Dear Dr. MacAyeal, thank you for your 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.

First comment:
I am struck by how useful this paper is. The computation of stress intensity factors is very difficult, and involves a lot of magic and approximation... and this paper presents a fully functional alternative to one of two prior approaches I have seen in my brief period of study (I am no expert, but am trying to learn): one method has been in glaciology for a while–the use of what are essentially tables of evaluations to account for geometry of a body with an idealized notch-shaped crack in it... the other method is the J-integral method, which is relatively new to glaciology (at least in my awareness) in the sense that it was used by Tsai and Rice (unpublished) to evaluate the hydrofracture of a crack at the base of an ice sheet for the purposes of studying supraglacial lake drainage in Greenland.

This paper presents an alternative to the above two methods, i.e., it computes a field called G within the body of the object, and uses this field to evaluate stress intensity. It is an alternative to evaluating the J-integral, and (I am guessing) is probably more consistent with the numerics of a finite-element model (where it is difficult to evaluate derived quantities associated with model variables on a point by point basis). The way I see it, the method uses the Eshelby stress tensor (page 474) as part of the field called G... This stress tensor (which otherwise means nothing to me as a glaciologist, having never seen or heard of it before) forms the integrand of the J-integral; so I suspect that the methodology here is related to efforts to evaluate stress intensity factors using the J-integral.
Overall, it seems to me that a numerical solution of a problem that has as its objective to find stress intensity factors should do well to use the method described here; and I wouldn't be surprised if the method here is superior to determining the same result by evaluating J-integrals using numerical data (that is possibly inconsistent with the finite-element nodal fields)...

Second comment:
In looking further into the subject, I read the famous paper by Rice (1968) where he derives the J-integral. At the end of his introduction he states:
"The J integral is identical in form to a static component of the "energy-momentum tensor" introduced by Eshelby to characterize generalized forces on dislocations and point defects in elastic fields."
This confirms the fact that the methodology used in the paper under discussion is closely tied to the alternative methodology for evaluation of stress intensity factors. The paper under discussion goes so far as to compute the Eshelby tensor everywhere (not just along a path integral, as is done in the J-integral).
D.R.M.

The method of evaluating stress intensity factors via configurational forces is indeed very useful and handy, especially when using finite element simulations as most of the calculations required are already done for the solution of the boundary value problem. In fact, the configurational force method can be regarded as a very general numerical realization of calculating the Eshelby stress and force, respectively, for any field and defect, i.e. of course also for a crack. As you mentioned, one advantage is, among others, that a contour integration is not necessary. In addition, the method yields a configurational force vector at the crack tip that may give an indication on the direction of crack growth. The absolute value of this configurational force vector at the crack tip represents the same physical value as the J-integral, considering the limitations of the J-integral due to loaded crack faces, volume forces and material inhomogeneities.

First comment:
One thing that I have worried about, but without progress (and this is *not* a criticism of the discussion paper) is to what extent is the realization of ice as an elastic body (e.g., with a Poisson ratio that is not 0.5, and where the pressure will not be lithostatic as a result) is different from its realization as a viscous (or "Glennian") body? The elastic stresses will be very different from the viscous stresses (but there can be only one stress field, right?) depending on the assumptions made... Also, elastic stresses are not temperature dependent, whereas viscous stresses depend on the temperature profile of the ice body (as analyzed in, e.g., one of the papers cited by the referee). This is something I would like to know at a deeper level (and regret that my expertise and education are not to the level that would allow me to know or understand the answer).

Third comment:
The referee raises a point that I have often wondered about (largely because my training in continuum mechanics is limited to fluid flows, with relatively little experience in elastic and other material constitutive relations): How does one reconcile the fact that two different stress fields would be computed for one application of external boundary stresses depending on whether the viscous (or "Glennian") or the elastic constitutive relation is used?

A subsidiary question relates to the fact that one of the papers cited in the reviews looks at how temperature variation through the ice column in an ice body might concentrate the stress at some depth and thus influence fracture propagation. This is only possible for a viscous rheology where the viscosity thickens with cooling temperature;
Elastic parameters (as far as I know) are not strongly temperature dependent (for linear elasticity).
Here's how I reconcile the fact that two different stress fields are computed depending on whether you assume elastic rheology on one hand or viscous rheology on the other:
Both are right. The elastic stress regime is what is correct immediately after the application of boundary conditions on the ice boundary. The viscous regime is what is correct after a long time period has passed so that differential viscous relaxation has relieved the elastic stress where the viscosity is low and has concentrated the stress where the viscous relaxation has not relieved the elastic stress.
I provide a figure to illustrate this point.
Finally, I wonder now whether glaciologists should consider two classes of fracture formation: those which "creep" open when the stress field is dictated by viscous rheology, and those which suddenly open when the stress changes and the elasticity of the ice immediately dictates the stress field.
Thanks for letting me struggle with the concepts of this paper.

The realization of ice as a purely elastic material to simulate the flow behaviour is as wrong as a purely viscous rheology to simulate fracture processes. The real behaviour is time dependent, visco-elastic and lies somewhere inbetween the incompressible viscous and the elastic behaviour with transverse deformation. Therefore the stresses in a "Glennian" body, as well as the linear elastic representation, should be understood as limiting values of an optimum visco-elastic model. The picture you included in your third comment nicely describes the assumptions we made for our linear elastic analysis: on the long time scale, the response due to creep is the significant one, for a short time fracture event, the elastic response is important. The temperature dependence is included so far only in the densification model that yields the applied density profiles. We do not consider temperature dependent elastic constants, as only little information about the temperature dependence in ice is available.

First comment:
I looked at the review, and think that it offers nice constructive criticism, however I doubt that it will be possible to fully answer part 1... there are numerous methodologies in use and each has its strengths and weaknesses...
On page 479, is there a "Mega" missing from the units for stress intensity in this sentence? The diagram shows, that the critical stress intensity factor KIc, which ranges between (1–4) [check these units] (Rist et al., 2002),
D.R.M.

We agree to your statement and we therefore limited the validation of the numerical methods to the comparison with quasi-analytical results for stress boundary conditions as can be found in standard literature about fracture mechanics.
We are sorry about the missing "Mega" on page 479. It will be corrected in the revised version of the manuscript.

We thank you for your encouraging comments and the remarks, which helped us to improve the quality of the manuscript.