Interactive comment on "Fracture-induced softening for large-scale ice dynamics" *by* T. Albrecht and A. Levermann

T. Albrecht and A. Levermann

torsten.albrecht@pik-potsdam.de

Received and published: 31 January 2014

Response to the interactive comment by Referee #2: Chris Borstad

Our point-by-point responses to the comments by Referee Chris Borstad are detailed below. Referee comments are printed in blue font followed by our responses in black.

General comments
This paper describes a model formulation for representing the softening influence of fractures on large scale (e.g. ice-shelf scale) viscous dynamics. This builds upon earlier work by the authors (Albrecht and Levermann, 2012) characterizing the surface

density of fractures using a scalar field variable. In this work, the fracture density is used to soften the ice by feeding back with the depth-averaged ice viscosity through one of two enhancement factors. The model is able to represent sharp gradients in velocity across adjacent flow units separated by fractures or across shear margins of an ice shelf better than a "standard" model with uniform material properties. This work is part of a growing body of literature on model formulations to account for the role of fractures in glacier and ice shelf evolution, and the work outlined in the manuscript does show promise toward contributing to this sector of the community.

I have only one potentially serious concern about the model formulation, which relates to the specific formulation of the fracture density source term and the associated feedback mechanism with the flow enhancement factor. Aside from that, in general I feel that this manuscript covers too much material in too little detail. I applaud the broad scope of the modeling work applied to numerous ice shelves, but for many particular ice shelves I feel that more questions are raised than answered, especially given that only a single transect of velocity is analysed in most cases. Given my concerns about the model formulation outlined below, I think more might be gained by carefully dissecting the results and model sensitivities for a single ice shelf before moving on to a broad survey of results over many ice shelves (though I note that this concern could be addressed by a simple change in the title and scope of the paper to something along the lines of "fracture-induced softening for ice shelf shear margins"). Otherwise the remainder of my concerns can be addressed with a careful rewrite, paying attention to using more direct and descriptive language and clarifying the graphical presentation of the results. I urge the authors not to be discouraged by the length of my review, as the comments are intended to be constructive and contribute toward improvement of the manuscript.

Specific comments

I'm concerned about the coupled forms of the fracture density source term (Eq. 2) and the softening feedback through the enhancement factor (Eq. 6). This concern arises from two observations of the model formulation and results. The first is the fact that such low values of the enhancement factor E_{SSA} $(0.05 - 1)$ are needed to tune the model to match observed velocity data when fracture-induced softening is activated. The second is the fact that there are actually two enhancement factors, one that is uniform across the shelf (E_{SSA}) that is used as a tuning parameter and the second through which fracture-induced softening acts. The former concern suggests that the fracture density source term may be inappropriate, and too much "fracture" is created by this term with the result being that an anomalously low enhancement factor is needed to offset this anomalously high production of fracture density. Rather than invoking a duplicate enhancement factor (E_{SSA}) I would suggest that the source term in Eq. 2 should be investigated for problems or possibly reformulated. I would start by comparing it to a more sophisticated source term, such as that proposed and validated by Pralong and Funk (2005), that uses a multiaxial stress or strain rate criterion (rather than a simple uniaxial strain rate) as well as a power-law dependence on the term $(1 - \phi)$ rather than a simple proportionality. Whatever the form of the source term, I think more could be learned physically about the initiation and evolution of fracture density by properly calibrating the source term rather than assuming a simple form and then invoking an independent tuning factor to get the model to fit the observational data.

We thank the reviewer for these critical but constructive comments. We will try to explain our model strategy more rigorously, which may help to relieve the referee's concerns: In order to discuss the benefits of the model, we need to distinguish between what belongs to the *standard* model and what to the *softened* model.

The E_{SSA}-enhancement factor is part of the standard model and has been used in various applications in PISM and elsewhere (Ma et al., 2010; Winkelmann et al., 2011; Albrecht and Levermann, 2012). It is associated with ice-internal properties like

anisotropy. Varying the E_{SSA}-enhancement factor for the Evans and Byrd Inlet model, the best fit of modeled and observed velocities is at values of about $E_{\text{SSA}} = 0.05$ (while for the entire Ross or Ronne Ice Shelf values of $E_{\text{SSA}} = 0.4$ were used in a previous study). This is exceptionally low and may counteract problems in the model setup, probably in the vertical temperature distribution. In this sense we use it as a tuning factor, which not only accounts for anisotropy of the ice. However, we can use this result and apply the softened model, where an additional enhancement factor E_{ϕ} is applied that solely accounts for the macroscopic effect of fractures on the viscous flow, which is what we actually want to discuss in this paper. This distinction is now more emphasized in the modified manuscript. In order to simplify the comparison, we changed the plots accordingly, such that the same flow parameters are used as in the standard model.

The presented simple model of fracture-induced softening has obviously limitations. A generalization in terms of a power-law dependence as in Pralong and Funk (2005), as suggested by the referee, may bring even more benefit, but data for calibration on the applied scales are rare. However, we want to give it a start and study, to what extent the interaction of fractures and ice viscosity changes the large-scale ice flow. We want to reduce complexity and learn with the model about the characteristics of this interaction. It is definitely not meant as a tuning exercise, but rather as an investigation of effects and feedbacks involved.

It appears that the formulation of the enhancement factor feedback mechanism is based on physically sound reasoning. Borstad et al. (2012) analytically related the classic enhancement factor E to damage D as $E = (1 - D)^{-n}$, which is a similar form as Eq. 6 in absence of the E_{SSA} term. According to this relationship, increasing damage/fracture density leads to increasing flow enhancement. However, the introduction of a leading coefficient $E_{\text{SSA}} < 1$ in Eq. 6 counteracts this softening feedback. In fact, for $E_{\text{SSA}} = 0.05$ as in Figure 8, the overall enhancement factor E_{A} is less than one for a fracture density below about 0.6 according to Eq. 6. An

enhancement factor less than one indicates stiffening of the flow, whereas $E_A > 1$ indicates softening enhancement. Thus the bizarre result here is that the flow of Byrd Inlet is modeled using an overall enhancement factor that *stiffens* the flow even when a moderate level of fracture density is present. This suggests a problem with the model formulation, as an enhancement factor so much smaller than one should not be needed to capture at least the bulk flow features in unfractured areas (a couple examples: Ma et al. (2010) found that enhancement factors of E $0.6 - 0.7$ are appropriate for accounting for anisotropy of an ice shelf, whereas Scambos et al. (2000) used $E = 3 - 8$ for Larsen B). My concern is that the term E_{SSA} may have been introduced post-hoc as a simple way to "tune" the model rather than diving more into the physics underlying the source term, where I suspect the real problem lies.

Regarding our response to the referee's concerns above may clarify the situation. The presented fracture-softening model is associated with only one enhancement factor here, E_ϕ, and it corresponds exactly to the enhancement factor in Borstad et al. (2012). We did not mean to irritate the referee and highlight his physical reasoning with the reference at the right position in the manuscript. Enhancement factors $E_A < 1$ are hence not problematic, since this stiffening adjusts shortcomings in the standard model setup. In contrast to the considered inlets in Ross and Ronne Ice Shelf, for Larsen B Ice Shelf an enhancement factor $E_{SSA} > 1$ yields more realistic velocity fields (dashed profiles in Fig. 12), as in Scambos et al. (2000). By using $E_{SSA} = 1$, we can show in this example that fracture-induced softening alone has the potential to explain the difference between the standard model and observations. However, other effects like marine ice accretion at the bottom (Jansen et al., 2013) may have contributions, too. This attribution problem is discussed in the revised manuscript.

Aside from that, the discussion of fracturing as a kind of "self-amplified" process (Sections 3 and 5.1, and sprinkled elsewhere throughout the manuscript) is a bit confusing, but I take it you are describing fracturing as setting up some kind of positive

C3159

feedback process, whereby an initial fracture causes an increase in the effective stress, which causes additional fracturing, and so on in a runaway feedback. I'm not sure this conclusion is supported by your results, nor does it seem consistent with observations. For individual crevasses, this logic would seem to imply that once a crevasse forms, it will continue to grow or propagate, though I'm not sure there are any observations of crevasse depths that continually increase along a longitudinal transect on an ice shelf. Also consider the recent results of Walker et al. (2013) who demonstrated that the majority of rifts studied in 13 different ice shelves did not propagate at all over the last decade; they were simply "dormant". If it is common for fractures to initiate, propagate for a short time, and then become dormant, then I hardly think a positive feedback mechanism is operating (or at least the question of what would interrupt such a feedback becomes pertinent).

Observations suggest, that brittle fractures propagate in regions of strong loads on a comparably fast time scale. We assume, however, that fracture growth comprises also the formation of additional fractures, as long as the prevailing effective stress exceeds the critical stress threshold. This formation happens with a rate that is proportional to the main spreading rate ($\dot{\epsilon}_+$) and which is reduced by already existing fractures ($1 - \phi$), as expressed in Eq. 2. From there the fractured ice is transported with the ice flow and fracture growth can become dormant, if the prevailing load gets too low for additional fracture formation. However it can be activated if the fracture band passes along a regime of high stresses. Due to the transport of fractures with the ice flow and the fracture-induced softening, fracture formation at one point in the ice shelf can influence the stress elsewhere and hence also the fracture formation in other regions. This applies also for the formation of rifts across long distances from pre-existing crevasses, though the propagation across several grid cells is not not captured yet by our fracture density approach.
So what do we mean with "self-amplification" in this interaction of fractures and ice flow? We can learn from the model that, if fracture formation becomes active at

C3160

one point ($\phi > 0$, either by reducing the threshold or by flow acceleration), it can potentially activate additional fracture formation also further downstream, such that a dense fracture band ($\phi \rightarrow 1$) may form all the way towards the calving front, if healing is low. Since additional fracture formation is limited by the interaction of fractures (factor $1 - \phi$), at some point fracture growth becomes dormant. In other words, we have basically two states: one state where fracture formation/density is too low for the formation of additional fractures further downstream and another state, where fracture density increases downstream towards maximal density, as shown in Fig. 7. Which of these states becomes realized depends with high sensitivity on the chosen initiation threshold. If we consider observations of fracture bands in ice shelves, it is hard too tell, which fracture band belongs to which state, because fracture density can stay constant along the flow in both cases, and initial fracture density can be constantly high also in the off-mode. However, regarding Larsen B Ice Shelf, we could think of a situation, where additional fracture formation due to meltwater infiltration could have initiated such a positive-feedback mechanism, where ultimately the whole ice shelf got mechanically decoupled and densely fractured leading to its disintegration.

Finally, can you rule out the role of temperature in accommodating the strong shear across the boundaries between flow units or shear margins that you analyse? If marine ice is present at the base of the shelf in any of these areas, then the ice column will be warmer and will thus deform more readily. Jansen et al. (2013) demonstrated that accounting for this warm layer of ice in an ice shelf model can produce the strong shearing across flow units observed in velocity data. It's not clear how (or if) you're treating the ice temperature in your model, which could be a significant limitation of your results. If fracture density is the only model parameter that can vary spatially, then you're implicitly lumping the influence of temperature in with your fracture-induced softening. Therefore it's possible that the temperature alone might explain the sharp gradients in across-flow velocity in some areas where you explain them by fracture-induced softening.

C3161

This is in fact an astonishing result by Jansen et al. (2013), which we cite in the modified manuscript. We also added a short description on how the three-dimensional temperature field is calculate in PISM: We are using an semi-implicit diffusion scheme for the vertical distribution, where upper and lower boundary temperatures come in as boundary conditions. This temperature is advected with the horizontal ice flow, where strain-heating comes in as heat-source. However PISM does not distinguish between meteoric and marine ice assuming constant density in the whole ice column, which is actually a shortcoming. Since marine ice formation areas coincides with those regions where fracture formation occurs (e.g. downstream of inlet margins and ice rises), and both get advected with the ice flow, it will be challenging to attribute the observed sharp gradients to the two different processes. It seems that both mechanisms could alone explain the observed flow features. We discuss this issue in the revised manuscript.

Figures

Many of the figures have components that are difficult to interpret, and could use clarification. In many instances, this could be accommodated by using explicit legends rather than describing each plot figure in the caption (I found myself manually labeling many of the figures to keep the different components straight). Some specific comments on the figures:

Figure 1: The solid and dashed lines need to be labeled more clearly, perhaps starting with a title for the legend. The values used for E_{SSA} (0.6, 0.8 and 1.0) do not cover the range of values used in the paper ($0.05 - 1$). For values of $E_{\mathsf{SSA}} < 1$, the viscosity of the fractured ice is actually *stiffer* (viscosity ratio greater than one) than for the model without fracture coupling. This is a problematic result.

C3162

We thank the referee for his valuable suggestions: Fig. 1 has been reorganized, varying only the parameter E_{SSA} in the range, that has been used in the realistic simulations. The aim of the figure is, to show how the direct proportionality of the viscosity to the fracture density translates into the non-linear softening.

Except for Larsen B Ice Shelf, in all other real-world examples $E_{SSA} < 1$ has been chosen, both in the standard model and in the softened model, accounting for material properties of the ice and shortcomings in the model setup (as explained above). This allows for a direct comparison of both models.!

Figure 2: The contour colors on panels a-c, indicated on the colorbar, are difficult to distinguish. Since no softening or healing is applied in panel d, can you quantify the amount of fracture density "lost" to diffusion along a flowline? It would be useful to know how much of your "signal" you are losing as you advect it.

We have reordered the color scheme of the thickened contour lines for the Larsen C example. However, the diffusion of the fracture density bands in this example consists of both numerical diffusion and diffusion due to the diverging flow field. In this example we used the improved transport scheme (Eq. 10), where numerical diffusion is less angle-dependent. We deal with this question in more detail in Figs. 4+5.

Figure 3: This is kind of a confusing plot. Perhaps a side-by-side comparison of your new advection scheme compared to a standard first-order upwinding scheme would make more sense?

Yes, a direct comparison side by side makes sense here, we followed the referee's suggestion here.

Figures 4 and 5: The plot panels need more labeling or a legend, as the colors for the different curves are not labeled nor explained in the caption. A conceptual

C3163

graphic to accompany this plot would be helpful.

We reorganized the panels of Figs. 4+5 and consider only the cross sections at three different distances. We added labels for the curves and conceptual figures as bird's eye view to explain the model setup.

Figure 7: The colors and different curves in panel b are confusing. For panel c, is this one curve or multiple curves? I'm not sure that the schematic for the potential feedback is appropriate, as it can be adequately explained in the text (same for Figure 13).

We simplified the color scheme of panel b distinguishing between four phases, which can be identified more easily in panel c. Every point in panel c corresponds to one curve in panel b, we make that clearer in the manuscript. The schematic may seem simplistic, but it is a standard for the visualization of feedback mechanisms, as widely used in climate science literature, and it may help the reader to orientate!

Figures 9-13: A spatial map showing the misfit between standard-observed and softened-observed cases might be helpful, as the information in the observed vs. computed speed panels can be conveyed without the figure by simply stating the rmse for each case. How is the FESOM melting-factor described in the captions used in the model? For Figure 12, it's not clear which arrows correspond to which plots.

We decided to attach the velocity anomaly maps in some supplement figures. However, we would like to keep the scatter plots, since the RMSE only is too simplistic. E.g., in the Byrd Inlet case the reader could be misled, where RMSE is larger for the softened case due to the side shift, but the gradient are much better represented.

The FESOM melting-factor simple scales the prescribed data for the ice shelf subsurface. This correction is necessary, since the coupling is only one-sided, the changing

C3164

ice geometry does not affect the basal melt rates here.

- p. 4502, Lines 5-6: was your objective really to better *understand* the role of fractures? The objective implicitly presented in the manuscript was to represent the role of fractures in a large scale model and compare the results to observations. You didn't conclude with any new understanding about the role of fractures, so you might consider changing the stated objective here.

This is an important statement, and we reformulated it accordingly in the manuscript: "In order to account for the macroscopic effect of fracture processes on large-scale viscous ice dynamics (i.e. ice-shelf scale) we apply a continuum representation of fractures and related fracture growth into the prognostic Parallel Ice Sheet Model (PISM) and compare the results to observations."

- p. 4502, Lines 12-13: this is a confusing sentence

Right, we restructured the sentence as follows: "As a result of prognostic flow simulations, large across-flow velocity gradients appear in fracture-weakened regions. These modeled gradients compare well in magnitude and location with those in observed flow patterns."

- p. 4502, Line 16: how does the model account for climate-induced effects on fracturing? Or do you mean that it is expandable to possibly account for such effects?

This is indeed confusing. We modified the sentence as: "This model framework is principally expandable to grounded ice streams and provides simple means of

C3165

investigating climate-induced effects on fracturing (e.g. hydro fracturing) and hence on the ice-flow."

- The terminology of "fracture-coupled processes", which is used in many places, is a bit awkward and confusing.

We improved the wording, using "the dynamic effects of fracture processes" in the abstract and "the feedback of fractures" in the introduction.

- p. 4503, Lines 7-9: The references at the end of this sentence do not support the assertion that fractures play a fundamental role in ice streams and ice shelves.

This is right, but we aimed at giving examples for the interaction of fracture processes with ocean melt and atmospheric warming. We split up the references, where the latter (MacAyeal and Sergienko, 2013) refers to " atmospheric-warming induced surface melt".

- p. 4503, Line 13: what do you mean by "expand"? Do the fractures grow longer or wider?

No, we changed the wording and used "extent" here, to make clear that the fracture bands can span the whole distance down to the calving front.

- p. 4503, Lines 14-15: The description is confusing, as you're already talking about fractures that are advecting with the flow. How can the stresses change to activate fracture formation if the fractures are already present?

We assume that fractures form if the effective stress is above the threshold. The rate of additional fracture formation depends on the abundance of pre-existing

C3166

fractures $(1 - \phi)$. In this sense we reformulated this sentence as: "On that journey along the stream, prevailing stresses can change and activate additional crevasse formation", which can be both, new crevasses or crevasse propagation.

We were thinking of the two direction of the interaction of fracturing and ice flow on each other. We reformulated the text accordingly introducing "the effect of the ice flow on fracture formation first" and later the"macroscopic feedback of fractures on the viscous ice flow". We added the reference to the book by Schulson and Duval (2009), which provides a nice overview to the topic.

Sentences in this part has been reordered: "The disintegration of large parts of the ice shelves provokes a more efficient drainage of the upstream glaciers".

We wanted to express, that this phenomenon has been investigated by considering some examples, but not in a comprehensive study. We see the point of the referee and try to improve the wording throughout the manuscript.

C3167

We added a short overview here with corresponding citations.

This is correct, we do not explicitly account for the complex dynamics of rift propagation, which occurs often transversally to the flow across many grid cells, as long as large stresses concentrate at the rift tips. This is definitely a limitation of the model and we would like to work on this in future studies. We will emphasize this limitation in the manuscript.

We agree with the referee, that the word "probability" is misleading here, we dropped it or replace it by "rate".

"Recrystallization and snow cover" belong to surface processes. However, "closure in moderate stress environments" can principally also affect bottom crevasses,

C3168

referring to the the ice overburden pressure. However, water pressure in bottom crevasses counteracts this form of healing, as surface melt water does for surface crevasses. And refreezing of ocean water in bottom crevasses can have a healing effect, too. We reformulated as: "Existing fractures can experience deactivation by snow cover and refreezing at the surface, or closure in moderate stress environments by the ice overburden pressure in both surface and bottom crevasses". And in the Introduction we added the sentence: "Generally, the ice-overburden pressure tends to close open crevasses as opposed to the tensile stress mode (Nye, 1957)."

- p. 4506, Lines 11-12: tell the reader what these sensitivities are, at least briefly, rather than making them chase down the details in another paper.

We avoided summarizing parameter sensitivities from the previous paper here, since "the insights of this previous study are associated with a model setup, where the evolving fracture-density pattern is considered for a prescribed ice thickness." Different parameter ranges are used in the current study with different sensitivities associated with the introduced feedback mechanism.

- p. 4506, Lines 16-18: What simplifications are you making from standard continuum damage mechanics? Be explicit, as this can shed light on any limitations (or possible advantages) of your approach.

Basically we avoid the generalized power-law formulation of the standard CDM and investigate, to what extent simple linear relationships may represent the fracture-induced effects on the flow. We emphasize this in the modified manuscript.

- p. 4507, Line 1: what are these "distinct dynamic characteristics"? How distinct are they? Be more specific and targeted with your language.

C3169

We made this comment more specific: "...separate slow-moving areas from flow units coming in from different inlets with different speeds".

- p. 4507, Lines 3-6 and Equation 5: here you're making it sound like you're applying continuum damage mechanics, yet this is not the form of the viscosity that would result from the strain equivalence principle because you've manually inserted an extra enhancement factor (Equation 6). Furthermore, the strain rate is unmodified by the equivalence mapping, but the stress balance equations are modified.

As mentioned before in this response, E_A the softening consists of two parts, $E_A = E_{SSA} \cdot E_\phi$, while the E_{SSA} belongs to the standard stress balance. However, E_ϕ is associated with the fracture-induced softening, which is exactly adopted from continuum damage mechanics, based on the strain-equivalence principle. Since E_{SSA} is an ice-property and used in both models, strain rate can be assumed to be equivalent when fractures come into play. We will emphasize this in more detail in the manuscript. We further changed the sentence: "the stress-balance equation changes using a modified effective viscosity..."

- Equation 6: I doubt it is coincidental that your enhancement factor formulation takes the form of $E_A \propto [1 - \phi]^{-n}$, which is precisely the analytical form derived by Borstad et al. (2012), so you should probably reference this study here.

Absolutely correct, we have added the reference at this point once more.

- p. 4508 Lines 1-2: Confusing sentence. What is the discontinuity, and how is this "ambiguous"?

We modified this sentence as: "Approaching vanishing viscosity and hence infinite enhancement can be interpreted as pathway towards quasi-discontinuous model

C3170

behavior, where domains seem mechanically decoupled."

Right, we expressed is more general and included "tension" at this point. However, this feedback effect appears to be strongest in shear regions of the model domain. We also changed the title of the manuscript as suggested by the referee.

Initiation here refers to a location, where fracture growth occurs, either by formation of new fractures or by the growth of existing fractures. We dropped "initiation" here.

This is right, we want to keep the introduction of initiation criteria and their application/comparison isolated in one paragraph. The small paragraph about vertical fracture propagation, however, has been converted into an extra paragraph of the introduction.

This is an important point and we have rephrased respective text paragraphs to make this distinction clear. However, in the rest of the manuscript we consider the von-Mises criterion only, which is a classical material-strength criterion.

This paragraph is meant to compare between different criteria on the large scale. Choosing the von-Mises criterion for the further experiments is a result from this comparison. We highlight this now in the text.

Added.

Right, has been changed.

This is an assumption where the vertical changes are still small compared to horizontal changes. However, this paragraph is not longer part of the manuscript.

● Section 4.4: Can you discuss the potential variability in the softening influence of your inferred fracture density depending on the nature and location of the fractures? It would appear that the surface expression of a basal crevasse gets "counted" the same as a surface crevasse, even though the basal crevasse might be expected to have a much greater influence on the flow and stress regime since it occupies a much greater fraction of the ice thickness. Furthermore, how are rifts handled? It seems to me that some kind of weighted fracture density calculation might be more appropriate, whereby a rift gets more weight than a basal crevasse which gets more weight than a surface crevasse. Of course this would assume that you could distinguish the difference between surface crevasses, basal crevasses and rifts in your imagery.

We thanks the referee for his nice suggestions. Yet, in the satellite images we do not explicitly distinguish between surface or bottom crevasses or rifts. We state in the text, that surface crevasses (and highly fragmented areas) will hardly be visible on these images, such that we can assume that the identified features correspond to dynamically dominant bottom crevasses (troughs) or rifts only. This provides a limited inventory for a comparison to the modeled data, why we compare roughly evolving patterns here. On the evolution model side, we do not consider explicitly the formation of rifts, since it is rather complex and involves vertical bending and localized stress concentrations, which can only be considered using finite element techniques. We are aware, that principally rifts may be considered in terms of damage evolution.

● p. 4514, Lines 7-8: Define "SOR." Also, using the 2007-2009 velocity data as Dirichlet Boundary Conditions for the inlets of Larsen A and B ice shelves hardly seems appropriate given that the tributary glaciers accelerated by 3–8 times following the collapse of these ice shelves. Shouldn't these velocities be scaled down to

represent more appropriate values when the ice shelves were present?

Added SOR "(successive over-relaxation)". We agree, that this temporal misfit of data may be problematic, but the acceleration did not occur uniformly for each inlet. Furthermore, older datasets have large gaps in the inlet regions. In our model setup, strong enhancement is needed to actually drain the inlet ice into the ice shelves, such that the ice shelf speeds are not that sensitive to the prescribed inflow at the inlet boundary. We will discuss this point in the manuscript.

● p. 4514, Lines 10-14: this description is confusing. Can you elaborate and clarify?

Since we leave out the Pine Island example, this paragraph is dropped anyway

● p. 4514, Line 24: What do you mean by "ice-free" walls? Are these friction-less boundaries?

In the simplified setups, friction along the ice-free side margins (walls higher than ice surface of the confined ice shelf) is prescribed in terms of a boundary viscosity. We added "friction along the side margins is prescribed, such that the intensity of shear flow can be controlled."

● For the ice shelves, are you using an equivalent ice thickness or the actual thickness of the ice shelf? This makes a difference for computing stresses within the shelf, which will impact where fractures are predicted to form (Kenneally and Hughes, 2004).

We consider the actual ice thickness with a vertically constant density?

• p. 4515, Lines 9-10: is the healing physical then, or is it due to numerical diffusion? Can you distinguish between the two, or quantify their relative significance?

Here, there is no explicit healing applied according to Eq. 4. However, the diverging flow in the shear zone contributes to the decay and certainly also some numerical diffusion. Regarding the magnitude of the latter component, Figs. 4+5 may give an impression. We restructured the whole paragraph of the manuscript.

• p. 4515, Line 19: Is this really hysteresis? I'm not sure I would interpret this result as the system having some kind of "memory."

We rephrased this sentence: "Hence, this switch between the two stable states occurs at different levels of the control parameter due to the memory of the system, which can be interpreted as hysteresis behavior."

• p. 4516, Lines 1-2: Which parameters? How are they "roughly" estimated?

We added some information here: "Parameters are for the standard run are estimated on the basis of an ensemble study minimizing the root mean square error (RMSE) of both ice thickness and ice speed compared to observations. However, this study does not intend to find the best possible fit, but to investigate the qualitative changes of the ice flow induced by the employed fracture feedback for each individual setup."

• p. 4516, Line 9: Define of quantify how the results are "reasonable"

We rephrased accordingly: "A constant flow enhancement $E_{\mathrm{SSA}} = 0.05$ yields a RMSE of about $95\,\mathrm{m\,yr}^{-1}$ for simulations of the whole Byrd inlet domain, where the standard model underestimates velocities in the inner inlet and overestimates

C3175

overestimated them closer to the margins respectively."

• p. 4516, Line 18: the orange contour lines very small and difficult to resolve in the figure.

This might be an issue of the vector graphics representation, we increased the line size.

• p. 4517, Line 11: this is a threshold stress, not a fracture toughness (same on next page, Lines 4-5)

Modified.

• p. 4519, Lines 9-11: Enhancement factors larger than 1 are actually more common than values less than 1 (e.g. Ma et al., 2010), so some kind of explanation or justification is needed here.

The results by Ma et al. (2010) support the use of flow enhancement factors larger than 1 in grounded areas. We rephrased the sentence: "This applies even for enhancement factor $E_{\mathrm{SSA}} = 1.0$ as base level for unfractured ice, which is more realistic for ice shelves in tension than values larger than 1 (Ma et al., 2010)."

• p. 4520, Lines 9-13: This makes it sound like you modeled grounding line retreat. Did you? If so, you should expand on this (probably a lot) here. If you're describing more of a hypothetical feedback scenario, then make this clear.

Case not considered any more.

• p. 4520, Lines 16-18: The ice flow dynamics of glaciers and ice sheets is al-

ready nonlinear. Hysteresis is not the same thing as irreversibility.

Sure, we have added "additional non-linear characteristics into the ice-flow dynamics such as dynamic regime shifts (bifurcation), hysteresis behavior and irreversibility."

- p. 4520, Lines 25-27: reference needed at end of sentence.

Added (Vaughan, 1993) one sentence earlier.

- p. 4521, Lines 15-16: actually, the tensile strength and fracture toughness of ice are not very sensitive to temperature (Schulson and Duval, 2009), even though this claim gets repeated frequently in the glaciological literature.

Replaced with "This cannot be just explained just by temperature effects, since the decrease in fracture toughness for increasing temperatures is comparably small (about $-0.5\,\mathrm{kPa\,m^{-1/2}\,^{\circ}C^{-1}}$ according to Schulson and Duval, 2009, Fig. 9.4)."

- p. 4522, Lines 15-16: I don't think you've substantiated this claim.

Dropped.

- p. 4522, Lines 17-19: This is confusing. Are you claiming that you've accounted for all the relevant softening processes you've listed, including microscale processes and damage-induced anisotropy?

No, but we used the word "comprising" in the meaning of "not distinguishing between". We omitted the examples.

- p. 4523, Lines 1-17: This is a nice discussion, but it might be better placed

C3177

(or repeated) near the beginning of the manuscript (Introduction or Background) to better frame the context of the study.

This is a very good idea, has been shifted to the introduction.

- p. 4523, Line 21: I'm not sure you accounted for fracture "interactions" explicitly, is this what you meant here?

We rather parameterize fracture interaction, expressed in the term $1 - \phi$ in Eq. 2. But in this paragraph we actually talk about (switched order) "the gross interactions of flow dynamics and fracture processes".

References

T. Albrecht and A. Levermann. Fracture field for large-scale ice dynamics. *J. Glaciol*, 58, 2012.

C. P. Borstad, A. Khazendar, E. Larour, M. Morlighem, E. Rignot, M. P. Schodlok, and H. Seroussi. A damage mechanics assessment of the larsen b ice shelf prior to collapse: Toward a physically-based calving law. *Geophysical Research Letters*, 39(18):L18502, 2012.

Daniela Jansen, Adrian Luckman, Bernd Kulessa, Paul R. Holland, and Edward C. King. Marine ice formation in a suture zone on the larsen c ice shelf and its influence on ice shelf dynamics: Marine ice impact on ice shelf dynamics. *Journal of Geophysical Research: Earth Surface*, 118(3):1628–1640, September 2013. ISSN 21699003. doi: 10.1002/jgrf.20120. 00000.

J. P Kenneally and T. J Hughes. Fracture and back stress along the byrd glacier flowband on the ross ice shelf. *Antarctic Science*, 16(03):345–354, 2004. ISSN 0954-1020.

Y. Ma, O. Gagliardini, C. Ritz, F. Gillet-Chaulet, G. Durand, and M. Montagnat. Enhancement factors for grounded ice and ice shelves inferred from an anisotropic ice-flow model. *Journal of Glaciology*, 56(199):805, 2010.

D. MacAyeal and O. V Sergienko. The flexural dynamics of melting ice shelves. *Annals of Glaciology*, 54(63):1, 2013.

C3178

A. Pralong and M. Funk. Dynamic damage model of crevasse opening and application to glacier calving. *Journal of Geophysical Research*, 110(B1):B01309, 2005. ISSN 0148-0227.

T. A Scambos, C. Hulbe, M. Fahnestock, and J. Bohlander. The link between climate warming and break-up of ice shelves in the antarctic peninsula. *Journal of Glaciology*, 46(154):516–530, 2000. ISSN 0022-1430.

Erland M. Schulson and Paul Duval. *Creep and Fracture of Ice*. Cambridge University Press, April 2009. ISBN 9780521806206. 00107.

D. G Vaughan. Relating the occurrence of crevasses to surface strain rates. *Journal of Glaciology*, 39(132):255–266, 1993. ISSN 0022-1430.

C. C. Walker, J. N. Bassis, H. A. Fricker, and R. J. Czerwinski. Structural and environmental controls on antarctic ice shelf rift propagation inferred from satellite monitoring: Antarctic ice shelf rifting. *Journal of Geophysical Research: Earth Surface*, pages n/a–n/a, November 2013. ISSN 21699003. doi: 10.1002/2013JF002742. 00000.

R. Winkelmann, MA Martin, M. Haseloff, T. Albrecht, E. Bueler, C. Khroulev, and A. Levermann. The potsdam parallel ice sheet model (PISM-PIK)–Part 1: Model description. *The Cryosphere*, 5:715–726, 2011.