balance field they use and this should be included as a figure or as a supplementary table. Personally, I would advocate that the authors include all inputs (basal friction, mass balance, bed geometry, etc) as a data table supplementary information to allow others to more easily reproduce the authors results. My major comments refer to previous comments and are labeled as such.

Response to response to comment 1: I'm still slightly concerned about the distinction between deviatoric and resistive stresses which is explored in the response to my comment from the previous review. I've pondered this and I think I'm missing something here. As I understand it, the authors substitute deviatoric stresses into Van der Veen's resistive stress formulation to calculate stress intensities. This is justified based on the fact that the pressure only deviates by a small amount from the hydrostatic pressure (cryostatic, lithostatic, what ever). The definition of resistive stresses given by the authors in the reply appears to diverge from Van der Veen's. Van der Veen defines $R_{xx} = \sigma_{xx} + \rho g \left(h_s - z\right)$ not $R_{xx} = \sigma_{xx} + \rho g H$, where h_s denotes the surface elevation and h is the ice thickness. I assume this is a typo in the authors reply (?). Nonetheless, my understanding is that the authors argue that deviatoric stress $\sigma'_{xx} = \sigma_{xx} + p$ is (nearly) equivalent to resistive stresses when pressure p is (nearly) hydrostatic, i.e., $p = \rho g(h_s - z)$ and the authors argue that pressure is in fact within a few percent of hydrostatic. However, in the shelfy-stream approximation, the vertical force balance condition requires that $p = \rho g \left(h - z\right) - \left(\sigma'_{xx} + \sigma'_{yy}\right)$. At least in the shallow approximation, the pressure is reduced by horizontal stretching. In the interior of the ice sheet, where the ice is frozen to its bed, longitudinal stretching is small and pressure is approximately hydrostatic. A consequence is that $2\sigma'_{xx} = R_{xx}$ (again in the shallow shelf limit). This argument only holds for the shallow models and doesn't hold for the full Stokes calculation, but I would expect qualitatively similar results in which deviatoric stresses act to reduce the pressure. This leaves me confused as to what the authors did to calculate crevasse depths and why the pressure in their model is (nearly) hydrostatic. I assume the authors did the right thing, but it is hard to decipher what that thing is.

Response to response to comment 2: This is probably reasonable, but again I'm still slightly confused. If crevasses have initial width w_0, then the rate at which they widen initially will be proportional to the extensional strain rate, say $dw/dt = E_{xx} w$, where E_{xx} is a measure of the extensional strain rate opening the crevasse. This results in an exponential widening rate early on in the evolution of the crevasse. This suggests that if crevasses are initially 20 m wide and the width triples to 60 m in time interval Δt, then a 1 cm wide (my calculations suggest more like 4-6 mm) should widen to 3 cm in Δt just based on the kinematics of the flow field. This would suggest that the width of crevasses later on in the simulation may be a function of the initial width of the crevasses and better (or worse) agreement with observations could be obtained merely by adjusting the initial width of the crevasse. Again, I assume the authors have done grid sensitivity experiments, but a statement or two (or a plot) to point out that the width of crevasses throughout the simulation is only weakly dependent on numerical resolution would be useful to fortify this in the audiences mind.

Technical corrections

I still have questions about the role of damping in simulating the stress field (especially as it translates to crevasse depths). The damping term can create an additional stress that acts to open crevasse because the ice shelf is never exactly in hydrostatic equilibrium with the ocean. However, this may be a higher-order numerical question.

Page 7, line 10: The time scale of major calving events from ice shelves is years-to-decades, not days to weeks. The days to weeks time frame is appropriate for grounded Greenland glaciers.