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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 #1

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The Cryosphere TC2017-166 *"Effects of undercutting and sliding on calving: a coupled approach applied to Kronebreen, Svalbard"* by Vallot and others.

A suit of 6 models is used all together to study the effect of undercutting and sliding on calving using the dataset of Kronebreen in Svalbard: (1) the surface mass balance and surface runoff are obtained from a coupled energy balance - snow model ; (2) water is routed through the glacier front using a basal hydrology model assuming zero effective pressure ; (3) at the front, melt distribution is inferred by plume model using the code Fluidity ; (4) undercutting is accounted by transforming the front surface,

initially vertical, according to the plume melt distribution ; (5) ice velocity, isotropic pressure and the glacier geometry are determined from the Stokes equation using the Elmer/Ice code ; and (6) calving is determined using a discrete particules model (HiDEM). All the models are not fully coupled and more often used in a one-way coupling.

One of the main conclusion of the paper is that calving rate is controlled by basal sliding. I can see two problems in the methodology that question the validity of this conclusion. First, the friction coefficient inferred using inverse methods with Elmer/Ice has to be scaled down by "some" orders of magnitude when used with HiDEM. I didn't understand the justification regarding the different time scales of calving and sliding processes to justify this scaling. The "some orders of magnitude" should be quantified. Is it a constant for the whole domain? Is it the same value (give it) for all simulations? This should be explained much more precisely. Second, calving rate is a continuous view of calving, as calving is discrete and a calving rate can only be inferred when averaging a number of calving events during a given time. Here, it seems that calving rate is inferred from one simulation of HiDEM and I then suppose that it is inferred from one calving event? Or a limited number of calving events arising during a very short time? Can we deduce a calving rate from that, and then conclude that calving rate is very sensitive to basal sliding? Also, it should be worse verifying that the result are not too strongly dependent on the time step in between two HiDEM simulations. How different are the calving rates obtained by running the HiDEM model every dt, $dt/2$, $dt/4$ time step of the Elmer/Ice model?

Minor remarks:

page 2, lines 12-15: The fact that it is untested against observations certainly also apply to the particule models (or you should give a reference in which the particules model is validated against data). The distinction between continuous and discrete approaches could be a bit more rigorous and objective. There are also some drawbacks in the particules model that will anyway render its use very difficult for large or long applications.

page 2, line 22: with the discrete element model HiDEM

Figure 1: what are the different colours? Especially the white versus grey?

page 3, line 10: as shown by Nahrgang et al. (2014) presenting (there are similar problems with the use of brackets for the references all along the manuscript. Please, check this).

Figure 2: should be Elmer/Ice not Elmer/ICE to be consistent with the text.

Table 1: give in the first column the number of day $t_0 = 0$, $t_1 = 11$ d, etc...

page 6, lines 1-2: here you are mentioning the one-way coupling between HiDEM and Elmer/Ice, and then saying that a completely coupled model would also couple the hydrology and the ice flow. But to be completely coupled, you should add the coupling with the plume model? I would suggest to modify the transition: *Also, an improvement could be to calculate the friction...*

page 6, line 20: in this part it should be clearly mentioned what is making the front position advance or retreat. Which equation is solved for the front position? Is it a similar to equation (2) and therefore the front is moving as a balance of ice flow and front melting?

page 7, line 15: on which Grid? The finite element one? Why not solving equation (3) using the finite element method?

page 7, line 19: I cannot understand what you mean by this last sentence... what is flow accumulation?

page 8, line 10: that sea level corresponds to $z = 0$ is already mentioned above.

page 9, line 5 and after: this part is not clear. What are the reasons for these 3 different treatments should be explained.

page 9, line 11: again, same remark as above: which method? Are you solving a free surface evolution equation for the front? Is the Elmer/Ice model time step also 11 days? This should be specified somewhere.

page 9, line 27: that varies spatially according to the inversion done using Elmer/Ice?

page 9, line 32: see my main point. It clearly questions the fact that a calving rate can be inferred from this approach? What does it change if you run HiDEM every two (or half) timestep?

page 10, line 4: which complication? As for the "some orders of magnitude", the explanation should be more precise.

Table 2, last line fisrt column : $C($ instead of $C_($

Figure 5: Downstream the front, one would expect zero friction? Are the value on this plot extrapolated? Should be mentioned.

Figure 6: I would expect that the discharge increase along the water path and I don't see this from the plot. On (b), the axis for SD should also start a 0 (and with a continuous curve going to 0 for no discharge).

Table 3: how the sum of SD and ND volume compare to the integrated runoff over the basin?

Figure 8; what represent the horizontal thin lines in the ocean?

page 15, line 6: To my understanding, a_c^{obs} doesn't include only calving but also melt at the front? So, it should also be mentioned.

Table 4: it should be a_m and not a_m^{obs} in the table? In the legend, I am a bit confused by what you call the tangential ice velocity (tangent to the front?). Isn't it the velocity normal to the front that you mean here? Same in the legend of Fig. 9.

Figure 11: As you mention in the text that Fig. 11 shows strain rates that ressemble crevasses pattern, would be nice to have an aerial image of the real crevasse pattern? How do you explain the very similar patterns for all simulations inside the domain? What drive these features? And why choosing to plot strain-rates when you could directly plot places where bounds are broken?

page 20, line 1: regarding the key role of basal friction, see my main comment

page 22, line 7: Elmer/Ice

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