We thank the reviewer for the valuable comments to our paper in discussion for TC. We considered them carefully in the revised manuscript. In the following please find our answers to the comments.

This manuscript presents an analysis of the instability of crevasses in ice shelves from finite-element calculations. This is an old problem, tackled by several authors in the past, generally from analytical calculations. However, the finite-element method used in this study can be potentially a way to explore more complex situations, such as complex density profiles or situations subjected to brine infiltration. In that sense, the approach proposed here is potentially interesting, the subject is timely and certainly relevant for The Cryosphere. However, in my opinion, several important clarifications and probably complements are needed before publication.

1) To estimate stress intensity factors (K), the authors calculate first stress energy release rates (Gy) using the method of "configurational forces". A detailed comparison with other classical FE methods to estimate K is needed here: - Is this "configurational
" method similar to, or (or not) inspired from the perturbation method (Parks, Int J Frac 1974) ? If not, what is its advantage ? - the calculations are done here with elements with quadratic shape functions. For fracture mechanics problems, special elements dealing with $1/(r^{1/2})$ singularities can be used and are more adapted to correctly estimate K. Did the authors try to use these elements ?

In linear elastic fracture mechanics many different approaches can be used to evaluate stress intensity factors (SIFs). The most modern ones are the computation via XFEM and Configurational Forces. The method of Parks also uses the energy release rate to compute SIFs. However, from the computational point of view, this method is too expensive and inflexible and therefore no longer in use in computational fracture mechanics.
The configurational force approach is rather versatile, as the configurational force at a crack tip can be interpreted as a crack driving force. This interpretation holds for inhomogeneous situations and problems with loaded crack faces. An extension to inelastic fracture is also possible, which is why we prefer and use this method.
We compared the simulation results to semi-analytical results of the SIFs as can be found in standard fracture mechanical literature (Sec. 2.5 and Fig. 1c). As our results with mesh refinement are in very good agreement with the semi-analytical results we did not feel to be in the need to use special crack tip elements. It finally should be mentioned that the K-factor calculation via configurational forces does not require the detailed determination of the singular crack-tip field. This is one advantage of the method.

2) Sections 2.4 to 2.6: In their simulations, the authors prescribe vertically constant displacements, as they argue that at large distance from the grounding line, horizontal velocities and displacements are depth-independent. Although some references would be useful here, this might appear as a reasonable approximation.

We added the reference to the book of Greve and Blatter in Sec. 3.4 to justify the assumption about the depth-independent horizontal velocities made in the manuscript.

However, to apply displacements instead of stresses can lead to some tricky problems. In section 2.4, the authors explain that the boundary displacement Delta_u is linked to the normal stress at the surface (no effect of the overburden pressure) through a linear elastic rheology. But, just below, they indicate that this stress is calculated using a viscous rheology (Glen's law). There is therefore a contradiction here. This contradiction is also present in Rist et al. (JGR 2002), but this was more justified in this case based on analytical calculations.

The application of displacement boundary conditions instead of stress boundary conditions is indeed tricky as this approach requires further consideration on how various material parameters and their variance over depth influence the outcome of the problem. However, not requiring elastic material parameters when applying stress boundary conditions does not imply that their influence does not exist and need to be discussed.

We do not see a contradiction in the transformation of the viscous surface stresses into displacement boundary conditions for our linear elastic analysis. In a visco-elastic material as e.g. represented by a non-linear Maxwell element, there is one and the same stress acting in the spring as well as in the dashpot. Depending on the time scale, the strain in the material will be dominated by the short-term elastic response or the long-term viscous response. We assume that for the fracturing process in a first approach only the elastic response due to a sudden change in the geometry, e.g. due to an initial crack is important. The stress overburden pressure is included in our model by applying gravity as a volume force in the entire computation domain.

In the real world, both rheologies are coupled, and play a different role depending on time scales. Finite-element calculations could be a way to model the full problem. In this case, velocities instead of displacements should be used as BCs. In the simulations of this manuscript, displacement BCs are applied (and do not evolve through time). Imagine an elasto-viscous body on which you apply - instantaneously – some displacement Delta_u. The obtained stress field at t=0 would be the one calculated by the authors, but this stress field will relax as the ice creeps. As the instantaneous application of a fixed displacement is an unrealistic scenario in case of ice shelves, this raises problems for the interpretation of the presented results in terms of crevasse instability. A more realistic scenario would be to apply velocities instead of displacements: in this case, the instability will depend strongly on the rate of loading, i.e. on the possibility to relax by creep the increasing elastic stresses. In addition, for prescribed velocity BCs, the presence of crevasses will modify the obtained surface stresses. These problems explain why previous analytical approaches (e.g. Smith, C93 J.Glac, 1976; Van der Veen, Cold Reg Sci Tech, 1988, . . .) considered stress BCs, even if they introduce other simplifications. In conclusion, I do not see a real break- through here compared to previous analytical works, whereas the FE approach could potentially allow such progress.

Laboratory studies and observations on ice shelves indicate that unstable, sudden and crack growth in ice shelves exists and happens on time scales where relaxation due to creep can be neglected. However, we totally agree with the reviewer, that a time dependent FE simulation of the visco-elastic material to analyse the crack propagation would be more realistic and desirable. Unfortunately, this approach needs even more knowledge and assumptions about the elastic and viscous material parameters as well as the boundary conditions at t=0 for the velocities and the displacements. For this reason we first concentrated only on the elastic properties and their influence on the stress intensity at the crack tip with the intention to expand the model to a visco-elastic one in further studies. Furthermore we would like to remark that analytical models can not consider material inhomogeneities, such as varying Young's modulus. In that respect, we believe that the method proposed here is more versatile and is applied to more realistic settings.

3) p471, L9: "an elastically compressible solid": this precision might be useful to avoid confusion with incompressible (e.g. plastic) flow.

4) p474, L7: "the identity tensor I" (and not 1)

We agree with the reviewer and will therefore adapt the sentence and the symbol.

Yes, in the elastic case, the crack driving force is an energy release rate. This term is commonly used in the literature and is appropriate and descriptive in the context of fracture mechanics.

Of course the refinement strategy is influencing the results. The comparison with the semi-analytical result however indicated that the results are accurate enough. As all meshes influence the results we do not believe that specifying the exact rate of refinement will provide a better understanding. In order to provide some more specific information we mentioned the minimal element size in section 2.5 and will add the average total number of elements (about 8400 elements for crack tips located in the domain centre) in the revised version.

We changed the sentence in the manuscript to: "… based on measurements of ice cores, rho_sw = 1028 kg/m^3 is the density of salt water and B the temperature dependent and therefore depth-dependent rate factor."

Rist et al. (2002) use the weight function method of Bueckner (1970) to calculate stress intensity factors. We used configurational forces and Eqn. (18) to reproduce the results for the validation of the model. We changed the sentence: "The stress intensity factor K_I based on σ_{xx}, is calculated using the weight function method (Bueckner, 1970)." to "Rist et al. (2002) use the weight function method presented in Bueckner (1970) to evaluate the stress intensity factor K_I based on σ_{xx}." .

We regret that we have forgotten the 10^5 here and changed in the manuscript. It should read (1-4) 10^5 Pa m^(1/2) or 100-400 kPa m^(1/2). However we choose to take 400 kPa m^(1/2) instead of 150 kPa m^(1/2) as upper limit, based on the measurements reported by Rist et al. (2002).

The effect of gravity acting on the ice must be transformed into a boundary stress, if stress boundary conditions are applied. By assuming a hydro/cryostatic stress varying with depth, which is added to the depth dependent or constant tensile stress, the mentioned authors tacitly imply an elastically incompressible material behaviour and therefore nu = 0.5. With the discussion about Poisson's ratio we want to sensitize for the fact that for an elastic analysis of fracture in ice one has to discuss the appropriate value for Poisson's ratio and accordingly adapt the boundary conditions. The results for nu=0.5 and nu=0.3 vary by a factor ½. This is not a matter of displacement or stress boundary conditions, but on how the gravity induced pressure is transformed into boundary conditions.

12) p482, L15-16: KI<KIc is a poor crack arrest criterion. In general, as dynamic effects have to be taken into account during unstable propagation (KI>KIc), the arrest is observed for K«KIc: see e.g. the classical (Ravi-Chandar, Int. J. Fracture, 1984). KI>KIc is a good crack initiation criterion for unstable crack growth. It tells nothing about how a crack (a crevasse here) can reach the critical depth (in this manuscript, the creep strain-rate, and so the surface tensile stress, is considered to be constant through time). To describe this, sub-critical crack growth has to be considered. In the context of crevasses, this point has been tackled by (Weiss, J. Glac., 2004).

In this paper only quasistatic crack initiation and growth is considered. In this framework a crack is considered as stable as long $K_I < K_{Ic}$ holds. We agree absolutely with the reviewer that this criterion cannot be applied as an arrest criterion in case of dynamic crack growth. For brittle materials, the (dynamic) arrest value K_{Iarr} usually is below K_{Ic}, but detailed data for ice to our knowledge seems not to exist in the literature, which is also stated in the very interesting paper of Weiss (2004) that the reviewer mentioned. In this context it also should be mentioned that a sound K_{Iarr} value would only make sense in conjunction with a full dynamic crack growth analysis. We added a reference to the paper of Weiss (2004) in the introduction of the revised manuscript: "A different approach was introduced by Weiss (2004) who argued that critical crack growth can not explain slow crevasse propagation. He therefore analysed subcritical crack growth for very simplified geometries, boundary conditions and material parameters. "

13) p484, L21-22: The exponential dependency of E is probably related to a strong dependency of E on porosity in firn (see e.g. Schulson and Duval, 2009). This strong, non-linear dependency of E on r calls for a coupled analysis of the effects of both parameters (i.e. to unite sections 3.3 and 3.4)

We chose to look at E and rho independently and added a sentence about their dependency and the motivation to perform a coupled analysis in the revised manuscript. In order to separate effects and mechanisms we prefer to keep the parameter studies in separate sections: "Rist et al. (2002) motivate a density related and therefore exponential dependency of the Young's modulus on the depth, which gives reason for a coupled analysis of the influence of both parameters. However, in order to compare the simulation results with former analyses applying depth dependent density profiles and to separate effects and mechanisms, we decided to look at E and rho independently."

14) the legends of fig. 1(c) are unreadable

We solved the conversion problems.
We are grateful for the very constructive and helpful comments of the reviewer that helped us to improve the manuscript.