The Cryosphere Discuss., 6, C1972–C1980, 2012
www.the-cryosphere-discuss.net/6/C1972/2012/

The Cryosphere Discussions

TCD

6, C1972–C1980, 2012

Interactive Comment

Interactive comment on "Mechanical effect of mélange-induced buttressing on embayment-terminating glacier dynamics" *by* D. Seneca Lindsey and T. K. Dupont

D. Seneca Lindsey and T. K. Dupont

dlindsey@uci.edu

Received and published: 26 October 2012

We would like to thank the referee for their thorough and thoughtful comments. We will reply in detail to address their concerns regarding this work. For clarity we'll quote the appropriate text from the referees comments, and then provide our response for each point.

1 Regarding merit

The referee states that:

"The point is that if one wants to go further, which is the aim of this paper, and try to quantify the real effect of ice mélange (which advance of the front can be expected, which seasonal variation of calving rate,. . .), the proposed model is by many purposes not adapted to this objective."

We agree that this model is not well adapted for predictively forecasting the seasonal advance of the ice front or seasonal variation of calving. In the revised paper we will make clear that such an attempt would require a system that captures a myriad of important factors such as ocean melt, strain history, and ice-front geometry. The goal of this process-level heuristic model is much more modest. Here we take a first step and assess the potential for mélange to affect calving rate through direct suppression of longitudinal strain-rate under specified conditions. Furthermore, we describe the character of the system response for a wide spectrum of mélange thicknesses. Our results address assumptions made by others that mélange does not significantly affect the force balance of the ice front (Reeh et al., 2001; Joughin et al., 2008).

2 Regarding thickness use for the mélange

The referee states that:

"Ice mélange is a composite material made of icebergs, parts of icebergs and sea ice. Even if there is not that much observations, ice mélange is certainly a very

heterogenous material, and at the zero oder, this heterogeneity can be described by the mélange thickness heterogeneity. It is well known that sea ice, because of its very low heat conductivity, starts growing very slowly after few meters, so that sea ice thickness stays below ≈ 3 meters. Even if small in area, sea ice is therefore the weak part of the ice mélange, and therefore determines the maximum resistance that can be expected from ice mélange. If using a continuum approach, as in this paper, the ice mélange thickness should then be set to a value much smaller than the ≈ 200 meters used here. In other words, the study should only discuss results obtained with mélange value lower than 0.3%, two orders of magnitude smaller than the value used here for the discussion."

We acknowledge the heterogeneous nature of mélange and the referee's point is well taken that if sea ice exists within the mélange it is likely to be on the order of meters or less. We are arguing that the weakest point in mélange need not necessarily be sea ice. While initial open areas of sea water within the mélange will fill with sea ice, we speculate that many of the larger bergs may simply collapse into these areas, forming densely packed bergs where the weakest point becomes the contact surfaces between bergs. The role of sea ice may simply be to stabilize bergs in the mélange in a similar process to that described in Amundson et al. (2010). Regarding mélange thickness at Jakobshavn Isbræ, an image from Amundson et al. (2010) shows mélange thickness matching the vertical extent of the ice-shelf front. Moreover, this image shows densely packed bergs with no observable sea ice. In addition, measurements of mélange thickness in crevasses by Fricker et al. (2005) in the Amery ice-shelf show mélange thickness of $60\ m$ for a shelf that is roughly $300\ m$ thick, a 20% ratio. Our value of $200\ m$ mélange thickness relative to $1100\ m$ shelf thickness is consistent with these observations, and conservative when compared to the image at Jakobshavn Isbræ.

3 Regarding calving law and other processes

The referee states that:

"The calving law used in this model is only phenomenological and is not enough physical to describe the mechanisms at the root of calving rate decrease for decreasing ice mélange weakness. For a realistic ice mélange thickness (few meters), the calving rate decrease induced by the ice mélange obtain from this law would just be insignificant. A more realistic law for calving should be used or formulated for this purpose. But in any case, it cannot justify the use of an ice mélange thickness of few hundreds of meters. Moreover, the authors don't discuss the other potential seasonal processes that can decrease calving rate: there is less water in crevasses during winter, air temperature is lower, . . . Ice mélange is certainly not the only contributor for the decrease of calving rate during winter, and other processes should be accounted for, or at least discussed. Ice mélange has certainly not only the effect of decreasing the strain-rate. It might also decrease the influence of tide and oceanic currents, and changes the sea water circulation at the calving front, leading to at least three other potential ice mélange induced processes that can contribute for a decrease in calving rate. This should be at least discussed in the paper."

The scope of this work is to seek an upper bound on the ability for mélange to mechanically suppress longitudinal strain rate rather than examining all factors that influence calving. The calving law we use is admittedly heuristic in nature, though based on observations taken at a wide range of ice shelves. The data used to formulate the parameterization incorporates processes mentioned in the referees comments and thus implicitly accounts for these processes. We agree the influence of these processes may be important, and leave further examination to future publications. We will clarify the limited scope, and the missing or implicit processes, in a revised manuscript.

TCD

6, C1972–C1980, 2012

Interactive Comment

4 Regarding boundary conditions and sea water pressure

The referee states that:

"I think the model needs five boundary conditions and not four. The hydrostatic water pressure has to be applied on the part of the ice-shelf front below the ice mélange, and not only at the front of the ice mélange. There is clearly a missing force in the system if this pressure is not accounted for. The response of the model for a very small ice mélange thickness should be equivalent to the flow of an ice-shelf without ice mélange. Did you check this? In total, whatever the ice mélange thickness, the resultant force exerted by the sea water pressure on both the ice-shelf and the ice mélange fronts should be the same. So, a modified form of Eq. (5) has to be applied at the shelf-mélange transition.
I did try to reproduce the 6 km/a velocity at x = 7.5 km with a SSA model. With a 7.5 km long ice-shelf without ice mélange, I get a much larger velocity. I get a 6 km/a velocity at x = 7.5 km only for a 14 km long ice-shelf. My feeling is that you applied Eq. (5) only at the ice mélange front but with h = 1100 m whatever the ice mélange thickness. I think this is wrong, and especially when looking for the strain-rate transmission from the ice mélange to the ice-shelf."

In revision, we will make it clear how the ocean pressure is being applied to the ice-shelf front. Assuming hydrostatic equilibrium, there are two instances where ocean pressure becomes important, at the mélange/ocean boundary and at the ice-shelf front. At the mélange/ocean boundary we use a traditional dynamic boundary condition as described by eq. 5 in the paper. At the ice-shelf front, the ocean pressure is handled naturally by application of the MacAyeal-Morland equations across the interface. If our ice-shelf front was a vertical face, then the ocean pressure would not be applied without adding an additional force, as the referees comment suggests. However,

TCD
6, C1972–C1980, 2012

Interactive Comment

here the transition zone is not a vertical face and only approximates a vertical face with mesh refinement. In this way the MacAyeal-Morland equations apply the ocean pressure as the x-directed projection of the ocean pressure. This is handled naturally during the vertical integration of the Stokes equations appropriate for ice-shelf flow. To see this we may decompose the non-zero driving stress in the transition zone as: $\rho_i g h \partial_x z_s = \rho_i g h \partial_x h + \rho_i g h \partial_x z_b$ where z_b is the depth of the ice-front bed. The $\rho_i g h \partial_x z_b = -\frac{\rho_i}{\rho_w} \rho_i g h \partial_x h$ term is the x-directed ocean back-pressure contribution to the driving stress from the basal boundary condition. Part b of our schematic diagram in figure 1 does not show this transition zone and we'll happily revise this image to better illustrate the model.

It is apparent from the referees comment that they have set the ice-shelf length to be $7.5\ km$, when it is listed in our paper as $15\ km$; we will endeavor to make the correct value clear in revision. Even so, we tried to reproduce the referees results using an ice-shelf model with no mélange and a traditional dynamic-boundary condition at the ice-shelf front (eq. 5 in the paper). We get a centerline terminus velocity of $6062\ ma^{-1}$ for an ice-shelf length of $7.5\ km$. In our model we use an ice shelf of $15\ km$ length, using the same model we get a velocity of $6165\ ma^{-1}$. This velocity is within 2% of the $6157\ ma^{-1}$ velocity given in the paper for mélange free summer condition. Reproducing the experiment for a shelf length of $14\ km$ we get a velocity of $6163\ ma^{-1}$. It is unclear to us why the referee's results should differ, what boundary conditions are being using to obtain these results?

5 Regarding other minor remarks

"End of page 4126: why not looking at the limit case with no ice mélange by only modelling the ice-shelf alone?"

As discussed above, modeling the mélange free condition with an internal boundary gives a centerline terminus velocity within two percent of the velocity found by letting the ice shelf occupy the full domain and applying the ice/ocean pressure imbalance as a boundary condition. For consistency and because it doesn't diminish results significantly, we use a domain that includes ice and a near zero mélange thickness. We would be happy to provide a figure that shows convergence of our results to the theoretical spreading rate with mesh refinement.

"Page 4127, line 21: I'm wondering how strong is this hypothesis since you are observing strain-rate at the front which is less than seven ice-thickness far from the inflow boundary (and the width of the domain is of the same order). If there is friction on the lateral side, the horizontal velocity in the flow direction is not expected to be laterally constant."

The referee is right that there is no reason to expect the inlet velocity to be laterally constant. Examining the plan-view velocity field and looking at the ice-front velocity, the lateral gradient in x-directed velocity dissipates within a few kilometers of the ice front. In this way a lateral gradient in x-directed velocity for the inlet velocity will disperse before reaching the ice front. We would be happy to provide a figure of the plan-view velocity that illustrates this point. Our choice for inlet velocity is the simplest, and we see no obviously better suited alternative.

"Page 4127, line 22: a non linear friction law for the x-directed flow... f is not defined and should appear somewhere in Eq. (1)? Is the value of B_s vertically integrated or is it integrated afterward in the equation? Since you gave no units for B_s, I couldn't check."

We regret that we didn't add units for B_s in table 1. Appropriate units are given as $Pa\ s^{1/3}\ m^{1/3}$. In revision, we'll be happy to restate the inlet and embayment wall boundary conditions in numbered-equation form to be consistent with the other boundary conditions and give greater clarity. The embayment wall boundary condition can be more succinctly stated as $\tau_{xy} = B_s u^{1/n}$ which is the depth-averaged marginal shear stress.

"Table 1: H and not h. Units should be given for B_s. The inlet velocity is denoted u_o in the text. I think the exponent 0.96 in the units of c_a should be 0.98."

Using H, instead of h, would be consistent with out notation of L and W for length and half width, however, we prefer to leave the thickness as h because this is also consistent with other spatially variable parameters. We thank the referee for pointing out the inlet velocity in the table; we will alter the table to make our notation consistent. We thank them as well for pointing out the units of the calving coefficient. The correct units are given by: $\frac{m}{a} = c_a(m \cdot m \cdot \frac{1}{a})^{0.98}$ leading to $c_a = \frac{m}{a}\frac{a^{.98}}{m^{-1.96}}$ resulting in units for c_a of $m^{-0.96}a^{-0.02}$. We will revise table 1 to reflect the correct units.

"Fig. 1, bottom: one can see on this figure that the ice shelf front is partly in contact with the ocean and thus a water pressure has to be applied."

We agree with the referee. If our model was implemented as this schematic diagram shows we would indeed need to add the ocean back-pressure as suggested. As mentioned above we will correct part b of the figure to illustrate the transition zone that is shown in part a of the same figure as well as described in the text.

"Fig. 3, label should be calving rate and velocity."

TCD
6, C1972–C1980, 2012

We agree, and will revise the figure.

“There are clearly missing references. The author should present more deeply what is ice mélange from previous publications (e.g., Fricker et al., 2005 ; Herdes et al., 2012) and recent works on calving law (e.g., Amundson and Truffer, 2010 ; Levermann et al., 2012).”

We agree the scholarship can be expanded, particularly in the introduction where a broader discussion of calving would be useful. Fricker et al., 2005 will be useful for discussing observed mélange thickness.

Interactive comment on The Cryosphere Discuss., 6, 4123, 2012.