Detailed response to the editor

For the manuscript "*Kinematic first-order calving law implies potential for abrupt ice-shelf retreat*" by Levermann et al.

I.M. Howat (Editor)

This paper has now been reviewed by two experts who agree that while this paper is a valuable contribution, some revisions are required prior to prior to publication in TC. I encourage the authors to consider and respond to each of the referee's concerns (as a replies to their discussion threads) and submit a suitably revised paper that incorporates those changes. Note that the revised ms will likely be re-reviewed by at least one referee.

Response (incl. general response to both reviewers):

We are happy that both reviewers deem our manuscript suitable for publication in *The Cryosphere*and would like to thank the reviewers for their constructive comments which greatly helped to improve our manuscript. We feel that there was one general misunderstanding of the scope of our work which we believe to have detected in the requests of both reviewers. This was obviously caused by a lack of clarity in our earlier manuscript. We have tried to make this clearer in the new version in a paragraph in the introduction which now reads as follows:

"Here we take a simplifying approach and seek the first-order kinematic contribution to iceberg calving ignoring higher order effects as well as potential interactions of material properties with the kinematic field. We thus do not seek to understand the initiation, propagation and alteration of material defects of the ice such as crevasses or other fractures. While such properties are important for calving they will be comprised here in a proportionality factor which we seek to determine elsewhere,for example, by use of a fracture field as recently introduced (Albrecht &Levermann, 2012). These material properties and their alterations with the flow field are beyond the focus of this study. Instead we seek to describe the effect that the large-scale ice flow will have on the calving of an ice shelf with homogeneous material properties represented by this proportionality factor which is taken to be constant in this study. We further do not seek a comprehensive kinematic calving law which will most likely be of complicated nature; instead we determine the first-order contribution that is consistent with universal characteristics found in any ice shelf. Herein we follow a procedure commonly applied in theoretical elementary particle physics using an expansion of the unknown comprehensive calving law with respect to the eigenvalues of the large-scale strain rate tensor (e.g. Peskin& Schroeder, 1995). General arguments often referred to as symmetry considerations then lead to the proposed parameterisation."

In order to meet the reviewers´ very constructive criticism we have added two additional figures with an analysis of the Rignot et al. velocity data. We also discuss the relation of our approach to the SSA-based balance considerations by Admundson&Truffer which we believe to be complementary but not contradictory to our results. This comparison is indeed very fruitful and we have added an additional figure with the comparison to their results. Furthermore we have added the variability of the calving front position in all figures as requested by the reviewers.

In summary, we are confident that we were able to meet the reviewers´ requests and would be happy if the paper would be considered ready for publication.

J. Bassis (Referee)

General Comments:

This manuscript proposes a two-dimensional generalization to a calving law recentlyproposed by Alley and others (2007). The authors further argue that the one of theimplications of the calving law is that some ice shelves, such as the Larsen B and Ross
ice shelves, have the potential for rapid collapse. Thus the calving law can potentiallyexplain the observed rapid retreat of the Larsen B and predicts the Ross Ice Shelf could
be subject to a similar catastrophic collapse scenario.In general the manuscript presents an interesting and novel idea that merits publication.

It would be nice to see the manuscript expanded to provide a more robust comparisonbetween observations and model predictions. For example, why not also include data from the Ross Ice Shelf and other ice shelves in Figure 1b. (I believe velocities for all of the Antarctic ice shelves are now available courtesy of E. Rignot so this should be possible.) If more data is included is there a single constant that can fit all of the ice shelves in Antarctica and how much scatter is there in this type of plot? I also have some questions about the calving model, described in more detail below. Other than these, relatively minor concerns this manuscript will be of interest to the wider glaciological community.

Response

We would like to **thank the referee** for his time to review our manuscript and are glad that he considers it interesting and worth to be published in TC. These were very useful comments, especially the reference to data from EricRignotand colleagues. We have **added two more figures** based on this data for five ice shelves. As we explain above in the general answer, it is not to be expected that the proportionality factor is indeed the same for all ice shelves since it comprises the material properties of the ice. We had not made this clear enough in the first submission. In fact it will not be homogeneous even within one ice shelf. In the meantime we have proposed a fracture field for ice shelves which we seek to associate with this proportionality factor. Having said that, we actually find some increase of the ice velocity near the calving front (which we use as a proxy for the calving rate) with the determinant of the spreading rate tensor, consistent with equation (1) in all the five shelves considered. The slopes of the scatter plots are similar (5-17 10^7ma) for the relatively broad ice shelves Larsen C, Ronne and Ross and significantly smaller (5-6 10^6 ma) for the more narrow ice shelves Amery and Filchner. This is consistent with a higher degree of ice defects in a more strained narrow topography, but these aspects of material properties need to be investigated in future studies, which we are currently working at. We would kindly ask the reviewers to allow us to defer these investigations to future studies.

Even though this is not crucial for the acceptance of the paper and we do not wish to annoy the referee in any way, we need to disagree on two statements made in his general comments: First, we have made it explicit in the manuscript that we do not suggest that the rapid retreat of Larsen B was caused by calving as represented by equation (1). Instead we specifically say that this was most likely not the case. The statement we are making is that the different metastable positions of the calving front that were observed are consistent with the stable states that we obtain under the proposed eigencalving. We thus do not provide an explanation for the Larsen B retreat, but consider the observed metastable positions of Larsen B evidence for the viability of our approach. Second, we disagree that eigencalving is a "two-dimensional generalization to a calving law recently proposed by Alley et al.". As we make specifically clear in the manuscript the calving law proposed by Alley et al would be unstable as Richard himself pointed out in his Science brevia, especially for less confined ice shelves. We discuss here that while a formal expansion of the kinematic contribution to a calving law would imply the Alley-law, it is the second-order components of the decomposition (as proposed here) that needs to be the leading term. For the sake of an open-review process, we would like to mention this here, but we believe that these are merely semantics and there is no need for a deep discussion on these two points here.

Specific Comments:

1. There are a couple of confusing factors associated with the calving model (Equation 2) proposed in the text. First, I'm not sure I understand the logic behind the proposed expansion. The expansion appears to rest on the assumption that the principle strain rates are small so that higher order products in the expansion can be neglected. Perhaps I've missed something here. However, I would expect that fracturing and hence calving is most vigorous at large strain rates. The authors should explain this puzzling conundrum. Alternatively, Equation (2) should be motivated as the natural two-dimensionalisotropic generalization of the empirical calving law proposed by Alley and others (2007)?

Response

As mentioned above we do not consider equation (1) a two-dimensional generalization of the Alley-et-al-calving law not least because it has qualitatively different stability properties. There are two aspects to this comment. First we need to emphasize as in the general answer above that we do not seek to explain the small scale processes which lead to fracturing, crevasses, etc. but rather ask the question what is the dependence of calving (of homogeneously fractured ice) on the large-scale velocity field. Consequently, the strain rate does not have to be large for calving to occur. However, as the reviewer points out, it can be large at some points. Mathematically one would have to introduce a (spreading-rate) scale for the entire ice shelf by which eps- and eps+ have to be divided in order to make them small quantities. In this case it is, of course, possible that the series of equation (2) does not converge. We must confess that we have neglected this possibility here. It is a very valid point.However, in order not to confuse the reader we would prefer to keep this possibility out of the paper. As we now explain in the introduction we follow a rather common procedure of theoretical elementary particle physics by which we expand an unknown law with respect to a quantity for which we have physical reason to believe that it is relevant for the law and then ask which terms in the expansion are consistent with the large-scale behavior observed. We cannot rule out that higher-order components are relevant and depending on the prefactor they might even be dominating in some cases. **We have tried to make this more clear in the text now**.

2. Second, it is unclear where Equation (2) applies. I suspect that the authors intend to apply it solely at the calving front as a type of boundary condition. However, if this is the case it is unclear physically why it is only the strain rate near the calving front that determines calving. This is especially puzzling since observations indicate that the large tabular calving events observed from ice shelves tend to initiate from rifts that develop far upstream of the calving front. Hence, I believe that the authors need to addthe additional assumption that calving is solely controlled by processes that are local to the calving front?

Response

We hope that this issue has become clearer by the answers above. The role of material properties was not properly discussed in the previous manuscript but we have tried to **correct** this in the new version.

3. Because Equation (2) only applies at the calving front, how dependent are the results on the model resolution?

Response

It is possible that K needs to be scaled. We will investigate this in **future studies** where K will be quantified.

4. The implication of the chosen form of the calving law is that ice shelves are only stable if they exist within confined embayments. While this is true, there are also numerous unconfined floating ice tongues that appear to be stable. How can these features be reconciled with the calving law proposed?

Response

These can be reconciled by use of a low K. In fact this might be a way to estimate K form observations. It is **not inconsistent** with equation (1).

5. Given the fact that we know that the sporadic detachment of large tabular bergs occasionally perturb the ice front position. Does the model predict that the calving front of ice shelves that are known to be stable are indeed stable in the face of these perturbations? Also, would the authors care to speculate on how they would introduce rifts prognostically into their simulation so that disintegration of ice shelves could be predicted?

Response

This is a VERY interesting question and we would like to collaborate with the reviewer on this question which tackles the question of the strength of the perturbation to which the ice front is stable. This will be different for different ice shelves and also depend on the distance of the "basin of attraction" of the different ice positions that are possible for one ice shelf. **We would prefer to keep it out of this paper** if possible, because it would expand the paper significantly. We have however **added the variability of the ice front** position in each of the figure which shows that for the chosen K value the variability is small compared to the distance between stable ice fronts, but that is strongly K dependent and needs to be discussed when quantifying K more precisely in future studies.

Technical Comments:

Section 2, after line 10: I'm not sure that dilation, i.e., sum of the components on the diagonal of the strain tensor is the appropriate term. I think the authors actually mean rate of dilation since they are dealing with the strain rate tensor. However, many glaciologists may also argue that the dilation rate is close to zero because ice is to a good approximation incompressible. In two dimensions, of course, this implies that the dynamic thinning rate is related to the 2 horizontal principle strain rates. Perhaps dynamic thinning rate is a more appropriate term?

Response

This is a good warning. We do not want to cause confusion and have **exchanged the word** "dilatation" with "expansion". Since it is defined at one of its first appearances in the text, we hope that it is more clear this way.

page 2703: Is Hughes (2002) the correct reference to cite for tabular bergs from ice shelves? I thought this paper was concerned with slab calving events from partially grounded tidewater glaciers.

Response

We are grateful to the reviewer for detecting this. The correct reference we meant to cite is Benn et al 2007,we have **corrected** this in the new manuscript.

Fig 6. (and a few other figures) show the present calving front position of the Ross Ice Shelf and compare it to the simulation prediction. However, the calving front position of the Ross Ice Shelf is continuously evolving as it advances and occasionally bits break off. It may be more useful to show the mean position or provide some measure of thevariation in the calving front position to let readers better assess how well the model agrees with the observations.

Response
Also this comment was very useful. Thank you. We have **added** the variability of the calving front as it arises from our simulations **to all the figures** as a superposition of gray lines of consecutive time intervals.

J. Amundson (Referee)
…with a bit of work I think this paper would be suitable for publication and would be of interest to a wide range of researchers. I would also like to thank the authors for submitting a well-polished manuscript – it made my job as reviewer a lot easier!

(1) The parameterization is hardly a law. This implies a degree of certainty/confidence that I don't see. For example, the proportionality constant is hardly a constant – it appears to vary by a factor of two between the two ice shelves that were considered in this study. I think its more fair to state that the authors were testing whether a calving law should/could depend on the determinant of the strain rate.
(2) Given the fact that K is not really a constant, how should we interpret it?

Response (to both requests)
We fully understand the concerns that the reviewer raises here but we believe that the reason for the concern is a misunderstanding that was caused by our insufficient explanation of the nature of the calving law. We hope that we have addressed this issuein our general comment above and the **additional explanation in the introduction**. Indeed the proportionality factor K is not a constant and we have now published a paper (Albrecht et al. 2012) which begins to specifically address the nature of this prefactor building on a number of earlier papers by other authors addressing the material properties and the small-scale kinematic physics that determine the prefactor K. We had not made this clear in the earlier manuscript. Having said that we would like to convince the reviewer, that equation (1) indeed has the nature of a law, in the sense that it provides the first-order contribution, that an ice-flow-field has,on the calving rate of a <u>homogenously fractured</u> ice shelf. We will address the matter of the other constants K1+ in the response to the next question. We understand that this point might be a matter of discussion, but we would like this discussion to take place within the community and would be grateful if we would be allowed to publish the associated reasoning as proposed here.

(3) I don't agree with the statement on page 2703 that K1+ has to vanish otherwise ice shelves would be unstable / not exist. That would be the case if the calving parameterization determined the terminus position based on the strain rates exceeding some threshold. If K1+ is not zero, then all that the parameterization is saying is that the calving rate would speed up as the shelf retreats toward the grounding line. Isn't that more or less what we observe for retreating shelves? Also, K1+ is likely not a constant (see point 1) and the strain rates near the grounding line will clearly not remain constant with time. And furthermore, what is meant by stable? Are ice shelves ever truly stable?

Maybe this discussion just means that its difficult to grow an ice shelf outward from a grounding line (as opposed to forming one by thinning grounded ice).

Response

The statement that K1+ must vanish does not mean that there is no large-scale dependence on the largest eigenvalue of the spreading rate tensor. Indeed we propose that there is a first-order contribution to it but it is captured in the term associated with K2+- as in equation (1).

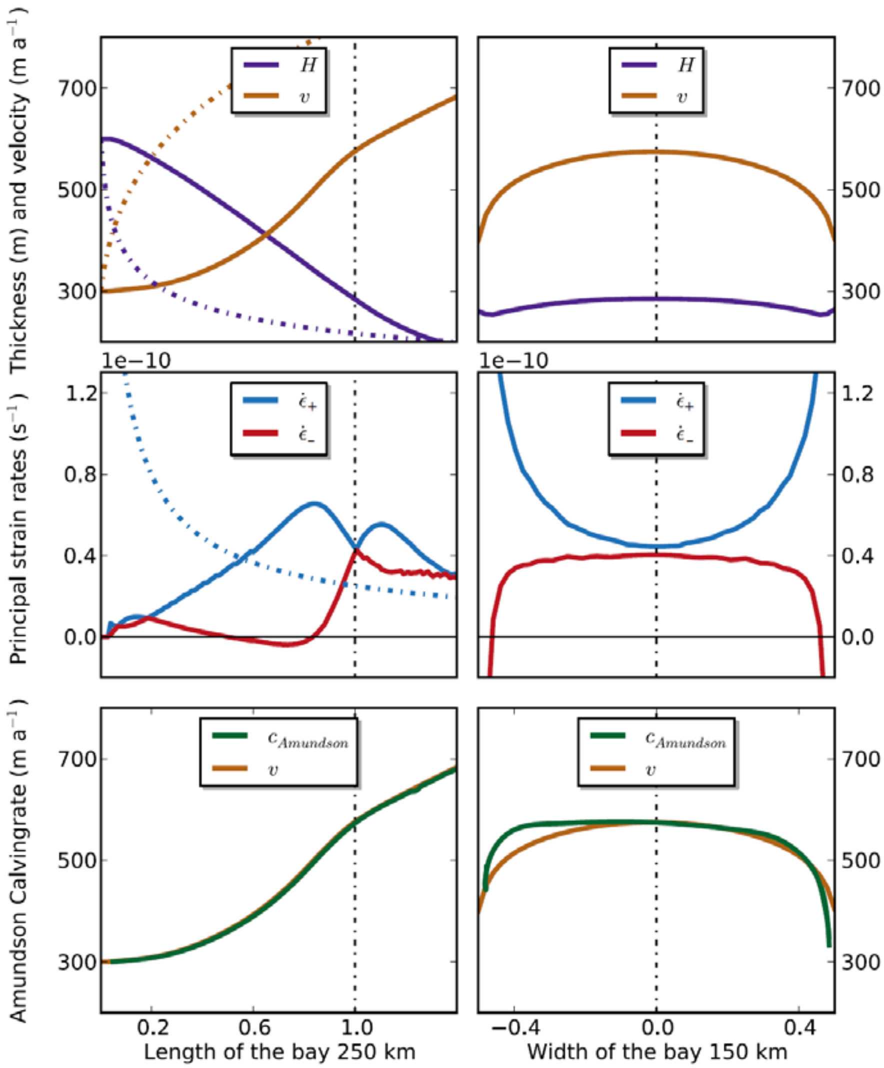

***Figure R1**: Ice shelf confined in a narrow basin of 150km width and 250km length. **Left column:** properties for the center-line along the main flow direction. **Right column**: properties for a cross-section at the mouth of the embayment. Shown are ice thickness and speed (H and v, top) along with the strain rate components (middle). Analytic solutions for an unconfined ice shelf are provided as dashed curves. The lower panels compare the steady-state estimate of the calving rate as computed from Amundsen &Truffer with the ice speed. While the two compare very well along the center-line, the estimate deviates slightly near the margin.*

Thus there is no contradiction of our approach to the observations that the reviewer refers to. On the other hand, if there was a contribution that was only dependent on eps+ and not on eps- then no (even meta-stable) ice front could be sustained. Thus our argument is not in contradiction to the fact that some ice shelves might be sustained under an K1+ contribution. What is required is that all existing ice shelves would be stable under a K1+ term and this is not the case. We have illustrated the instability for a broad ice shelf as represented by the unconfined analytic solution in figure R1 above (dashed lines). While we would prefer to keep it out of this paper, in order to keep the paper focused, we can state here that we have applied the Amundson&Truffer formula as a time-dependent calving law and were not able to obtain stable calving fronts. As we state above, we are however convinced that the complementary use of the two equations will shed some light on the position of stable ice fronts in future studies.

(4) In light of point 3, what is the physical motivation behind choosing the determinantof the strain rate tensor? Why not choose the trace of the tensor, which is also rotationally invariant and is physically motivated by the fact that it appears in the steady-state calving relation (Amundson and Truffer, 2010)? (Isn't dilation rate the trace of the strain rate tensor?) There might be a good reason to choose one over the other, but I would like to know why. (Or maybe it would make sense to use both?)

Response

The relation to the approach by Amundson and Truffer is very interesting. It is however a different approach. This very elegant formula provides the SSA-consistent calving rate that would be required in order for the calving front to be in steady state. This is a diagnostic approach and is not at all at odds with our approach. In fact it could be used to derive further insight into an ice front whose advance is governed by the SSA but its retreat is governed by the eigencalving approach.

With respect to the trace we would like to refer the reviewer to the statement we made above, because the trace is simply the equal-strength contribution of K1+ and K1-. While it is rotationally invariant, we argue that for a universally applicable kinematic (not material) contribution the dependence on eps- and eps+ needs to be within the product of the two, in order for ice shelves to be generally stable.

We have added a figure and a discussion on the matter in section 2.

(5) If this paper is truly building on previous work, then why ignore the potential dependence on ice thickness and thickness gradient (which depends on ice shelf width) (Alley et al., 2008; Amundson and Truffer, 2010)? Including these parameters may help to explain the differences in the proportionality constant from one ice shelf to the next...Again, there may be good reason to do this, but I'd like to see some justification.

Response

We fully agree with the reviewer. Material properties like ice thickness and thickness gradient will be important and need to be captured in the material constant. We have tried to clarify this better in the text now.

(6) There is an assumption in the paper that is somewhat hidden. At the top of p. 2703, the authors claim that the determinant of the strain rate tensor appears to be proportional to calving rate (as estimated by terminal velocity). This assumes that changes in glacier length are small compared to changes in velocity and calving rate. I don't have a problem with that assumption, per se, because it is consistent with

observations – most of the time. I just think it needs to be stated more explicitly as an assumption.

Response
We agree and have made this more explicit on page 2703.

A couple of minor points:

(1) The first few sentences of the abstract are not really abstract material.
Response
We understand the reviewers concern, but feel that this is a matter of flavor, really. If the reviewer does not feel too strongly about it, we would like to keep in order to put the results in a wider perspective.

(2) Line 10 on p. 2702: "Perpendicular" compression also favors ice thickening; thick shelves seem to be more stable than thin shelves.
Response
We hope to have covered this topic in our initial statements, but are happy to elaborate if requested.

(3) Why was Equation 1 presented before Equation 2? A more natural order would be to first present Equation 2 and then use various arguments to reduce it to a simplified form.
Response
We understand the reasoning, but since the reviewer agrees that the paper was well polished we would like to keep the structure in order not to disturb the flow that it currently has. Even though other solution would of course be possible.