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Interactive comment on "Modelled fracture and calving on the Totten Ice Shelf" *by* Sue Cook et al.

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Review: Cook et al.,

General Appreciation.

This manuscript describes a suite of models used to describe the fracture patterns observed around Totten Ice Shelf, East Antarctica. The authors apply a discrete element models (HiDEM) a full Stokes model (Elmer/Ice) and a model meant to describe basal melt (the KPZ equation) to describe the formation and evolution of basal crevasses. The authors use the suite of models to infer that basal crevasses that initiate near the grounding zone advect to the calving front and are responsible for the checker board pattern of fractures observed near the calving front of the ice shelf. That basal crevasses that initiate upstream play an important role in calving closer to the calving front is not that surprising and is consistent with several prior studies. However, this is an interesting hypothesis—especially in light of the argument that the channel geometry is related to submarine melt—because it provides a direct connection between fractures/calving and ocean forcing and this study provides the first direct numerical simulation of some of the fundamental processes. Hence, the study has the potential to be improve our understanding of a set of processes that remain poorly understood.

Organizationally, I found the manuscript a bit confusing to follow. I think it would help readers significantly if the authors presented their hypotheses or questions earlier on in the manuscript. On my first reading, it seemed as though the authors were merely throwing a couple of unrelated models at a problem without any underlying reason or rational. It wasn't until near the end of the manuscript that I realized that the authors were testing the hypothesis that crevasses near the grounding line control the fracture pattern near the calving front. That the authors need the basal crevasses is interesting and should be emphasized to readers earlier on. Here, I think the authors could significantly improve the impact of the manuscript by adding section subtitles and sketching out the logical flow. In other words, it would be helpful to see that the authors are trying to explain the fracture distribution near the calving front of the ice shelf. This could either be presented as a fundamental question early on (i.e., basal crevasses are needed to explain the fracture pattern), or complexity could be introduced into the suite of models expressly to match this fracture pattern (i.e., the HiDEM simulation without basal crevasses could not represent the fracture pattern). Without this additional text, some of the models appear unmotivated. I do also have some questions about the models and assumptions, described in more detail below.

Questions about models.

Overall, I think that the hierarchy of models is interesting. I do have a few questions about the models. For example:

-How many particles are used in the HiDEm model runs. What is the vertical number of particles? More details on the numerical setup would be helpful to readers.

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Discussion paper

-How are the basal crevasses/channels used in the last simulation specified in HiDEM? Are these based on observations? Providing this data is critical.

-What ice temperature is used in the ELMER/ICE simulations? This should be important in determining the rate of crevasse widening. I'm also curious about the effect of the strain rate on widening. The strain rate is sufficient to allow crevasses to widen, but I would expect crevasses to actually close if the strain rate is smaller. This would be interesting to check.

-Similarly, I would have liked to see some more quantitative comparisons between observations and data if feasible. For instance, how wide are the fractures predicted by HiDEm and do they penetrate the entire ice thickness? It is difficult to detect if fractures penetrate the entire ice thickness using surface observations, but one could estimate the width of fractures (based on pixels) and compare this to the predicted width of fractures from HiDEM. In fact, it would be really nice to have a statical comparison between length, width and orientation of fractures predicted by HiDEM and those observed in the images. This would significantly bolster the authors claims and avoid the need to rely on qualitative inspection of figures to determine how well the model is performing. This is important for a range of reasons, but it is important to recognize that the iceberg size distribution is modified by interaction with the ocean (bergs melt, erode, tip over, etc.) and hence the comparison between berg sizes is not necessarily a one-to-one comparison with bergs that detach. Fracture distributions should be more straightforward to map and compare.

A few other quantitative questions I had about data included: How many total icebergs were detached in 2009, 2010 and 2011 respectively and how many icebergs in each size class were detected? What time of year are these images? Is there a bias associated with the time of year? Furthermore, it is possible that the distribution of icebergs/calving rate is not stationary stochastic. If this is the case, then the distribution of icebergs would not be stationary stochastic, as the authors assume.

KPZ equation The use of the KPZ equation in this context is interesting and novel. I have played around with it a few times for various things. My understanding of the usage of the KPZ equation here is the ν, λ and F (which includes two terms to allow for a linearly varying profile) are all adjusted to match observations. This means that the authors have 4 free parameters to adjust. From looking at Figure 4, it also looks like the position, width and initial depth of the crevasses are all also determined as part of the solution. There is an old saying that given three parameters you can fit an elephant. Given 5, you can make its trunk wiggle. Although, I find this a bit extreme, I do think it is worth discussion the robustness of the results in the face of 7 unconstrained parameters. This should start by listing the numerical values of parameters used. Are results sensitive to order of magnitude changes in parameters? Factors of two changes? I also have questions about the fact that, as far as I can tell the melt rate (F) is not related to ocean forcing at all. I guess this is just an empirical constant, but the KPZ model is of limited utility if we cannot relate it to ocean forcing. At the very least, we have observations of the melt rate and hence, can constrain the average value of the melt forcing term. Similarly, the time at which the simulation result is obtained is not given. How long does it take for these channels to evolve and how different do the channels look at different instances of time. This is crucial because the KPZ equation does not necessarily admit a steady-state—similarity solutions exist that allow for continuous variation. If the solution is steady-state, then this should be declared along with how long it takes the simulation to arrive at steady-state.

As I mentioned, I think the use of the KPZ equation here is interesting and novel. However, it appears as though the KPZ equation as listed in Equation (4) has an error. The actual equation should read:

$$\frac{\partial h}{\partial t} + u \cdot \nabla h = \nu \nabla^2 h + \frac{\lambda}{2} \left[\nabla h\right]^2 + F + \eta. \tag{1}$$

where u is the velocity along the interface. There is no physical reason to omit the advection term. Typically, the KPZ equation is used for materials where the interface does not have a background velocity (as in the refs cited) and hence, one can replace the material derivative with the partial derivative with respect to time, as the authors seem to have done. In this case, however, the interface is moving with the background velocity of the ice and there is a gradient in the velocity. If the “fractures” are indeed enlarged by a constant melt rate, then including the advection term should allow the authors to reproduce the spatial pattern of fractures using a constant melt rate F instead of a linearly varying melt rate. Now, because velocity is not Galilean invariant one need only specify the difference in velocity across the domain. One could do this in a consistent manner with the Elmer/Ice model by using the same strain rate across the domain.

There are other, subtle, issues associated with the use of the KPZ equation in this context. One prominent assumption of the KPZ equation is that the erosion of the interface does not depend on the absolute elevation of the interface. This is the reason why only scalar powers of ∇h appear in the expansion. In the case of submarine melt, however, this assumption is violated because of the pressure dependence of the melting point. Erosion of ice should occur more rapidly deep beneath the surface of the ocean and erosion (or even marine ice deposition) should occur closer to the ocean surface. As a consequence, I'm not even sure that the *sign* of the $[\nabla h]^2$ term is right. Expanding on this point, the KPZ equation is useful for describing interface evolution because it represents a large universality class of processes that, irrespective to process details, ultimately behave like the KPZ equation. The universality class, however, is not infinitely large. Here, I think the authors should attempt to establish whether submarine melt actually resides in the broader universality class exemplified by the KPZ equation. Failing this, I would really like more detail on **why** the authors think the KPZ equation is an adequate description of the submarine melting process and the role of the pressure dependance of the melt point in this argument.

Finally, I was confused about the role of the Elmer/Ice and KPZ simulations of basal channel evolution. Why not incorporate the KPZ equation into the ELMER/ICE simulations so that the authors can self-consistently simulate the strain induced widening and erosion? If this isn't feasible, then what are we supposed to learn from the two simulations? As far as I can tell, the Elmer/Ice simulation tells us that crevasses widen and become shallower over time, but the Elmer/Ice simulations omit ocean forcing. In contrast, the KPZ simulations tell us that crevasses become wider and deeper over time, but this simulation omits any ice dynamics (included advection!). Do these effects cancel out? Does one dominate? Do we need both or can we omit one of them? I do wonder if the widening by ice dynamics alone is sufficient to explain the width of observed basal crevasses. If it is not sufficient, then this argues that erosion by submarine melt is necessary.

Detailed comments:

Page 3: "We also use a continuum ice flow model and a stochastic equation describing fracture development" This is unclear. It seems as though the KPZ equation is used to model the evolution of the interface in response to melt. The KPZ equation is indeed stochastic. However, the authors omit the stochastic terms, at which point version of the KPZ that they use is entirely deterministic . . . Also, the authors claim η is a stochastic term, but then they set it equal to the initial width/length of basal crevasses. This seems to confuse initial conditions with the stochastic forcing. Typically, the stochastic term in the KPZ emerges as a white noise Gaussian process, which is quite different from the assumptions made here. It might be less confusing to separate this into an initial condition and noise term with the noise term set to zero.

This is a minor point given the fact that the stochastic term is set to zero, but stochastic partial differential equations fall into two categories the so-called Ito and Stratonovich interpretations. These two interpretations are subtly different in important ways, but necessary to place stochastic differential equations on firm mathematical footing. What interpretation is assumed here?

Page 4: How big of a difference does it make to results if a different percentage of bonds is broken? Say 0.1% or 10%?

Page 4 last sentence after equation 3. The constant “c” is apparently dimensionless (at least no units are given) and thus friction does not have the correct units. Either “c” should have units or units of friction needs to be more carefully explained (have the equations themselves been non-dimensionalized)?

Figure 2: Am I missing something? Fractures look like the run both parallel and perpendicular to the front?

Figure 3 is hard to decipher. Is it possible to show Figure 3 in the same style of as Figure 2 so that we can see where the crevasses are located on an image?

Figure 6 is very pixelated (maybe a conversion issue?) and hard to interpret.

Figure 7. It doesn’t look like the simulation obeys a -3/2 scaling law. According to the red curve, the computation predicts more icebergs at low iceberg area and not enough at large iceberg area. The observations are really only power-law over about an order of magnitude. Fitting power-laws to such a limited data range is fraught with peril. This maybe an example where it is better to avoid a logarithmic plot AND to include error bars in the observations. How many icebergs are there with sizes close to 10^6 m^2? Is this actually statistically significant?

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