The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2017-166-RC2, 2017

The Cryosphere Discussions

Interactive comment on "Effects of undercutting and sliding on calving: a coupled approach applied to Kronebreen, Svalbard" *by* Dorothée Vallot et al.

Anonymous Referee #2

Received and published: 4 October 2017

1 Summary statement

The manuscript by D. Vallot et al. studies the ice front evolution during the melt season of Kronebreen glacier, Svalbard. The authors use a global approach that combines different models computing ice flow evolution, surface mass balance, subglacial hydrology, plume model, undercutting model and fracture dynamics, to assess the calving of this glacier. They study its seasonal evolution and compare modeled results with observations. They also investigate the impact of several parameters on the location and rate of calving, and conclude that the glacier geometry controls the location of calving, that the basal sliding controls its rate, and that undercutting is necessary to reproduce observations.

This manuscript presents an interesting study that not only focuses on one aspect of calving (e.g., fracture, undercutting) but also combines many different processes that are known to affect calving. Modeled results are compared with observations, and experiments are performed to assess the influence of several parameters. However, the manuscript is not always clear, and seems to have been put together in a rush. There are inconsistencies between some variable names (e.g., $C(t_i, \beta_i, u_i)$ and $C(g_i, \beta_i, u_i)$), and symbols that are used for two different variables (e.g. u for both velocity and undercut). It therefore needs some major clean up to be clarified and ensure consistency in notations (see detailed remarks below). Furthermore, some choices made are not explained or justified. This is the case for the reduction in basal friction for the discrete particule model, or the decision to keep or remove the undercut depending on the ice front location relative to the previous position and current observation. These decisions are not physically justified, and the impacts of such choices are not studied. Finally, some conclusions are also not well supported by the analysis. One important result is that “the geometry controls the calved zones while the basal friction (glacier dynamics) controls the magnitude of calving (calving rate)”. However, the impact of the basal friction on Fig. 10 (a and c only show the impact of friction) is not clear, as the calving rate is not very different for the high and low friction scenarios, and adding undercutting (Fig. 10c) has an impact similar to changing basal friction. So if these three parameters (geometry, basal friction, and undercutting) have an impact on calving, more experiments are needed to conclude on the specific impact of each individual parameter.

2 Major comments

The title of this paper and subsequent references to “coupling”are misleading. There is no coupling performed in this model. The different models used to represent the calving front processes are put together and outputs from one model are inputs of other models, but there is no coupling. Fig. 2 illustrates this very well: arrows all have the same direction and outputs from HiDEM are never used as inputs for other models. What this paper does is provide a comprehensive approach to the question of calving, and I think a title using “global approach” or something similar would be more accurate.

As mentioned above, the conclusions separating the impact of geometry, basal sliding and undercutting are not well supported by the results provided. Looking at Fig. 10, it seems that all parameters have an impact on both the location and extend of retreat, but they cannot be clearly distinguished without further experiments.

It is not clear if all the 11 time steps described in Tab. 1 are modeled, or if only a subset of these times are used. Results from t_0, t_4, t_6 and t_{11} are mostly presented, but Fig. 10 also shows results at different time steps.

What is the rational for keeping or removing the undercut in one case or another when the ice front advances or retreats (Fig. 4 and p.9)? Some explanations justifying these choices should be added as opposed to presenting the choices made without any justification. I cannot quite figure out why the undercut from the previous profile is not always considered.

3 Line by line comments

p.1 l.17: “rigorous methods”: the problem is not so much about rigorous methods but more about some processes impacting calving that we still don’t understand, as well as small scale features (mm long cracks) that cannot be observed and included in models.

p.1 l.20: “impacting on submarine melt rate” → ‘impacting submarine melt rate”

p.2 l.1: “during the summer and the autumn” → “during summer and autumn”

p.2 l.3: “followed by ice-front collapse”: not clear

p.2 l.12: The problem is actually not so much the representation of calving in models but the processes impacting calving that are not enough understood and therefore cannot be included into models.

p.2 l.16: Again here, it is not really coupling but feeding the particule model with appropriate inputs from Elmer/Ice.

p.2 l.32: “one of the fastest” → “one of the fastest glacier”

p.2 l.33: How much seasonal variation is this glacier experiencing?

p.3 l.3: How large are the seasonal variations? What is the velocity in winter?

p.3 Fig.1: Consider adding Kongsfjorden on the figure. Calving front position for 16 Octobre 2013 is not visible, consider changing it.

p.3 l.10 “by (Nahrgang et al., 2014)” → “by Nahrgang et al. (2014)”

p.3 l.11-15: Past tense should be used to describe measurements made in 2013.

p.3 l.11-15: How representative of the seasonal cycle are these values?

p.4 Fig.2: There is no feedback and therefore no coupling shown on this figure. Outputs from one model are used as parameters/inputs for the next model.

p.4 l.7: What about the other observations (geometry, ice temperature and viscosity)? Where do they come from?

p.5 l.6: Is the sliding inverted just at the beginning of the simulation or recomputed for each time step? In this case how is the change in the glacier geometry computed (or maybe observations are used)?

p.5 l.9: How are the front and surface evolved? Are they run from the previous iteration and therefore the 12 time steps are run with the model? In this case, why only show results for 6 cases and not the entire melt season? If not, how are the front and surface evolved? Also, modeling ice front changes in ice flow model is not an easy task and is currently the subject of active research. How is the front evolved with the Elmer/Ice model? There is no reference or explanation of how the ice front migrates and no Elmer/Ice paper describing such an evolution to my knowledge. This has to be better explained.

p.6 l.1: "coupling": same as above

p.6 l.1: If the front position is not used as inputs for the Elmer/Ice initial front position, what is used then?

p.6 Eq.1: Consider using vectors. Also u is used both here for the velocity, and later (e.g. Tab.2) for the undercutting. Change one or the other.

p.6 l.8: Again here, is the friction optimized at each time step or just at the beginning of the simulation?

p.6 l.9: "the self-adjointness" $\rightarrow$ "a self-adjoint algorithm"

p.6 l.10 and l.11: Consider adding older references that first used such methods.

p.6 l.13: This paragraph could be put in the data section (section 3.1) to improve consistency.

p.6 l.20: "The front position is also able to advance": How is it able to advance? See point above

p.6 l.20: "$F_i^{obs}(0)$": I would imagine that observations show the front position on the surface of the glacier and not at sea level.

p.6 l.20-21: There are several front positions observed and computed. The authors should start by listing all the front position computed (F^{elmer}, F^{HiDEM}, ...) and explaining where they come from. That might be something to add on Fig.2.

p.6 section 3.3: What is the resolution (horizontal and vertical) of the model, especially close to the ice front? What are the time steps used for the continuum model?

p.7 l.8: convention for the reference (twice)

p.7 l.29: "five kilometers" → "five kilometers away"

p.8 l.1: How long does it take to reach a steady-state?

p.8 l.4-7: So my understanding is that the discharge varies but not the ocean conditions. Ocean conditions are reported quite accurately on p.3, so why not use these conditions instead of uniform ambiant ocean properties? Also, in all these cases, the ice front is assumed to be vertical, why not try cases with pre-existing undercutting? I understand that it might not be possible to test all these cases, but at least assessing the uncertainty caused by such assumptions would be important.

p.9 l.6-10: What is the rational for keeping or removing the undercut in one case or another? Some explanations justifying these choices should be added.

p.9 l.19: How many broken beams are added and how was this number chosen? What is the impact of increasing or reducing this number on the results? Also does the number of broken beams increase during the melt season as the ice gets more damaged?

p.9 l.30: How long is the HiDEM model run for at each time step? And how long does it take to run it?

p.10 l.2: What kind of instabilities are developing and why?

p.10 l.7-11: What is the rational for decreasing the friction? How is the choice of friction impacting your results?

p.10 l.19: It should be mentioned that this is volumetric ablation rate (same for volumetric calving rate in the rest of the paper). Many people use calving/ablation rate as changes per unit area (in m/yr), which can be confusing.

p.10 l.20: Integrals over $Gamma$ usually refer to contour intervals and not surface integrals, using S or Σ instead would be more consistent with literature.

p.10 Eq.5: What is $z\Gamma_w$?

p.10 l.28: “parameterisations” $\rightarrow$ “parameters”

p.10 l.30: u was already used for velocity (see above)

p.10 l.30: Only 4 time steps are mentioned here. What happens to the other ones, are they just excluded? In this case, what is used for the prior undercut?

p.11 l.1: Only a subset of $(i, j) \in [0, 4, 6, 11]$ is covered, not accurate.

p.11 l.6: configuration C_k is not defined and not used anywhere else, should be consistent with the rest of the paper

p.11 Tab.2: Configuration is here a function of time (t_i) as opposed to geometry (g_i) in the rest of the paper

p.12 Fig.6 caption: “data gaps corresponds” $\rightarrow$ ‘data gaps correspond”

p.13 l.Tab.3: Discharged should be provided in m^3/s to be consistent with the rest of the text. No data between t_{10} and t_{11}, this should be added even if the values are just zero. Also, how are the melting rates for each case computed based on Fig.7? Is an interpolation between the four cases been performed? Or something else?

p.13 Fig.7: How different are the results if there is undercut introduced in the geometry?

P.14 Fig.8: It is the only time in the paper, where results from times other than t_0, t_4, t_6 and t_{11} are presented. Are the other time steps computed? And what is the rational to only present some ice front positions here?

p.14 Fig.8: If z is the height above sea level, Fig.8 b and c are for $z = -3m$ and $z = -42m$, and the stars in Fig.8 d and e indicate the plan view elevation, why are the start not aligned at the same height on Fig.8 d and e? Also it might be more clear to use "Elevation from sea level" or something similar instead of "Distance to the bed" on Fig.8 d and e as this is what is used in the rest of the figure.

p.14 l.11: What do the authors mean by "smoother"?

p.14 l.12: "stretching" → "stretches"

p.15 l.2: Why not do it more often? How long does it take to run the model?

p.15 Tab.4: "modelled" → "estimated": the melt is estimated from observations, not modeled. What is $\dot{a}_m^{obs}$? Also add zero values where appropriate instead of leaving empty spaces.

p.16 Fig.9: $\dot{a}_c = \dot{a}_{c,u} - \dot{a}_{c,L}$ on p.15 (minus not plus), so it's not clear what is shown on this figure.

p.19 l.5: "Due to this, there is some uncertainty" → "There is therefore some uncertainty"

p.19 l.7: Add references (or something else) to justify this statement that is not supported.

p.19 l.24: Is that what is observed by the authors?

p.20 l.1: "no retreat at all": this is not supported by the model results, there is some retreat in the southern part of the domain.

p.20 l.9-10: I don't think that the experiments made and results support such a conclusion, the role of sliding and geometry cannot be clearly separated.

p.20 l.19: "the velocity higher" → "the higher velocity"

p.20 l.19: "seem" → "seems"

p.20 l.30: “reproduces” $\rightarrow$ “reproduced”

p.21 l.12: “in 2D” $\rightarrow$ “with a simplified 2D geometry”

p.21 l.14: How do the melt rates compare to previous results?

p.21 l.23: What is referred to as the calving model?

p.22 l.5: “would be implemented” $\rightarrow$ “were implemented”

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