Response to Doug Benn comments

J. Krug, J. Weiss, O. Gagliardini, G. Durand

September 1, 2014

Correspondence to : jean.krug@ujf-grenoble.fr

We would like to thank Doug Benn for comments and advice, who greatly improved the description and the hierarchy of calving processes presented in the literature, and for highlighting the necessity for further inter-comparison benchmark for calving models. We tried to incorporate most of these comments within the revised version of the manuscript. Changes have been highlighted in red.

First, in the introductory section referencing of earlier work is very careless, and suggests a very casual approach to the literature. Proper acknowledgement needs to be made of how the present model builds on previous work, especially the crevasse depth model developed by Benn (2007a, b) and Nick (2010). Indeed, the present model is not so much a "new framework", as a refinement of the existing crevasse-depth model framework, using more sophisticated treatments of damage and fracture.

Regarding this first comment, we do not meant to suggest that our work success better than the classical approaches in simulating iceberg calving. Benn's criterion performs well, and several experiments prove the multiple interests of this criterion, which have been largely explained in various papers, and this is the reason for which this criterion is the most physically-based used criterion.

Following the work of Benn et al. (2007a,b), we actually stated that the calving event occurs as soon as the tip of the crevasse penetrates below the water line (even if the condition is not only on crevasse depth, but also on the criterion $K_I > K_{Ia}$). But this argument is the only point in common between Benn's criterion and ours. The use of damage mechanics to define the initial crevasse depth and the linear elastic fracture mechanics describing crevasse propagation does not appear neither in the formulation of Nye (1957) nor Benn et al. (2007a,b), and as such represent a new approach in calving modelling. Thus, we believe that our framework is not just a higher refinement of the crevasse-depth framework.

However, the manuscript has been modified accordingly following the general suggestion regarding the literature.

Second, it is arguable whether the new model has been "validated" by the Helheim model exercise, as against "tuned". As the authors are no doubt aware, tuning of model parameters to fit output to observations does not mean that the model correctly represents reality. I believe that the current paper represents a significant conceptual advance in how calving processes might be represented in continuum models, but it has not been demonstrated that the model will necessarily perform better than simpler formulations. Formal model inter-comparisons will be required to test this.

The used of word "validated" is indeed a misleading from our part. Obviously, "calibrated" describes better the level of development of the presented calving law. We prefer this word to "tuned", which holds a negative connotation.

Additionally, we totally agree with Doug Benn's remark, regarding the fact that a model inter-comparison should be undertaken, to evaluate the different behaviours of the currently-used criterion. Creating such a benchmark would probably lead to more clarity in discretizing calving parameterizations, but it is out of the scope of the present study. The manuscript has been modified to include these comments.

Third, as regards modelling calving at Helheim glacier, the omission of basal crevasses in the new model is a major shortcoming. Observations by Murray et al. (2013) demonstrate that surface crevassing does not contribute significantly to large calving events at Helheim, but basal crevasses do. The model used by Nick (2013) to model calving losses at Helheim includes basal crevassing, and is more likely to capture the actual processes of mass loss than a model based on surface crevasse propagation alone. This means that in its present form the damage/LEFM almost certainly misrepresents calving at Helheim Glacier.

What Doug Benn is pointing out here clearly highlight one of the current limitation of our model. As mentioned in our paper, we did not include the basal crevasses, and we are perfectly aware that this omission could biased the amplitude of the resulting calving events.

However, we are currently working on the modelling of the effect of basal crevasses on iceberg calving, and this suggestion also pointed out by referee #2 has been included in a section regarding potential improvements of the model.

Some detailed comments:

Before answering the specific comments, we just want to remark that the pages and lines numbers refer to the old version of the manuscript, which was closed on February, 25^{th}, before review, following authors request. However, most of the reader's comments are still valid for the new version. Otherwise is specified in the answer.

p. 1111: modify wording in the abstract on line 2 ("new calving modeling framework" to "new development of crevasse-depth models") and line 8 ("validated" to "tuned").

As stated above, we do not believe that our model represent a simple development of crevasse-depth model. However, we changed the manuscript according to pertinent remarks regarding the literature.

p. 1113, line 6. The referencing here is very inaccurate. The wording appears to imply that Nye proposed a calving criterion, not just a simple formula for the penetration of crevasses. Instead, it should be stated that the crevasse depth calving criterion was proposed by Benn (2007a, b), and implemented in a higher-order flow model by Nick et al. (2010). Mottram and Benn (2009) did not ?use? the calving criterion, but compared predictions of the Nye and LEFM models with field data. Their study found that the Nye model performed almost as well as the LEFM approach, and had the advantage that crevasse spacing did not need to be known. The choice of the Nye formula in the model of Nick (2010) was therefore based on a rational argument backed up with field data. At present, there is no means of telling if the new approach is better or not.

It is true that our referencing was inaccurate. Changes have been proposed in the new version of the manuscript. Additionally, we did not mean to suggest that our approach was better or not than Nick's one, and we apologize if the understanding was biased.

p. 1113, lines 12-15. It is true that the Nye crevasse depth formula does not incorporate stress concentration effects, but as pointed out by Benn (2007b), stress concentration effects are relatively small in fields of closely-spaced crevasses. The statement that the Benn-Nick model does not account for "the crevasse depth" is wrong, as calculation of crevasse depths is at the core of the model. The statement about the "ice discharge" is also incorrect. The approximations used in the Benn model may result in some error in the position of the margin, but the predicted discharge mostly reflects basal sliding functions, which are not at issue here. In addition, as pointed out in Borstad's review, it is by no means clear that the concept of a lone crevasse running ahead of all others is an accurate representation of reality. So it remains to be demonstrated that the new approach is superior in practice to models incorporating the Nye function.

The sentence about the "crevasse depth" was an unfortunate shortcut. We meant that the Benn-Nick model supposes that the crevasse depth is given by the stress field alone. In the LEFM theory, the stress intensity factor, which describes the ability of a crevasse to propagates and thus determine the final crevasse depth depends on both the stress field and the initial crevasse depth. The paragraph was clarified by highlighting the instantaneous approach on which Benn-Nick criterion relies, compared to the combined CDM-LEFM approach.

Wording modification regarding the "ice discharge" now focuses about calving time and amplitude.

Intercomparison would be an essential tool to highlight the potential differences between calving criteria, and to understand them. In such an absence of comparison, it is clear that no one can claim that a model performs better than another one.

P. 1113, line 19: the Åström model can simulate both viscous and brittle behaviour, so it is wrong to say that its "non-continuous approach" is a limitation. Its main limitation at present is that it is very computationally demanding, but in concept it is actually better for all types of glacier modeling than continuum models.

The idea was to say that theoretically, discrete element models are especially designed to represent heterogeneous media, which is not the case when dealing with ice. However, it is right that Jan Åström's model performs well in representing the ice fracturing and the calving size distribution.

Consequently, we modified the introduction section to precise that the main limitation is the computational cost, and to mention that coupling is engaged between discrete element models and finite element models.

At present, the paper is rather dismissive of previous approaches, but in fact it borrows heavily from earlier work and it should be clearly acknowledged that the authors' model is closely similar in concept and structure to the existing crevasse-depth calving model. The new paper develops the concept to incorporate more detailed formulations of damage and fracture propagation. This is a significant development of the concept, although it remains to be seen whether the modifications result in improved model performance.

Once again, we did not mean to be dismissive of previous approaches, and we apologize if the reader has encounter this feeling. We have modified the referencing, and we hope that the improved version of the manuscript is now free from misleadings and inaccuracies.

p. 1113, line 20: "apparition" in English usually means the appearance of a ghost. "development" would be a better word.

Done.

p. 1113, line 26: Van der Veen's papers did not apply LEFM to calving, but to analyze the penetration of surface and bottom crevasses. Also change "employed to described" to "employed to describe"

Done.

p. 1114, line 3: The IGS is a "large glaciological body". Better to say "large ice masses"

Done.

p. 1121, line 18: "pretty well" is a very vague! And of course, the Nye model performed almost as well.

Done.

p. 1123, line 27. "we do consider" should be "we do not consider". As noted above, this is a major shortcoming when it comes to modeling Helheim glacier.

This sentence was changed since the second version of the manuscript was submitted.

p. 1124, lines 4-5: this is inaccurate. For a fixed small (several m) water depth, there will be a corresponding finite crevasse depth. Full depth crevasse penetration requires continuous input of water to keep the crevasse nearly full.

Yes, this is what we meant, but the formulation was inaccurate. However, maintaining water level at the same height requires either a free connection with the pro-glacial water body, or a continuous input of water (melt water for example) into the crevasse. This latter case requires the knowledge of the rate at which water flows into the crevasse, which should be also evaluated before been incorporated in the model.

A paragraph regarding this question and possible improvements was added to the revised version of the manuscript.

P. 1125, line 7: It is now possible to acknowledge the source of the re-meshing scheme - which was vital to the simulations - by referencing Todd and Christoffersen (2014). This should be done as the brief statement in the acknowledgements does not communicate to the reader the importance of this input.

The reference was added into the revised version of the manuscript.

p. 1125, line 22: "validate" should be "test" or "evaluate".

We changed it to "calibrate".

p. 1131, line 6: the glacier studied by Mottram and Benn is in fact in Iceland, not Svalbard, and calves into a lagoon, not the sea. Did the authors actually consult this paper? But in any case, the crevasse depths observed at their site, where strain rates are quite low, do not provide useful validation of the modeled thickness of damaged ice on Helheim.

We apologize for the mistake in the manuscript, which has been modified according to Doug Benn's comments.

However, we do not understand why this measurement of crevasse depth would not support our claim. Crevasse depth measurement in the vicinity of glaciers front are difficult to obtain, and the paper we rely on presents measurements of crevasse depth near to the front.

As our model furnish a potential accumulated depth of crevasse field, which is slowly advected with the ice flow and deepens under the effect of the stress field, it does not contradict the statement of low strain rates in this area.

p. 1131, line 27: "At last" should be "Finally"

Done.

p. 1132, last paragraph: the arguments here are rather weak. Given that: a) the simulations on which this statement are based are an arbitrarily chosen subset of an ensemble that exhibits a huge range in behaviour, b) the simulations do not replicate the observed oscillations of the front, and c) a major process (basal crevassing) is missing, there are really no grounds for making any claim about the causes of the observed glacier behaviour.

We do not question the fact that several processes are not represented in the model, and that our simulation does not replicate the observed behaviour of Helheim Glacier between 2001 and 2005 (which was not the aim of the study anyway).

However, the simulation used to sustain this statement all belong to those which successfully overcome our sanity check (*i.e.* for which the front remains between the observed extent of ten kilometers), and thus are not arbitrarily chosen. Moreover, we stated that the cycles of advance and retreats are not related to any variability in external forcing, which is true, as we are applying the same constant forcing for all these simulations.

This is why we stated that, according to our modelling, a specific dynamics of the front can emerge from glacier internal dynamics and geometry, and does not necessary requires external forcing. Further information were provided in the answer to Referee #2.

p. 1133, lines 19 and 22: "reliable" should be "reasonable" in both cases.

Done.

Additional references:

Benn, D.I., Hulton, N.R.J. and Mottram, R.H. 2007a. ?Calving laws?, ?sliding laws? and the stability of tidewater glaciers. Annals of Glaciology 46, 123-130.

Murray, T., Rutt, I.C., O?Farrell, T., Edwards, S., Selmes, N., Martin, I., James, T., Aspey, R., Bevan, S., Loskot, P. and Bauge, T. 2013. High-resolution monitoring of glacier dynamics during calving events at Helheim Glacier, southeast Greenland. AGU Fall Meeting abstract, C21A-0614.

Todd, J. and Christoffersen, P. 2014 Are seasonal calving dynamics forced by buttressing from ice mélange or undercutting by melting? Outcomes from Full Stokes simulations of Store Gletscher, West Greenland. The Cryosphere Discuss.

References

Benn, D. I., Hulton, N. R., and Mottram, R. H.: 'Calving laws','sliding laws' and the stability of tidewater glaciers, Annals of glaciology, 46, 123–130, 2007a.

Benn, D. I., Warren, C. R., and Mottram, R. H.: Calving processes and the dynamics of calving glaciers, Earth-Science Reviews, 82, 143–179, 2007b.

Nye, J.: The distribution of stress and velocity in glaciers and ice-sheets, Proceedings of the Royal Society of London. Series A. Mathematical and Physical Sciences, 239, 113–133, 1957.