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Discussions

Interactive comment on "Calving cycle of the Brunt Ice Shelf, Antarctica, driven by changes in ice-shelf geometry" *by* Jan De Rydt et al.

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1   Overarching comments

This study describes a comparison between observations and model inferred stresses for the Brunt ice shelf. The authors demonstrate that the collision of the Brunt ice shelf with a pinning point resulted in increased compressive and tensile stresses and argue that this resulted in the increased tensile stresses needed to reactivate rift propagation. Overall, the results are highly glaciologically relevant and add to our understanding of ice shelf rift propagation.

The manuscript does feel a little bit like it was originally intended for a short form journal

with a restrictive length requirement and has not taken advantage of the more generous space allocated by longer form journals. As such, I had a hard time understanding what was actually done and it seemed as though there were critical details missing from the exposition. Without those details it is hard to assess the reliability of the methods and conclusions drawn. There are also a few places in the manuscript where the text does not appear to accurately reflect the literature or conclusions are not fully supported by the results. My review is relatively long, but most of the issues are (hopefully) easy to correct by expanding the text to include more critical information.

2   Major Comments:

The most significant issue in the manuscript relates to how the authors define a "rift" in the model and how this compares to how the rift actually behaves. We know that rifts in ice shelves can be discontinuities in the ice (fractures) that are filled with ocean water. Rifts are also often filled with a mixture of snow, sea ice and blocks of ice called melange. The melange can become structurally coherent to the point that rifts are barely visible on the surface of the ice, although this tends to be more common in relic rifts that have not been active for decades (or even centuries). Rifts can be represented in models in different ways. The most physically consistent way is to incorporate rifts as actual discontinuities in the ice shelf (see Larour et al., 2014). One then needs to account for the thickness of melange that fills rifts when applying a normal traction boundary condition along the rift walls, analogous to the calving front boundary condition. Historically, rifts have also been represented in models as regions of intact ice with a rate factor that is set (or inferred) to be much lower than is traditional for intact ice. In this representation, rifts not discontinuities and these features essentially behave like diffuse zones of really warm ice and NOT as fractures. The authors need to be clear about how rifts are represented in their model before any inferences can be made about the effect of "rifts" on the dynamics of ice shelves.

A second, and closely related point, arises from circularity in tuning a model to fit observations and then using the fact that the model fits the observations to argue that the model is appropriate. As noted earlier, there are different ways of representing rifts in a model and it is far from clear that the stress field associated with these different representations of rifts are equivalent. In fact, representing rifts as discontinuities will generate stress concentrations near the tip of the rift that will not be present in models that represent rifts as diffuse zones of soft ice. Moreover, when tuning a model to match observations, it is often possible to absorb all uncertainties and errors into the parameters that are being tuned. For example, in traditional damage mechanics, the damage affects the Cauchy stress and thus would also affect the driving stress on the right hand side of the SSA equations. Similarly, errors in density or briny layers of marine ice could also affect the driving stress. One can, of course absorb this error into inversions for the rate factor, but it is less clear that the inference of the rate factor is *physically* significant. Here, the authors can do more to make their case by describing how rifts are represented in the model and showing actual maps of the rate factor and, if possible, converting those maps to physical variables, like equivalent ice temperature. Fortunately, the Brunt ice shelf itself appears to be well studied and the authors should be able to compare inversion results with the position of known bands of marine ice inferred by King et al.

We get into similar issues when the authors argue that rift widening causes stress concentrations ahead of the rift. I don't follow the inference if all the authors have done is tune the model to match observations. The authors can, however, make this inference, if they have instead performed a forward model run and the widening of the rift predicted by the model matches observed widening rates and the stress increase near the tip of the rift matches the inferred stress increase. Again, this points to a need for some explanatory text.

Larour, E., et. al., (2014),Representation of sharp rifts and faults mechanics in modeling ice shelf flow dynamics: Application to Brunt/Stancomb-Wills Ice Shelf, Antarctica,

J. Geophys. Res. Earth Surf., 119, 1918-1935, doi:10.1002/2014JF003157.

3   Detailed comments

1. The introduction states that the focus of the manuscript is "on the more commonly neglected internal drivers that underlie rift initiation and propagation." It is far from clear to me that this accurately reflects the literature. For example, Fricker et al., 2002, Joughin and MacAyeal, 2005, Larour et al., 2004; Borstad et al., 2012 and 2017 all examine the glaciological stress as the dominant factor driving rift propagation. (There are many, many more citations. These are just a few examples. In fact, almost all of the literature that I can find points towards glaciological stress as the driver.)

More recently (and in the Cryosphere) Arndt (2018) note the role of pinning points in generating rifts in Pine Island glacier, which seems analogous to the study here. In fact, as far as I can tell, with the exception of the Antarctic Peninsula Ice Shelves (that all seem to have effective social media accounts and PR departments), most of the literature for ice shelves does focus on internal stresses. That said, the controls on rifting remain poorly understood, in part because of the long time scale between calving events make the process difficult to directly observe. This observational deficiency, in my opinion, is one of the more significant motivations and strengths of the present manuscript and it would be useful to re-emphasize this to readers.

Fricker, HA, Young NW, Allison I, Coleman R. 2002. Iceberg calving from the Amery Ice Shelf, East Antarctica. Annals of Glaciology, Vol 34, 2002. 34

Joughin, I., and MacAyeal, D.R. (2005),ÂăCalving of large tabular icebergs from ice shelf rift systems,ÂăGeophys. Res. Lett., 32, L02501, doi:10.1029/2004GL020978.

Larour, E., Rignot, E., and Aubry, D. (2004), Modelling of rift propagation on Ronne Ice Shelf, Antarctica, and sensitivity to climate change, Geophys. Res. Lett., 31, L16404,

doi:10.1029/2004GL020077.

Borstad, C., McGrath, D., and Pope, A. (2017), Fracture propagation and stability of ice shelves governed by ice shelf heterogeneity, Geophys. Res. Lett., 44, 4186–4194, doi:10.1002/2017GL072648.

Borstad, C.P. et al. A damage mechanics assessment of the Larsen B ice shelf prior to collapse: Toward a physically-based calving law. Geophy. Res. Lett. 39, L18502 (2012).

Arndt, J. E., Larter, R. D., Friedl, P. , Gohl, K., Hoppner, K.Âăand Âăthe Science Team of Expedition PS104, (2018): Bathymetric controls on calving processes at Pine Island Glacier , The Cryosphere, 12(6), pp. 2039–2050. doi:10.5194/tc-12-2039-2018

2. Iceberg calving is a natural process and this needs to be more clearly emphasized

The broader context in the introduction is the looming calving event from the Brunt Ice Shelf. Building on the previous point, it is known that ice shelves exhibit a natural cycle of decades to centuries advance punctuated by episodic retreat associated with the detachment of large tabular icebergs (see, Fricker et al., 2002, Walker et al., 2015). These calving events are believed to be driven by a combination of the accumulation of fractures coupled with changes in the glaciological stress. Yes, there is evidence for climate driven disintegration of ice shelves, primarily on the Antarctic Peninsula, but this is more of an exception to the norm. I suggest that the authors consider adding more context to the introduction, explaining not only that calving is part of the natural cycle of ice shelves, but how the calving event from the Brunt Ice Shelf fits into this larger context. How similar is this event from previous events? Or are are there no records of previous events? How does the cycle compare to other ice shelves?

Similarly (and sorry for being pedantic), one of the issues that hindered my understanding of the broader context of the study is that the term "unique" is used frequently (4 times at least) and it was unclear what, exactly was unique in each of these instances?

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The first time unique was used, it was used to describe the 50-year time series. This seems like appropriate usage. But, the next time we are told there is "a unique opportunity to enhance . . . process-based understanding". What is unique about the opportunity? Is this the 50-year time series? If this calving event is a continuation of the natural cycle, then (pedantically), the opportunity is not unique. There are also other rifts on other ice shelves that have been (or can be) studied. What exactly is unique about this opportunity/rift? The third time we are told there is a "unique, network of up to 15 GPS". GPS have been deployed around rifts (propagating and not propagating) in ice shelves multiple times so what about this particular deployment is unique. Finally, we are told "BIS represents a unique setting . . . calving processes can be studied" This sounds like the authors are arguing that rift propagation/iceberg calving is different in this situation than the calving cycle that is observed elsewhere? It would be helpful to clarify all of this.

Overall, I think that the authors could help readers understand the significant of the Brunt Ice Shelf and the particular rift system by sketching out what is common about the iceberg calving process across ice shelves. Then, tell us what is unusual in this situation (is it just the observations? the pinning point?) and what is truly unique here (is Brunt itself unique due to the large heterogeneities?). This would enable readers to better understand how this study fits into the broader context of rifting and calving from other ice shelves.

Walker, C., Bassis, J., Fricker, H., Czerwinski, R. (2015). Observations of interannual and spatial variability in rift propagation in the Amery Ice Shelf, Antarctica, 2002–14. Journal of Glaciology, 61(226), 243-252. doi:10.3189/2015JoG14J151.

3. Methods, part 1 (inversions)

This is the where I really started to struggle to understand what was done and there is critical detail missing from the description of the model and inversion process. The model is described as a shallow-shelf approximation model SSA, which is standard

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for ice shelves. My understanding of the inversion is that the authors invert for the rate factor A(x,y) by ingesting surface velocities into the model. However, the inversion uses two regularization parameters γ_a and γ_s neither of which are defined in the text. Digging into Reese et al., 2018, it looks like the regularization parameters correspond to the ice softness AND basal friction coefficient. But an ice shelf, by definition, is freely floating and there is no basal friction. Are the authors inverting for basal friction beneath the pinning points? Is this done everywhere or in certain places? More details and more clarity are needed to understand what has been done here. I now see all the way at Page 8 that a Weertman sliding law is used specifically for the pinning points. This needs to be explained much earlier if it fits into the inversions. Also, what is the shape of the pinning point? Is the ice shelf plowing over it or is the pinning point just tickling the bottom?

4. Deviatoric stress is not constant with depth

There are also more subtle issues associated with the interpretation of the inferred rate factor. In the SSA approximation strain rate is independent of depth. However, the stress is only independent of depth if the temperature in the ice is constant within each column of ice. What the authors are really inferring is the depth averaged rate factor. A consequence is that the authors are also only able to show the depth averaged deviatoric stress. Stress could be much higher near the surface of the ice, where temperatures are likely much colder. This needs to be recognized and explained and in particular, related back to the physical interpretation of the rate factor of ice.

5. Methods, part 2 (maps of rate factor please)

It helpful to readers to see the actual maps of inferred ice softness and basal friction (if this was also inverted for). This would certainly help convince readers that patterns of rate factor are realistic and not spurious artifacts. This is a matter of preference, but I personally also like to see the inferred rate factor converted into an ice temperature so that we can be sure that the ice temperature is semi-realistic based on known con-

ditions. The authors note that these are related to structural properties of the ice. In particular, it would be helpful to know if the inversions for ice softness correspond to regions of marine ice documented by King et al., (2018). In fact, one also wonders if the inversion could resolve the sharp variations in material properties associated with the bands of marine ice documented by King et al., (2018). A standard way to test this is by doing a "checkerboard" test. You compute the forward model using a checkerboard or other pattern. You then add noise to the signal and invert based on the synthetic data. This would give a sense of the resolution of the inversion and if the inversion can pick up relevant structural features. The more formal way of doing this would be to construct resolution kernels to formally determine what can and cannot be resolved.

6. Observations, how do you shift the data to a date?

I thought the observation section was much clearer and easier to understand. But it was unclear to me how you "shift" a DEM to an effective time step? There is no reference or description of the method used to do this. This, along with any error associated with the procedure should be described.

7. Can a viscous ice shelf model really accumulate stress at the tip of a rift by dissipating gravitational potential energy?

The authors argue that rift widening results in accumulation of stress ahead of the rift. The energy balance in an ice shelf model tells us that gravitational potential energy is dissipated through viscous flow. The accumulation of stress seems to imply energy is being added to the system faster than it can be removed. What is the source of the energy that is added to the system that drives energy accumulation? Is this related to torques associated with rotation of the blocks of ice? Is this conclusion supported by forward model runs or this based on tuning the model to match observations? Given the fact that rift widening is documented by GPS, it seems as though the authors should be able to do a forward model run to compare simulations with observed rift widening and use the forward model to show that stress is concentrated ahead of the rift.

8. Conclusions

This is up to the author, but there has been little doubt that "calving laws" are needed in ice sheet models and this is not the main conclusion I would draw from this study. I don't think it will come as any surprise to most readers that iceberg calving is an important process in ice sheet evolution. The authors also have a typo in their description of the so-called marine ice cliff instability. The marine ice cliff instability assumes that there is a **maximum** ice thickness, not a **minimum** ice thickness. Moreover, the marine ice cliff instability generally applies to thick grounded ice and not thin ice shelves, like the Brunt Ice Shelf. Minimum ice thickness models have been a mainstay in ice sheet models for decades largely as a means of preventing ice shelves from indefinitely advancing. Hence, the authors have a good argument that these minimum thickness criteria are not physical.

Actually, coming back to the Deconto and Pollard marine ice cliff instability study, my understanding is that the parameters used by Deconto and Pollard were derived based on parameter sensitivity studies for past sea level rise. These are, technically, observations are they not? Direct observations of ice flow of ice sheets hundreds of thousands of years ago, similar to the GPS and satellite imagery used in this study, remains a challenging problem in paleo ice sheet studies. And if the marine ice cliff instability is really a thing, the only evidence we have likely comes from past ice sheet conditions when these processes may have been active. Here, it would be useful if the authors put their results in context of past and future projections of ice sheet change. If structural heterogeneity is important, is it possible to predict it instead of tuning a model to match observations? How important is structural heterogeneity versus the geometry of pinning points? It seems like knowing the location of pinning points (which is possible) could at least provide a first order approach to rift generation even if it does not match the detailed sub-decadal trends? This study potentially offers a lot of information and it would be useful to readers to see how this fits into the bigger picture.

Minor comments:

Page 5, line 25: What is "Geometric Deformation"? Do the authors mean that the geometry of the ice shelf is changing? I actually googled this term, but all of the hits directed me to papers on differential geometry, which seems like it is not what the authors are talking about.

Page 6, line 26. Ice is in hydrostatic equilibrium. A force balance at the ice-ocean interface (analogous to that at the calving front) within a melange free rift suggests a deviatoric stress pointing into the rift. Why is the ocean pressure pulling the rift apart? The large scale stress of the ice shelf might pull the ice apart. This should be clarified.

Page 3, line 28: We were told there is 50 years of data, why only focus on the period from 1997-2018? What is the benefit of the long time series if less than half are used? The earlier emphasis on 50 years of data seems like a bit misleading at this point.

Page 1, line 20: This is pedantic, but I would consider the 5000 km^2 berg that detached from the Larsen Ice Shelf to be a small to mid-sized berg. Iceberg B15 that detached from the Ross Ice Shelf was twice as large and Shackleton documented icebergs that were even larger.

Page 1, line 20: The word "since" refers to time. For example, "It has been a long time since the Knicks won the championship." In this case, I believe you want "Because".

Page 2, line 9: The references given here document thinning of ice shelves and do not appear to describe any links between thinning and calving.

Everywhere: space between numbers and units 3m should be 3 m

Page 7, line 2: comma after "but"

Page 7, 2nd paragraph: Now I'm really confused about what is going on. Are the authors introducing a rift into the model and widening it, based on observations to examine the stress field. Or, have the authors inverted for stress (OK, actually ice softness) based on surface velocities at several intervals of time? In the first case, I think the authors are safe saying that the increase in stress is due to rift widening. In

the second case, I don't know that you can say that the stress is caused by widening when no rift widening has been included in the model and the model has been tuned to reproduce surface velocities (and hence stresses). Morever, the assumption that rift widening results in stress concentration should be checked against other periods of time when rift propagation did not occur. For example, there is a long history of rift widening without propagating prior to Chasm's reactivation. Does this period of time correspond to the rift propagating into a zone of marine ice?

Page 7, line 26: The technical jargon "damage" is introduced here. Authors should avoid the term or define it. Keep in mind that "damage" has a precise definition in the fracture mechanics literature and is, most generally, a tensor. The term damage is often used heuristically in glaciology in confusing and imprecise ways. If the authors mean rifting, I recommend just saying rifting.

Section 6, Page 8, section paragraph: Wait a minute. Why is the rate factor A(x,y) not a property of ice that advects with the ice? Conventionally, the rate factor has been linked to temperature, grain size and crystal structure of the ice. If reductions in the factor A(x,y) are linked to fractures then surely these must also advect with the ice? If the advection of the rate factor is not important, then why is heterogeneity of the ice important? I'm missing something critical here because this seems like this contradicts the authors main conclusion that heterogeneity is important.

The "extrusion" method for calving front advance is known to generate significant artifacts if not treated carefully. The calving front should advect as a sharp shock and accurate shock capturing methods are needed to avoid overly diffusing the calving front. Numerical details of advection should be included with limitations described. Does the advection scheme preserve mass? It is diffusive? Does it preserve the shock-like nature of the calving front? Are results sensitive to grid size or time step size?

Page 8, last paragraph: The statement that ice sheet models keep the calving front pinned to present day conditions might have been true a decade or two ago, but pretty

much all of the major ice sheet models at this point allow the calving front to evolve. PISM uses a wetting drying algorithm combined with "eigen calving". ISSM uses a level set method combined with a Von Mises calving law. BISICLES and CISM have their own methods to advance the calving front and use a spectrum of calving laws. These days, models allow the calving front to advance and retreat according to heuristic (and often known to be incorrect) parameterizations. Whether advancing and retreating the calving front based on inaccurate and unphysical calving laws is progress is a question that I will leave to others.

Page 12, line 12: Reference to Lipovsky, 2018b appears to reference an unpublished manuscript. Check Cryosphere style guidelines for rules on references to non-peer reviewed literature. This is prohibited by AGU publications, but the standards of TCD might not be as stringent

Figure 2-3. Best not to use a red-green color scale.