The Cryosphere Discuss.,
doi:10.5194/tc-2016-60-RC3, 2016

The Cryosphere Discussions
Open Access
EGU

Interactive comment on "A simple equation for the surface-elevation feedback of ice sheets" *by* A. Levermann and R. Winkelmann

Anonymous Referee #1

Received and published: 3 May 2016

General Comments

This paper shows an interesting way of deriving the melt time of the Greenland Ice Sheet for different warming levels using a very simple approach based on three observable quantities: the equilibrium-line altitude (ELA), the atmospheric lapse rate and the melting sensitivity of the ice surface to temperature. The most interesting result is that the derived decay time quantitatively reproduces the range given by existing process-based numerical simulations. This study is relevant in the current context of Greenland Ice Sheet mass loss.

However, the approach suffers from several drawbacks that we detail in the Specific Comments below, especially the non-applicability of the decay time equation if dynamic discharge is taken into account, the lack of experiments to confirm the results given by the proposed equation, the lack of connection between different sections of the paper and the poor discussion. Therefore, we advise the authors to revise the paper either for providing a more substantial analysis of their work or for summarizing their results into a brief communication.

Specific Comments

1. The decay time equation proposed here does **not take into account ice dynamics**, as the authors state in section 5. However, a number of studies have shown that, even if the contribution of the dynamic part in Greenland ice loss seems to be less important than surface mass balance (SMB) changes, it is still quite substantial. One of the most recent modeling studies about this topic (Furst et al., 2015, TC) shows that 40% of the recent loss (2000-2010) is due to an increase in ice dynamic discharge (60% is due to SMB decrease). In terms of projections, using a 3D higher-order model with climate anomalies coming from 10 AOGCMs forced by the four RCPs climate scenarios, Furst et al. (2015) conclude that the sea-level rise of 1.4 to 16.6 cm by 2100 is predominantly caused by SMB decrease. They suggest the dynamic discharge contribution is limited by margin thinning and retreat as well as a competition between surface melting that removes ice before it reaches the calving front. Another modeling study based on four outlet glaciers that drain 22% of the Greenland Ice Sheet (Nick et al., 2013, Nature) shows that the dynamic contribution would be about 4-8.5 cm sea-level rise by 2100 versus 2.5-9.8 cm for SMB. Finally, radar (ERS-2) and laser (ICESat) altimetry observations show that mass changes in Greenland were dominated by SMB changes between 1995 and 2001, and then both SMB and dynamics equally contributed to the negative mass balance from 2001 to 2009 (Hurkmans et al., 2014, TC). Therefore, we think that not taking into account the dynamic part is a very strong assumption and we question the pertinence of the results presented here. At least, a scaling taking into account dynamics could be proposed in the decay time equation as well as a stronger discussion related to those three studies.

2. Even if we assume that ice loss only comes from SMB changes (which is the case of this study), the study **lacks some proofs that the decay time equation is robust against process-based studies**. Only Figure 2 clearly shows that the results agree well with two process-based numerical simulations, even if it does not show the time to lose 10% of ice for Ridley et al. (2010) under 1°C warming above threshold. Figure

3 shows the same quantity but for 50% of ice loss with only one numerical simulation (Robinson et al., 2012). What about Ridley et al. (2010) in Figure 3? In order to validate the simple equation proposed here, we think that the decay time for other α values (20%, 30%, 40%, 100%) should be shown along with results from process-based simulations.

3. It is not straightforward to understand **how sections 2 and 3 really fit** into the paper since the authors do not use the equations (1) to (10) related to the Vialov profile and the critical SMB for deriving the decay time equation (17), except equation (6) that relates surface melt rate and elevation. It is nice to see how the critical SMB and surface elevation below which a meltdown is inevitable are calculated but they are not really used in computing the main results of the paper (since the decay time only depends on the warming level, lapse rate, ELA and melting sensitivity). As far as we understood, one of the main purposes of sections 2 and 3 is to show where Figure 1 (which is quite nice) comes from.

4. The **discussion** clearly misses a robust analysis of the results. For example, some drawbacks related to the use of the decay time equation are presented at the end of section 5 but we think that they should really go into the discussion and be more detailed.

5. The whole paper talks about the surface-elevation feedback but in reality this is the **SMB-elevation feedback** (IPCC, 2013; Edwards et al., 2014, TC; Goelzer et al., 2013, J. Glaciology). Furthermore, the paper does not talk about the feedback of ice sheets in general but of the Greenland Ice Sheet in particular. Finally, the results of the paper focus less on the SMB-elevation feedback than on the melt time. Therefore, we suggest a different title: 'A simple equation for the melt time of the Greenland Ice Sheet'.

6. In **section 1 (Introduction)**, the first paragraph is very long and could be separated into two different paragraphs, one with the general Greenland ice loss context and the

other one with the temperature threshold and the SMB-elevation feedback. In any case, the link with the last paragraph of section 1 is not really done. We would add a clear explanation about the SMB-elevation feedback and the importance of determining the melt time for Greenland.

Technical Corrections

P2, L9: 'has been loosing' instead of 'is been loosing.

P2, L12: 'the' instead of 'The'.

P2, L13: Rephrase 'the lower the ice surface reaches into the atmosphere' since this is not clear.

P2, L14-15: The sentence 'The rate of ice loss is highly relevant for coastal protection worldwide' does not really fit here. It could go to the beginning or the end of the abstract.

P2, L16: Delete 'as it should be'.

P2, L16: Is the bit 'In order to contribute a little to the conceptual understanding' really needed? We would remove it.

P2, L18: We would cite the three observable 'characteristics', which we think are better defined as 'parameters'.

2, L20: 'critically depends' instead of 'depends critically'.

P2, L21: Use of 'critical' and 'critically' in the same sentence. Maybe replace 'critically' by 'strongly'.

P2, L21: 'the' instead of 'The'.

P2, L24: 'meltdown' instead of 'melt down'.

P3, L27: The first sentence is not totally accurate. Maybe: 'Global sea level rise has been raising in the past decades mainly due to ocean thermal expansion and melting ice (Church et al., 2013).' The last reference is more accurate than 'IPCC (2013)'.

P3, L28: 'past two decades' instead of 'two past decades'.

P3, L29-31: We think that some older references could be deleted and some newer

studies could be added, e.g. Kjeldsen et al. (2015, Nature) who study the Greenland ice loss since 1900 using aerial imagery, Khan et al. (2015, Reports on Progress in Physics) who provide a review of Greenland Ice Sheet mass balance, Shepherd et al. (2012) who provide results from the Ice sheet Mass Balance Inter-comparison Exercise (IMBIE).

P3, L36: 'Greve, 2000' instead of 'Greve, n.d.'.

P3, L38: 'critically depends' instead of 'depends critically'.

P3, L43: The authors need to agree whether they use 'meltdown' or 'melt-down' throughout the article (see also L24).

P3, L45: We did not find that Howat et al. (2014) mention a sea level rise contribution from Greenland of 7 m. Maybe Gregory et al. (2004, Nature Brief Communications) is a more suitable reference. Please also check your references for Howat et al. (2014) because you list both the TC and TCD articles: is it really necessary?

P3, L50: 'surface mass balance (SMB)-elevation feedback' instead of 'surface-elevation feedback'. Please check this for the whole paper (e.g. title of section 2).

P3, L51: 'one dimension' instead of 'zero dimension'.

P3, L51: 'section 2' instead of 'section 1'.

P3, L53: 'section 3' instead of 'section 2'.

P3, L51: 'section 4' instead of 'section 3'.

P3, L55: 'feed' instead of 'enter'.

P5, L61: '(e.g.' instead of 'e.g. ('.

P5, L71: The authors already mention the Vialov profile above (L65), so there is no need to recall it.

P5, L73: Please define all quantities, i.e. h, x and L just after equation (1).

P5, L73: h_m is more the surface elevation at the ice divide rather than the maximum surface elevation (Greve and Blatter, 2009).

P5, L74-75: Rephrase 'we do not aim for a realistic representation of the ice flow', which is not 'politically correct'.

P5, L77: 'constant and equal to surface accumulation' instead of 'homogeneous at a value, a'.

P5, L78: Define L after equation (1) instead of here.

P5, L79: 'icebergs' instead of 'ice bergs'.

P5, L83-85: What is the purpose of writing down equations (2) and (3)? Mean surface elevation is not used at all in the study. If the authors demonstrate their usefulness, what is the derivative in equation (3)?

P6, L90: Precise which quantities you normalize.

P6, L93: 'equilibrium-line altitude (ELA)' instead of 'equilibrium line'.

P6, L108: We did not really understand how you 'rescaled' the SMB by A in equation (7). Don't we miss A in this equation, i.e. $h^m A - \gamma \Gamma h - a_0 = 0$?

P8, L121: Is it really necessary to have an entire section only for 13 lines? Wouldn't it be more useful to merge it with section 2?

P8, L122: 'ice sheet' instead of 'ice-sheet'.

P8, L124: Rephrase. Maybe: 'conditions, i.e. being a solution of the governing equation (7) and a minimum of the function...'

P8, L131: Equation (10) could be written more easily if starting by '(1-m)' instead of '-(m-1)'.

P9, L136-137: The first sentence is not really necessary since it was done in the previous section.

P9, first paragraph: Since you extensively compare your analysis to Ridley et al. (2010) and Robinson et al. (2012), maybe it would be useful to give us more insights about their methodology in the introduction (e.g. which models they use) and to try to provide an explanation for the differences between their models and the simple equation.

P9, L145: Why did you choose a threshold of 1.6°C? Is it only based on Ridley et al. (2010)?

P9, L145: What do you mean by 'both models'?

P9, L148: Define ΔT: is it T-T_c with T being the temperature above the threshold?

P9, L158: You previously defined the melt rate (Δa_0) as negatively related to melting sensitivity and warming level (see L147)? And now it is positive. What is right?

P10, L170: 'with time, which' instead of 'with time which'.

P11, L188: 'choose' instead of 'chose'.

P11: The second and third paragraphs should be re-organized as they are a bit confusing: Figure 2 is explained only in the third paragraph but is already mentioned in the second paragraph.

P11, L200: 'translate' instead of 'translates'.

P11, L204: 'strongly depends' instead of 'depends strongly'.

P11, L204: 'threshold' instead of 'thresholds'.

P12, L211: Figure 4 is (almost) not discussed in the paper.

P12, L212-213: Rephrase, maybe: 'Since results obtained with equation (17) do not account for any dynamical discharge or even ice motion, they strongly deviate ...'.

P12, L215: It is not really apparent in Figure 3 that results deviate more strongly with a higher ice loss. Rephrase or rescale the figures.

P12, L221: Dynamic discharge is not as limited as suggested by different studies (see first specific comment).

P13: Rewrite discussion taking into account all specific comments.

P13, L234: Rephrase 'For these curves in this figure'.

P13, L238: Precise that you mean sea-level contribution from the Greenland Ice Sheet.

P13, L241: 'dominant' instead of 'dominate'.

P13, L245: 'multi-millennial' instead of 'mult-millennial'.

P14, Tab. 1: Write down ΔT somewhere in the table.

P16, Fig. 2: 'median (...), likely (...) and very likely (...)' instead of 'median (...) and the likely (...) and very likely (...)'.

P17, Fig. 3: To be consistent with Fig. 2, it would be better to get the time in years (and not kyears).

P21, L338: Complete reference Greve (journal, year, volume, etc.).

P22, L347: Do we need to have the Howat TCD article since the TC article is listed in L342?

Interactive comment on The Cryosphere Discuss., doi:10.5194/tc-2016-60, 2016.