This manuscript describes the use of a finite element model to simulate the penetration of surface crevasses within ice shelves extending previous semi-analytic treatments of crevasse penetration to include vertical variation in ice properties. Overall, this manuscript treats an interesting and timely problem and despite some of my confusion about some of the modeling descriptions (see below) is a valuable contribution to studies of ice shelf fracture.

Specific comments:

Boundary conditions:
1. Viscous versus elastic loading: The authors assert that ***"The stress field at the ice shelf surface is evaluated from the flow velocity in the ice shelf using Glen's flow law (Glen, 1958)."*** This statement is confusing. Normally, one would suppose that the ice shelf surface is traction free and use the condition that the dot product of the stress tensor with the outward normal vector from the surface must vanish. This doesn't involve specifying any velocities nor the use of Glen's flow law. It may be that the authors are using a viscous rheology to determine a velocity along the rightmost side (i.e., calving front edge) of the domain and turning this into a displacement boundary condition? The reason to do this is unclear since it introduces an inconsistency into their model. If the ice is behaving viscously then it is unclear why the elastic as opposed to viscous stresses are important. On the other hand, if elastic stresses are important it is unclear why the viscous flow of the ice should play any role. If this is done purely for numerical convenience the authors should clearly state this. This is all the more important since the viscous flow results from the dissipation of gravitational potential energy and it is far from obvious that viscous displacements due to the dissipation of gravitational potential energy can increase elastic loading? This seems likely to lead to a violation of conservation of energy. I realize that one of the challenges in simulating fracture of glacier ice is that fracturing is usually considered to be an elastic process whereas the flow of glacier ice is predominantly viscous. The authors need to confront this more explicitly and provide a more clearly reasoned explanation and rationalization for their choice of boundary conditions

2. Benchmark: The fact that the numerical model is able to reproduce the quasi-analytic solution of Rist (2002) is encouraging and this may be sufficient to ferret out any errors in the numerical algorithm. However, there are several approximations implicit in the Rist study and it is often best to test numerical solutions again analytic solutions where available so that one can examine the convergence of the numerical solution to the "true" solution. This is often an important step in the numerical approximation of LEFM since there is a square root of r singularity near the tip of cracks that will be poorly represented by conventional finite element shape functions. Because of the fact that numerical methods often fail to converge, numerous authors have introduced special crack-tip shape functions that more accurately incorporate the singularity. I'm fairly confident that the numerical model is sufficiently stated, but if the authors have done grid refinement studies to examine if the singularity is negatively impacting the results it would be useful to state this.

There are a few statements that I think are misleading and should be clarified.
1. Velocity in ice shelves is independent of depth. This assumption is usually justified based on a long wavelength asymptotic expansion that is valid for length scales that are large compared to the ice thickness. Over smaller length scales, flexural bending stresses may be important. This is especially true near the front of an ice shelf, which seems especially relevant here since observations and theory indicate that there should be bending near the calving front of an ice shelf.

2. Domain size: The authors are using glacier length of 2 km for an ice shelf that is uniformly 250 m thick. Given the assumed symmetry, the 1 km length between the fracture the end of the domain is comparable to the flexural wavelength within the ice. This would lead me to wonder if the results may be contaminated by edge effects from the domain boundaries and would be consistent with the large flexure that the authors observe in the constant stress boundary conditions. The authors should test to make sure that there results are independent of the length of their glacier.

3. It looks like all computations were performed for a 250 thick ice shelf. It may be helpful to readers to report crack penetration depths as the ratio of crack penetration depth to ice thickness. I suspect that if the authors can perform the same simulation for a range of ice thicknesses then they will find that the ratio of crevasse penetration depth to ice thickness may be constant for a given set of parameters. If it is not, then this would be useful to report.

Section 2.4:
I had a very hard time following the description of the boundary conditions used. I suspect that the authors treatment is correct and the misunderstanding is on my part, but it might be helpful to other readers if the authors can clarify the following questions.

1. It is not clear to me why the authors want to neglect bending stresses in the formulation. One often neglects these stresses our of convenience, but this does not imply that they are not present.

2. “Sufficiently long rectangular domain” is vague. Can the authors confirm that they have performed domain size tests to determine if their results are independent of the length of the domain?

3. “The stress field at the ice shelf surface is evaluated from the flow velocity in the ice shelf”: It would be helpful if the authors explained how the authors are computing the viscous flow field. For instance, are they solving the full Stokes equations in the interior of the domain subject to traction boundary conditions on three sides?

4. Glen’s flow law depends on temperature and. How does the assumed ice temperature influence the authors results?

4. In a typical ice shelf model, one would assume that the ice surface is traction free. Why is this not the case for the elastic model? Do the authors mean the ice-air-water interface?

Figures:
Figure 1: Difficult to read the numbers on panel (b) and the axis labels in panel (c) appear garbled on my screen.

Figure 4 (b): It would be helpful to have actual numbers on the colorbar so we can see the actual magnitude of the stress.

Figure 6 (a): Axis labels are also garbled (perhaps a conversion issue?)

Figure 8 and 9: More problems with the axis labels -- make sure you check all of these