Reply to referee's comments

13th June 2018

The authors wish to thank referee 1 who spent time to correct many grammar and spelling mistakes and to provide us with many useful comments.

Anonymous Referee #1

The revised manuscript is improved over the original submission.

From a science standpoint, this is an interesting study, which takes advantage of new satellite products to evaluate the mechanisms driving the breakup of iceberg s (basal melting and fragmentation). The study shows that existing models are r elatively successful at producing basal melting and fragmentation estimates that are consistent with observations. This is valuable information, which will prove useful in advancing modeling of freshwater inputs to the ocean from icebergs.

The manuscript still suffers from substantial English problems (despite the fact that the authors say that they had a Canadian colleague proofread the manuscript---next time they may need to find a colleague who is willing to make more heavy handed use of a red pen.) Because I found myself marking corrections throughout the manuscript, so I eventually just created an annotated pdf. Please see the attached document. I do not know if I was able to flag every error. I would strongly recommend that the authors run a spell checker and grammar checker if possible.

Thank you for your time we did our best to correct the typos and erros.

While it is easy for authors to blame font substitution problems on the reader, that is not a constructive way to communicate with the audience. Regardless, I did not find font substitution errors in this version, and I see that the pdf file indicates embedded fonts.

Sorry but we checked again the pdf (both initial and revised versions) and didn't find the equations errors signaled by the reviewer.

Anonymous Referee #3

Bouhier and co-authors present an in-depth study of the melting and fragmentation of two large Antarctic icebergs. The study is concerned with an important topic in climate and cryospheric physics; a topic which has seen a recent surge of interest. The ideas and methods underlying this study are a good fit for The Cryosphere. However, I have a few major and more minor concerns that I believe should be addressed before this manuscript is accepted for publication.

Major Comments:

- I am aware that the authors have had somebody proof-read the language of the manuscript. However, there are still a large number of grammatical errors, typos, and formatting issues in this revised version. This has made reading and reviewing the manuscript unnecessarily difficult. I want to illustrate this with just the first lines of the introduction:

l.16: misplaced superscript "3" l.17: "(1.500 km^3 yr^-1 ~80%)" -> "(1500 km^3 yr^-1, ~80%)"

l.17: "Tournadre et al. (2016)" -> "(Tournadre et al., 2016)" l.18: "as a reservoir to transport ice" -> "as reservoirs transporting ice"

l.18: "Antarctic Coastline" -> "Antarctic coastline"

l.19: "diffuse" -> "diffusive"

l.20: "the ter input" -> "the water input"

Sorry for the typos and errors. We did our best to correct the typos and errors (many thanks to reviewer#1 for the corrected pdf)/

I could go on. I would keenly urge the authors to revise the language and format to bring it up to the high standard appropriate for The Cryosphere. I would advise to consult a native English speaker once more.

- P.11 Estimation of V_w and T_i: As far as I understand the method here, the authors use one equation (eq 3) to determine two unknowns (V_w and T_i). This system is thus underconstrained, no? Please explain your

We thought it was clear enough in the text that the solution to estimate two unknowns from one equation is the minimisation of the difference between model and observations as stated in the text. We changed the sentence to

As current velocities and iceberg temperatures are not constant during the iceberg's drift, the modelled thickness loss is fitted to the measured loss for each time step t_i over a $\pm$20-day period by selecting the $V_w(t_i)$ and $T_i(t_i)$ that minimise the distance between model and observations.

- P.11, P.15, P.17: I'm confused about the "99.9% correlation" between the models and observations (and reviewer #2 has hinted at this, without a satisfactory answer, in my opinion). Since the models are fitted to the observations (over small time steps) isn't a high correlation guaranteed by design?

To fit a model doesn't guarantee a high correlation. Even if the model is inadequate, there is still a solution that minimises the distance between model and observation. This solution can have a low correlation with model. It is true that if the model is adequate the solution that minimises the distance will have a high correlation.

Or rather, can you speak of "correlation" in the typical sense here?

I don't understand. We use correlation in the mathematical sense of correlation coefficient.

I see this issue with all 3 models that are discussed. Furthermore, If I understand this correctly I would have to disagree with the first line of the discussion (p.15 l.25): the authors have merely fitted V_w and T_i (in an underconstrained way(?)) such that the modeled loss of thickness matches the observed. P.15 l.25 reads as if the model ran independently from the observations and recovered the same thickness evolution. This is certainly not the case.

We think that it is clearly stated in the text that we fit the models to the observations and that this method allows to reproduce the observed variations of thickness with high precision. May be the sentence was not clear enough, we changed paragraph 3.3

- On closer inspection it becomes clear that the two models of eq (3) and eq (5) are not that unlike each other. Both depend (slightly non-linearly) on relative velocities and linearly on the relative temperature difference between ice and water. However, a direct comparison between the two models is made difficult by the different notations used. The models should be formulated as similarly as possible to make a comparison more intuitive.

The two parameterisations of the melt rate differs primarily in their representation of the heat transfer coefficient γ_T. The Week and Campbell parameterisation can be considered as a bulk and a simplified version of the Hellmer and Olbers one. The notation we use is the one used in all the literature and are identical for both parameterisation. We could provide an annex presenting a theoretical comparison of the two parameterisations. However, we think that it would be, firstly, quite long (at least 3 pages) and, secondly, that it won't be of great interest for the modeling community.

Also, there are some issues with units (e.g. unit of the 0.58 prefactor in (3), unit of water viscosity (p.12, l.25)?).

We introduce the units for both parameters.

It would be informative to see how the two models compare for standard values of the drag and material coefficients. I'd suggest a plot for M_b as functions of V_w-V_i for both models (although V_w is presumably a different velocity in eq (5), or as functions of T_w-T_i (or T_b-T_w).

Equation 3 shows that Mb depends on the iceberg's longer axis (L) maximum length, the temperature difference and the velocity difference while the Equation 5 shows a dependency on velocity difference (through u* and γ_T), the temperature difference between the iceberg and the boundary layer and the temperature difference between the boundary layer and the water. It is quite difficult to make a significant plot as the parameters are quite different. In figure 1 we plotted the ratio of the thermal turbulent melting rate and forced convection one for L=120 km (top) and L=30 km (bottom) as a function of water temperature and velocity difference. T_i is fixed to 4^oC and $T_b$to -2^oC. The ratio depends on the iceberg's length. It is of the order of 5 for icebergs around 30-50km and velocity difference <0.4 m/s and temperature <10°C. We added a sentence in 4.3 in the text explaining that the computation of Mb with the two formulas with the same environmental parameters gives a factor 5 difference for B17 and 6-8 for C19a.

Furthermore what values of S_w and P_w are used? (If they are taken to be constant, it might make sense to just give T_b as a constant).

The salinity varies very little in this region and is fixed to 35 PSU. The pressure is the one at the base of the icebergs (estimated from the thickness of the iceberg). It is now precised in the text.

Overall, section 4.2 appears to be more or less copied from previous work without putting it into the context of the present study.

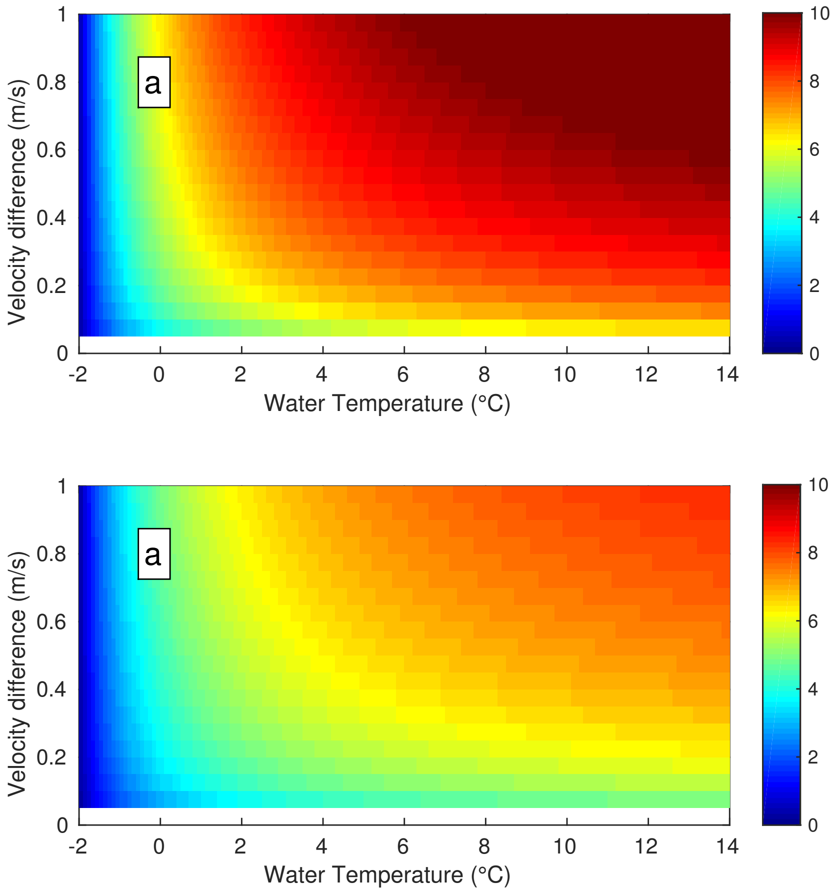

Figure 1: Ratio of equation 5 over equation 3 melting rates for L=120 km (a) and L=30km (b) as a function of water temperature and velocity difference.

We don't understand the remark. Why 4.2 and not 4.1. The two paragraphs briefly describe the two melting parameterisations.

- Regarding firn compaction: I agree that it is important to mention this in the main text and to provide the 2-5% error estimate. However,I would argue that it doesn't need to get a full appendix (the error is small and the matter is rather tangential to the story). I thus recommend just removing Appendix A.

It was a demand from reviewer 2.

Minor Comments:

P.3

l.13: "area, size, and shape" - What's the distinction between area and size here? Does size refer to longest horizontal dimension(s)? Please clarify.

Changed to area

l.21: "The first section" - The Introduction is really the first section. You should probably refer to the sections by the numbers they are given.

Changed

Figure 1: - mark grounding sites - change time labels on legends to Jan 2014, Feb 2014, ...

Changed

P.4

L.6: delete "(latitude, longitude)" L.9: "Altimeter data can" L.17: "final detectable collapse"

Changed

Figure 2: - add a legend with "* - MODIS, o - Altimeter"

Already in figure caption

P.5 Section header: "2.3 Environmental data"

Changed

P.7

L.14 So the +- 0.9m represents the standard deviation of the standard deviation? I would just report the std as +- 3m. Or am I misunderstanding?

Correct. Changed to +-3m.

L.18 It's difficult to reconcile these numbers with Fig. 4a. There seems to be a faster melt period between Sept '14 and Nov '14? The melt appears to be slowing down again after May '15?. If you want to give these three regimes you should probably indicate the slopes with dashed lines?

The numbers in the text are complementary information and help understand the figure. Adding slopes with dashed lines won't give much information and will crowd the figure.

L.23 Stern et al (2016), "Wind-driven upwelling around grounded tabular icebergs" talks particularly about the unbalanced forces around grounded icebergs.

Added ref.

Last paragraph: If I understand this calculation correctly it assumes that all sidewall erosion is due to fracture and all bottom erosion is due to melt. I agree that this is a good approximation, but it should be stated explicitly.

Correct. We added, **For large icebergs, the sidewall erosion/melting, which is of the order of some meters per day, can be considered as negligible compared to breaking**.

Equation 1: This has a dimensional issue. The right hand side is M = dV/dt = m^3.d^-1? The l.h.s is m^2*m. I guess you assume dT has units m.d^-1. You should probably write something like M = \Delta V/ \Delta t = S* \Delta T/ \Delta t, where \Delta t = 1 day. I'd argue for the use of \Delta T, rather than dT, since you're looking at finite intervals.

The text was not clear enough and allowed a confusion between M the cumulative volume loss by melting and $M_b$the melting rate. Here M is in m^3 and dT in m. We changed the sentence and the equations for M and B.

Equation 2: Similar arguments as for eq (1)

idem

Figure 4 - panel a. The caption doesn't match the colors of the figure. Also, maybe make the stars the same color as the continuous lines (red and blue?)

We changed the lines and the individual measurements (circles and stars) and the figure caption for figures 4 and 5.

Equation 4: what do the different terms represent physically?

We think that the sentence introducing the equation clearly describes the different terms. **"It assumes heat balance at the iceberg-water interface and was originally formulated for estimating ice shelf melting . The turbulent heat exchange is thus consumed by melting and the conductive heat flow through the ice:"**

P.16

L.20 delete parentheses

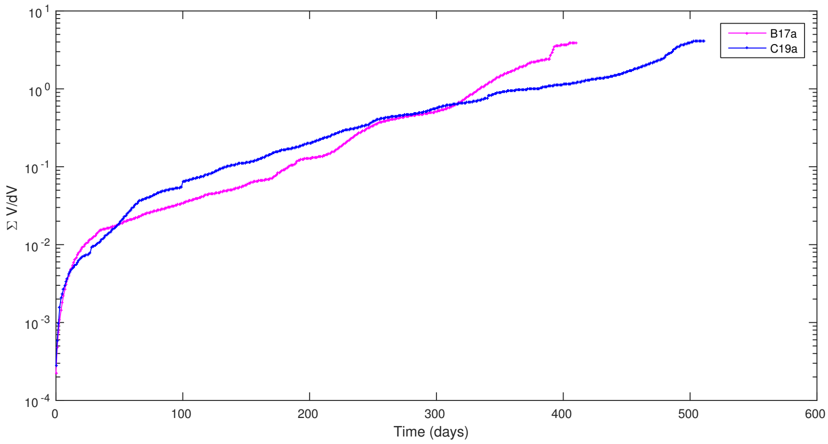

Figure 2: Comparison of the cumulative relative volume loss by fragmentation for B17a and C19a.

Done

L.23: what are the 63% and 64% values? Correlation coefficient r?

Changed to 0.63 an 0.64. We changed all correlation from % to linear.

P.17

L.1: "a-dimensional loss" -> "relative volume loss"

changed

Equation 8: While the form of eq (7) makes obvious sense, I don't have an intuition of why a second dependence on V_i should be of the multiplying form (1+exp(...V)). Could the authors explain this choice?

As shown by the correlation analysis M_{fr} depends primarily on temperature and secondly on velocity. To introduce a second order dependency on velocity we consider the velocity contribution as a corrective term so in the form of 1 + correction

Figure 8. I find it hard to see anything in this figure. The panels should be substantially revised and rethought. To start out with, I suggest two columns, with column 1 for C19a and column 2 for B17a.

The main point of this figure is to show the correlation between DV/V and environmental parameters. As it is we think it clearly shows the primary correlation with SST and the secondary one with velocity. We don't think that doubling the number of subplots would improve the comprehension.

Figure 9. I would put these on a log-lin scale (by construction of equations (7) and (8) this seems, no?).

We changed the y scale of the plot to log.

Furthermore, I'd suggest a plot where the B17a and C19a curves are laid on top of each other to compare the two melt rates visually.

The comparison is not very significant as the two icebergs experienced very different environmental conditions especially near the end of their drift. We don't think that such a figure will be real interest for the study.

Fig 11. Some of the labels appear to be messed up, although I'm not entirely sure which ones

Corrected. There was an inversion of the x and Y label for subplot a.