August 29, 2015

Dr. Andreas Vieli,
Associate Editor
The Cryosphere

Re: Manuscript TCD-9-2915-2015, "The relative contributions of calving and surface ablation to ice loss at a lake-terminating glacier"

Dear Dr. Vieli,

We wish to thank the reviewers for their comprehensive reviews and very useful comments that will greatly improve our manuscript. We have undertaken significant effort to substantially revise this manuscript to address the reviewers' concerns on four main points: 1) correct grammatical and definitional errors, 2) restructure the manuscript and reduce length by eliminating redundant text, 3) focus on improved clarity in our methods and provide a stronger, clearer justification for our study design and assumptions, and 4) re-focus our Discussion section to provide a more comprehensive discussion of the transferability of our findings to other studies and lake-calving glaciers worldwide.

We have accepted and amended the text to address all editorial suggestions provided by the two reviewers, and thank them for taking the time to make these suggested changes. Below we summarize our primary changes and respond to specific reviewers' comments and critiques on the paper content in a point-by-point fashion.

We hope you will find our revised manuscript has adequately addressed all of the concerns of the reviewers, and is significantly improved.

We appreciate the opportunity to revise our manuscript, and we look forward to any additional feedback that you or the reviewers are willing to provide.

Sincerely,

Matthew Chernos, Michele Koppes, and R. D. Moore

<u>RESPONSE TO REVIEWS</u>

Original in **bold**, response in *italics*

Reviewer #1, Roman Motyka

1. There is a woeful lack of referencing recent publications, and also older ones, that are really pertinent to their work. A list of these references plus others is provided at the end of my comments. In particular, the authors would find Trüssel et al. 2013 and Trüssel et al 2015 instructive. It is surprising that at least the 2013 paper wasn't referenced because it fits precisely within their stated desire to put Bridge Glacier into a "contextual" framework. For additional "contextualizing", see papers on Patagonia lake calving glaciers: Sakakibara et al 2013, Sakakibara and Sugiyama 2014, Warren et al 2001. You should also look at Larsen et al 2007 for additional information on lake calving glaciers in SE Alaska.

We have added references to more recent papers to the Introduction (see text comments below), as well as changed wording on framing previous research. We also highlight that we are comparing Bridge Glacier to other freshwater calving glaciers where not only calving but also mass balance or surface ablation have been measured.

2. Perhaps the most egregious lapse in this paper is the use of terminology. The authors should avail themselves of the Glossary of Mass Balance and Related Terms, (2011) available online and acquaint themselves with the basic definitions concerning mass balance. All too often the authors use the term "ice loss" to mean summer surface melt below the ELA in a given year. This occurs in the title of the paper itself! In the context of mass balance, ice loss implies thinning and shrinking of the glacier, i.e., a net mass loss over time. If a glacier is in quasi-equilibrium, then ice lost in the ablation area is replenished by ice flux from the accumulation area. Even with negative mass balance, the summer melt is not equivalent to glacier ice loss, it is just summer the summer melt. Understanding the differences is important because other publications they cite compare calving losses to glacier mass balance and not summer melt. The entire paper needs to be rewritten to emphasize these distinctions.

We thank the reviewer for this comment; it has helped us clarify our manuscript. We have rewritten the paper to reflect that we are focusing on net ablation of the ice, through calving and surface melt. We now define the term ablation in the Introduction at lines 65-67, and distinguish it as ice loss that is a separate entity from annual snow and firn losses. Furthermore, we would like to stress that we are only looking at ablation of glacial ice. As such, we can consider calving to be 'frontal ablation', while surface melt is equal to the net surface ablation below the snowline (ELA).

3. DEMs: the authors need to state the date of the Lidar survey that they are using for hypsometry. Is it 2013? If so, is the glacier outline, particularly the terminus, from this Lidar survey? If not, what is outline based on? A graphic displaying the hypsometry would be useful.

We note that the glacier outline is from Sept. 2013 (now noted in Figure 1), and the Lidar DEM is from 2006. The AAR of Bridge Glacier, based on the average 1984-2012 ELA (~2100 m), is 71%, and this has been added to the study area description (Lines 82-83).

4. But what about other DEMs? Isn't there one from the original map for this region? And also the SRTM data from 2000? Can't you compare the Lidar DEM to these older DEMs to get a handle on the actual amount of drawdown Bridge Glacier has experienced? Perhaps do a geodetic determination of mass balance? Perhaps someone has already done this?

DEM data are also available from TRIM data for 2000. However, these data are in a coarser resolution (25 m), and have several digital artifacts (such as an unrealistic 'reverse slope' of 50 m in the lower reaches of the glacier). These artifacts, combined with the relatively small period of time between DEMs, and that the Lidar DEM was obtained with snowcover (estimated at 2-5 m), we did not feel confident that any DEM comparison would yield reliable information on thinning rates. However, this question could be more fully explored in future research.

5. Another egregious error is hind-casting their melt model without consideration of the so-called Bodvarsson effect: *Bodvarsson G (1955) On the flow of ice-sheets and glaciers. Jo¨kull,5, 1–8.* Thinning due to a negative surface mass balance can cause the ice surface elevation to lower and expose the ice to warmer climate conditions. Progressively larger areas of the glacier then lie below the equilibrium-line altitude (ELA). This effect becomes even more pronounced if the ELA rises to higher elevations due to changing climate.

While we agree that thinning can drive velocity changes (i.e. the Bodvarsson effect), for Bridge glacier we note that the elevation difference between the 1970 and 2013 terminus positions was <100 m. Moreover, the change in ELA during this period was less than 200 m (see Figure 7c). Hence, we conclude that thinning and lowering of the surface elevation below the ELA is likely would only have a minor effect on modelling results, and its error would be difficult to quantify.

6. When you are hind-casting, what climate conditions are you assuming in order to drive your melt model? Or are you just assuming same as 2013? If so, that is quite an assumption!

To hind-cast the melt model we are using piece-wise linear mass balance model with measured ELAs from Landsat imagery (see lines 310).

7. Firn line vs. snowline: if ELA rises high enough, then firn from previous years will be exposed to melting. Are you ignoring this in your analysis?

In our melt model, firn is treated as snow (i.e. it is omitted from ice loss calculations). Moreover, ELAs are measured using end of summer Landsat imagery, from which we could detect the snow-firn transition, and used this to define the ELA and the extent of ice melt.

8. Regional climate indices: Why would Vancouver be representative of this mountainous region, which you previously said was under a mixture of climate influences? You need to defend your choice or find a closer index. Perhaps the mean annual flow anomaly is a better proxy but I am not sure since you don't state the size of the basin the gauge samples and the influence of rainfall.

During summer, air temperature exhibits a high level of regional coherence due to the spatial scale of the synoptic-scale features that dominate the weather – especially anticyclonic systems, which produce widespread fine, warm weather that is conducive to surface melt. The weather station at Vancouver Airport is free of signals associated with local land use change and we consider it a good broad-scale climate proxy for the Bridge Glacier area because it is well correlated with discharge from the gauge at the outlet of Bridge Lake. We also compared the discharge record with the climate record at Whistler, BC, which is only ~50 km to the south; however, we found that the relation was weaker (R^2 of 0.59 vs. 0.65 for Vancouver Airport).

The Bridge River streamflow gauge is located just downstream of the outlet of Bridge Lake, about 1 km east of the edge of our map in Figure 1. The gauge captures the outlet of the lake basin, of which 60% of the watershed is filled with the glacier; hence, other precipitation inputs are considered negligible. We have updated our description of the study area at lines 93-105 to better explain this.

9. Terminus retreat: you need to be consistent when providing data. In the methods, you state terminus change was determined by comparing successive Landsat images and measuring the area of change. Yet Figure 3e and later in the text, you use m per yr, not area! How did you convert area to linear retreat?? There are now standardized methods for doing so to get an average rate of retreat. In Fig. 3e, did you plot all Landsat data or just one from each year? It appears that the terminus advanced in some years. Not unusual, as we see calving of floating tongues in lake systems to be quite episodic, on the scale of years sometimes, see Trussel et al 2013.

Retreat rate (m/yr) was found by averaging the surface area lost (in m^2) by the terminus width (in m). Negative 'retreat rates' would correspond to advances, the most recent of which occurred in 2007. In Figure 3e, we plotted end-of-season terminus positions for each year (between Sept and Oct. of each year).

10. Velocity data: please show all of your velocity results somewhere, either as vectors on the map or in a table with reference to position. This is important for a reader to assess the validity of your ice flux calculations.

Thank you for your comment. We now include velocity vectors measured across the glacier in Figure 5.

11. Lapse rate: what do you mean "standard lapse rate"? Need a reference.

We have clarified that the lapse rate was derived by Stahl et al. (2008) (see lines 263).

12. Section 7.2: comparing summer surface melt that takes place below the ELA to calving losses seems to be the crux of your paper. I am having much trouble understanding the data in this section and much more explanation is needed. Furthermore, I do not understand how you arrived at your 85 day retreat area. I also have problems with water depth, flotation and ice thickness. a. b. c. First of all, you should show all of your velocity results somewhere, either as vectors on the map or in a table with reference to position.2cd, just how did you determine the 65 m terminus retreat over the 85 day period? Is this an average? Or can you show schematically the area of retreat? 3rd, what width did you use to get an area of -0.297 km^2 for the retreat?? You would have to have a width of 4.6 km for a retreat of 65 m in order to get your answer of - 0.297 km^2! If I use the width you used for calculating ice flux, 1.055 km, and a retreat of 65 m, that gives me an area of 0.068 km^2, not -0.297 km^2. Or was this a typo and you meant -0.0297 km^2? That would fit withe a terminus width of ~ 0.5 km.

a) As mentioned above, the spatial distribution of surface velocities is now included in Figure 5.

We are grateful for these comments, for they have helped us clarify our description of the methods we used. We have reworded this section and now state: "Over the 85-day study period in 2013, a change in terminus area (dA_T) of -0.297km^2 was measured from repeat terminus delineations. The average velocity at the terminus (U) was 139 m/yr, across a terminus width (W) of 1055 m, yielding a calving flux of 0.00342 km^2.

13. OK, now for ice flux area. a. So where was the velocity = 139 m/yr (0.38 m/d) measured?? Are you assuming plug flow? What about drag from the valley walls? What does the cross-valley velocity profile look like? b. Where is your "flux gate", i.e., where on the glacier are you measuring this flux? You state a width of ~ 1 km, so that would put it about a km from the terminus? Please show it on one of your figures!

a) Velocities were measured from Nunatak TLC (see above), and along the floating tongue (Terminus TLC). We believe that plug flow is a reasonable assumption in this case. The terminus region (see Figure 1,5,6) is not constricted by valley side-walls. The cross-valley velocity profile at Nunatak TLC does show measurable lateral drag (see above), although velocities along the centre flow-line are very close to measured velocities along the floating terminus.

b) Thank you for the comment - we now show the flux-gate in Figures 1 and 5. (Study Area, Bathymetry/Velocity)

Now for ice thickness: Why are you measuring water depth 500 m from the June 2013 terminus to calculate flotation thickness in 2013?? Aren't the appropriate data the soundings right next to the terminus?? Using 109 m is wrong! From your Fig. 6, maximum depth is about 90 m at the terminus and much shallower on either side of the lobe, so perhaps an average of 80 m or so?

The median depth of 109 m is a typo (this is the estimated ice thickness). If we assume *a water depth of 91 m, and an H_b of 10 m, the ice-height is ~109 m. We believe that the median water depth is an appropriate estimate for the terminus given the steep bathymetry on either side, and the relatively flat 'U-shaped' bathymetric cross-section anticipated at the flux-gate. We have re-written: "The median depth was 91 m, corresponding to a height above buoyancy of 9.9 m, and an estimated ice thickness of 109 m"*

15. Speaking of ice thicknesses of floating tongue: why use equation 13 when you have a highly accurate Lidar DEM? If it is really floating then just use the freeboard to estimate ice thickness. You also have your TLC data to give you floating tongue freeboard. Judging from Fig. 5 photo you may be overestimating the ice thickness. For floating tongues, ice thickness is primarily controlled by the thickness at the grounding line. At Yakutat Glacier, the lake depth was 325 m but ice thickness was about 175 m.

We clarified the text at lines 290-297 to indicate that we did use the freeboard to estimate the ice thickness

16. Figure 10 and 11. Again the terminology is really confusing. What you are measuring is summer melt below ELA, specifically for 2013, not surface melt, not glacier mass balance. To be accurate, surface melt would include all melting, including snow above the ELA. The confusion comes from thinking in terms of glacier mass balance, where net ice loss (or gain) has a specific meaning, i.e., net accumulation minus net ablation.

As mentioned above, we have rewritten the text to reflect that we are modelling ablation (i.e. only glacial ice losses). All terms are defined in the last paragraph of new Introduction.

17. Figure 11: is never cited in text. I presume it was to be keyed to section 7.3? What are the shaded envelopes? Some sort of estimate of uncertainty? If so, it needs discussion and explaining.

Calculated uncertainty is discussed in the Results Section. We have now updated the figure caption to include this.

Figure 12: these sorts of figures were in vogue a couple of decades ago when researchers were first trying to understand the drivers of calving. I am not sure how useful they are anymore, particularly for floating tongues. Although these figures do point out the difference in calving rates for marine vs. lacustrine glaciers, water depth is clearly not the reason why.

We thank the reviewer for the comment. We think that this figure helps position Bridge Glacier within the broader context of worldwide calving glaciers. In particular, we find it helpful to quickly position both the magnitude of calving and the size/depth of the lake relative to study sites across the world.

Redundancies: the discussions sections contained so much of what was already said, it was hard for me to read through it. Filled with too many generalities.

We thank the reviewer for this comment, and have re-written the Discussion section to be more concise and streamlined.

Uncertainties: a section on propagation of all of the uncertainties should be included in Methods.

Uncertainty estimates/calculations have been re-organized to follow results for each section.

Text Comments:

Title: This title is confusing to me because I think of ice loss in the context of glacier mass balance. Here, you are not looking at overall mass loss or gain (positive or negative mass balance) (accumulation - ablation) but instead simply comparing summer surface melt below ELA to frontal ablation (calving losses).

We re-titled paper as "Ablation from calving and surface melt at lake-terminating Bridge Glacier, British Columbia, 1984-2013" to attempt to minimize confusion and improve clarity.

P1

L 7: "surface melt": This implies across entire glacier, whereas you are only measuring below ELA?

We now use the term surface melt below the snowline - see comments above. We have re-written the text throughout to improve clarity.

L11: What do you mean by summer balance? You do not have info on accumulation to make a glacier wide assessment.

Here we use the term summer balance to define net annual surface ablation below the ELA. We assume that below the ELA, the local net balance is equal to the ice loss because there is no net change in snow storage on an annual basis. We redefine these terms at lines 65-71.

L 23: Include more modern references: Shepperd et al. 2013, Radic and Hock 2011, Also see Clarke et al. 2012.

We have added Radic and Hock 2011 as well as Gardner et al. 2013 and Zemp et al. 2015.

P2L 4: Include Radic and Hock 2011, Shepard et al 2013 L 18: see also Larsen et al 2007

We have added Radic and Hock 2011, Gardner et al. 2013

L 23- 25: Again ignoring more recent work of Japanese in Patagonia on Upsala and other glaciers. See ref. list for Sakakibara et al 2013 and 2014 and also Warren et al 2001 for Glaciar Nef in Chile. Also for Alaska, see Larsen et al 2007, Truessel et al 2013 and 2015.

We apologize for not including more recent work in Alaska and Patagonia as part of our review of relevant literature. We have now rewritten the introduction to include other freshwater calving glaciers where both calving dynamics and glacier mass balance (or ablation) have been quantified, including Yakutat Glacier (Truessel et al 2013.) and Upsala (Sakakibara 2013).

P4

L 13-15: rewrite this confusing sentence.

"Calving rates and retreat from Bridge Glacier are then compared with findings from other lacustrine calving glaciers in Alaska, New Zealand and Patagonia. Commonalities in the nature and timing of calving fluxes and summer ice ablation allow for a broad understanding of the temporal pattern of ice loss during the transient calving phase of a retreating alpine glacier."

L 19: Cite Fig. 1 here.

Included

P5

L 7: 1972? Fig. 2 starts with 1985.

This section has been re-written and moved to lines 86-90. We chose 1985 as the earliest Landsat image in Figure 2 because the image is visually of much higher quality than those from the 1970s.

L 13, Fig. 3e: How is terminus position defined? Is an average? How measured? Advanced in some years??

See comment above.

L 15: Why would Vancouver be representative of this mountainous region, which you previously said was under a mixture of climate influences? You need to defend or find a closer index.

See comment above.

L 17 -20: Rewrite, too confusing.

We now state:

"However, this period of elevated melt conditions did not continue into the 21st century as retreat continued to accelerate." (Note: this section has been moved to Results (line

320-340)).

L 22-23: OK, once again mixing apples and oranges. Your study is not measuring surface mass balance so you don't really know what the annual ice loss is!

See comment above. We are measuring ice loss only below the ELA, where the net balance is equal to ice loss.

L 26: reference the model being used.

The model is a combination of methods described in MacDougall and Flowers (2011), Hock (2005), Hock and Holmgren (2008), and Shea et al. (2010). Relevant citations are made within the model methods.

P6L 10 – 14: To really check the model, you need ablation measurements at higher altitudes too.

Thank you, this is a valid point. While we agree that melt near or above the ELA would be necessary to fully constrain our model, we were constrained by logistics and timing and were unable to install ablation stakes in the upper reaches of the glacier this season (this will be the focus of future efforts). However, we stress that the error is partially constrained by our application of an observed snowline to the data, which restricts the potential for 'runaway' melt in the higher reaches of the glacier (i.e. further from our measured ablation points)

L 19 – 20: This is confusing. Were 74 pts measured or interpolated between measurements?

We have re-written this section to now say:

"Due to the presence of large, unstable icebergs throughout the lake, depth measurements were taken at 893 discrete points in an irregular grid. Access to the terminus and the middle part of the lake was hindered by the presence of icebergs, necessitating the inclusion of additional 74 points that were added by linear interpolation using known depths along east-west transects to improve coverage. "

L 23-24: OK but in results, you state 65 m retreat not area.

Re-written. See comment #12

L 26: ?? what rgeos?

rgeos is package function in the R statistical software language. The function we used was gArea().

L 28: TLC 1.5 km east: Location not shown in Fig.1.

Thank you for the comment, Figure 1 has been amended to now show the location of both TLCs used in the study.

P7

L2: References on how this is done? E.g., Krimmel or Harrison? See ref. list.

We have re-written to add clarity: "following Harrison et al. (1992) and Eiken and Sund (2012) (see Chernos 2014, Chapter 4 for further details) "

L 10 – 11: These velocity vectors should be plotted on one of your maps along with magnitude. This should be part of your results! Also, what is ice surface elevation of both your ablation stakes and your velocity markers? Please state somewhere!

Velocity vectors have been added to Figure 5. The elevations of the stakes have also been included in the text (line 120).

L 13 – 15: What is the date of the Lidar?? Reference here and in Fig. 1. Lidar is usually very accurate so you should know surface elevations quite well.

The Lidar survey was conducted in winter 2006. This has been added to Figure 1 caption.

L 15 – 20: You are ignoring the Bodvarsson effect.

Please see comment #5

L 16 – 18: Sentence as written is confusing.

We have re-written this sentence to clarify the text, and now state:

"Annual terminus positions and equilibrium line altitudes (ELAs) were reconstructed from Landsat images from 1984 to 2012. All Landsat data images taken between September 12 and October 24 to represent end-of-season snowlines and terminus positions."

P8

L 10 - 14: This is totally confusing. Snow is ice! Total ice loss during summer implies melt from above snowline too! Basically what you are measuring is specific balances on exposed ice below the snowline.

We have now defined our use of the term 'ice loss' in the Introduction (see comment above), and have re-written to clarify:

"As our purpose was to calculate total ablation during the summer melt season, we only consider ice melt and not snowmelt, and hence the model was only applied to exposed glacial ice below the snowline at each time step."

P12

L 2: Reference for lapse rate. How do you know whether it applies to Bridge?

We now cite Stahl et al. (2008), who determined a lapse rate of 6 degC/km by calibrating a model applied to Bridge River's catchment for simulating both glacier mass balance and streamflow.

P13

L 19: Why are you using this equation? If you know ice surface elevation above lake and you believe it is floating, then way not use free board estimate??

Given there have been several observations from the time-lapse cameras of tabular calving events that show some movement across the lake immediately following calving, combined with an estimate of height above the waterline and measured bathymetry, we feel confident that the terminus is near or above the threshold for flotation. We confirm this calculation using the freeboard estimate.

P14

L 1- 2: I would be really dubious about this assertion. Ice thickness of floating tongue is more likely established at grounding line.

See comment above.

L 2: There are two red arrows in the Fig. 5. Which is which?

Figure 5 has been replaced with a new figure in hopes of better illustrating the location of the inflection point marking the transition to flotation and approximate grounding line of Bridge Glacier.

L 8: Is this average speed, max speed or what? What is the gradient across glacier? Makes a difference when computing fluxes.

Here we are estimating the average speed, which is now explicitly stated

L 21-22: Poor coverage ?? Not according to your Fig. 6.

The bathymetric coverage was increased for areas that were covered by icebergs using linear interpolation. As such, we have less confidence in the bathymetry in this small region relative to the rest of the lake, where depth measurements were more closely spaced.

L 24: ?? You are not measuring mass balance are you? So how do you know about long-term mass loss?

Estimates of historical annual ablation rates are derived using ELA observations and a fitted mass balance gradient derived from several glaciers in the region (Shea 2013).

L 26: What makes you think summer specific balance is linear with altitude?

We arrived at this assumption from the work of Shea (2013), which is based on mass balance observations from several glaciers in the region (including Bridge Glacier). We *now include this in the text.*

L 26: What are the terms in the equation? Define them! Is this a specific mass balance measurement?

We have re-written this line "Below the snowline, the net balance (b_n) at a point is equal to the summer balance (b_s)…"

L 27: This again mixes apples and oranges. It may be equal to summer ablation but not glacier ice loss.

This point has been corrected for clarity (see above comments)

L 27: Is hypsometry from Lidar?

The hypsometry is derived from the 2006 winter lidar survey, shown in Figure 8.

P15**L 9 – 10: ?? Why not use elevation from Lidar?**

It was not possible to derive elevations for the lower reaches of the glacier that had calved before 2006.

L 10 - 16: How do you know hypsometry for prior years? What about Bodvarsson effect? Also, you are measuring seasonal melt not overall ice loss. The latter is mass balance. This gets really confusing!

We are assuming that hypsometry pre-2006 (date of Lidar survey) is similar, with the expectation of the loss/gain of glacier coverage in Bridge Lake (which we assume is approximately equal to the elevation of current terminus). See previous comment(s) on the Bodvarsson effect.

L 19 – 22: I think it would be good to have a table of hypsometry vs. ice loss. Reminder, you are modeling specific balances. Also, by definition, shouldn't your summer specific balance at the ELA be zero?

Figure 6 shows the net balance to be 0 at ELA. It also plots net balance against glacier elevation. We believe this is better means of visually representing the change in melt over elevation than could be seen in a hypsometric plot or table.

P16

L 1-8: What are the elevations of your stakes? From your figure, they all appear to be clustered at between 1500 and 1600 m. How far apart are they? Also, that's great that it works at your terminus ablation stakes but you have no upglacier control. Also, I believe you said earlier that stakes were 3 m long? But you are measuring ablation on the order of 4 - 5 m?

The location of stakes was constrained by logistics/timing. However, the error is partially constrained by having applied an observed snowline to the data, which restricts the potential for excessive melt in the higher reaches (i.e. further from measured ablation *points).*

While our drill only allowed for stakes to be at most 3 m long, the stakes were re-drilled in mid-July (see Field Methods): "The stakes were installed on June 18, and were resurveyed and re-drilled on July 19 and September 13, 2013." Hence we were able to capture up to 5 m of melt over the entire season.

L 9 – 16: Where did you measure this width? First of all, you should show your velocity results somewhere, either as vectors on the map or in a table with reference to position. 2cd, just how did you determine the change in terminus area over the 85 day period? 3rd, what width did you use to get -0.297 km2?? You would have to have a width of 4.6 km to get your answer! If I use your width of 1.055 km and a retreat of 65 m, that gives me an area of 0.068 km2. You need to show where on the glacier is your flux gate and also show just how you computed the 85 day loss in terminus area. Finally, I don't understand why you are using a position 500 m downstream of the terminus!

see comment #12

Sect 7.3: Totally ignores changes in surface elevation (Bodvarsson effect). P18

see comment #5

L 1-2: Hmm! This is all very obvious, does it need stating?

Have edited Discussion section extensively to reduce redundancy and remove generic statements.

L 13-14: ?? How did water depths increase? Did the lake level rise somehow? L 16: ??? Water depth at terminus looks deeper to me during 2004-2012.

Thank you for your comment. We have extensively re-written the Discussion section to be more focused on the general findings from Bridge Glacier, their agreement with findings from other lake-calving glaciers, and the transferability of our findings towards a more comprehensive "life-cycle" of a calving glacier. In particular, please see Lines 423-437.

L 19 -22: What's this all about? How do you get thinning rates from Landsat images? Where is this data published??

Please see above comment. In particular, this section has been re-focused in Lines 454-465.

P19

L 1: Why would glacier thicken? Positive net balance? Floating tongue thickness probably set at grounding line.

Please see above comment. We have re-written this section, see Lines 454-465 and Lines 501-512.

L 8-11: !!! Again, you need to be clear on what is being compared! Annual ice loss (or gain) usually refers to glacier-wide mass balance. Here, you mean summer ablation below ELA!

Thank you, we have re-written for clarity:

"…to a flux responsible for between 20-45% of the annual ice loss"

P24

L 28: published in 2002 not 2003.

Thank you - apologies for the oversight. This has been corrected.

Figures.

We have amended all figures as suggested.

Anonymous Referee #2

General

This manuscript deals with recent calving and mass balance changes taking place on Bridge Glacier, Canada. This issue is relevant for the journal, and can be of interest for a wide community of people working on calving glaciers. The number of measure- ments done by the authors is very extensive and I think they are describing important processes taking place in the area. But, I consider that the introduction is not well based on recent literature and that presentation of the data, methods and study area are not well organized. I think there are several concept confusions that need to be ad- dressed and improved before discussing and obtaining conclusions. I'm giving below several detailed comments/critics/questions. I'm afraid it was difficult to follow the text, for example when conclusions are presented in the study area before discussing how the data used to reach these conclusions were collected. I recommend re-writing the first sections before presenting results, discussion and conclusions.

We thank the reviewer for these supportive comments. They have helped us to restructure and significantly improve the text.

Detailed comments

Title:

I think the word "relative" is misleading. Is the manuscript dealing with water production? Surface ablation is important for quantifying how much water is leaving the glacier, but if they are interested in the mass balance, they must incorporate accumula- tion and see if the glacier is in balance, is gaining mass or is losing mass. The relative in this sense is not clear to me

Thank you for your comment. We have revised the title to better reflect this lack of clarity.

<u>*"Ablation from calving and surface melt at lake-terminating Bridge Glacier, British Columbia, 1984-2013"*</u>

Abstract:

1) I suggest changing the first phrase to: Bridge Glacier is a freshwater calving glacier located in the Coast Mountains of British Columbia, Canada, which has retreated over 3.55 km since 1972. The majority of this retreat occurred since 1991.

We have re-written: "Bridge Glacier is a lake-terminating glacier in the Coast Mountains of British Columbia, which has retreated over 3.55 km since 1972. The majority of this retreat occurred since 1991."

2) I suggest revising the use of two significant figures (3.55 for example), in order to be consistent with the accuracy in determining frontal changes or any other parameter.

We have re-ordered and significantly revised all Figures in the text, in particular Figures 1,3e, 4 and 5, to more clearly highlight the frontal and surface changes we observed.

3) I think the asseveration that the retreat is "out of proportion to surface melt" is confusing two different processes. The retreat is a response to mass balance changes and calving. Mass balance is a result of ablation and accumulation. The glacier can have a huge amount of ablation (and calving by the way), but its front can be stable or even advance, depending on the relationship with accumulation and therefore, with the total mass balance. Surface melt is certainly an important process for understanding glacier changes, but the consequences are not directly converted into frontal changes.

We now more clearly define the processes we are investigating, namely the contributions to total ablation and ice loss from surface melt and from calving, in the Introduction and Discussion sections. We also re-worded this statement for better clarity, and now state:

"This retreat is substantially greater than what has been inferred from regional climate indices, suggesting that retreat rates have been driven primarily by calving as the terminus retreated across an over-deepened, water-filled basin."

4) "Calving is responsible of 23% of mass loss". I don't understand this asseveration. Mass loss includes ice thinning? Did the authors estimate the mass balance of the whole glacier during this period, in order to reach this conclusion? Maybe they are only talking about frontal changes during the melt season.

We have re-worded to emphasize that the study considers only ice loss due to ablation (frontal and surface ice ablation, and not snow/firn loss) below the ELA over the melt season.

5) Then they talk about summer balance in relation to calving. Again, mass balance (even if only during the summer season) is not equal to surface melt.

Thank you. We have re-written to clarify that we only consider surface melt below the ELA, and therefore only concern glacial ice.

6) ". . .expected to diminish as the terminus recedes into shallower waters" Do they have any estimation of ice thickness upstream the present front? I can expect this trend if I have some data about the thickness, otherwise is just speculation.

Our ability to estimate ice thickness is admittedly coarse, but is based on the lake bathymetry, which gives an indication of ice thickness at the terminus, and of the thickness where the glacier becomes grounded. We estimate the ice thickness at the grounding line in 2013 is ~110 m. See text at lines 300-305 and Figure 4.

Introduction

1) I suggest that in the introduction they quote more recent and more closely related to the study area papers when giving examples.

Thank you for the comment. We agree with both reviewers and have added more recent and relevant references, including: Radic and Hock, 2011, Gardner et al. 2013, Zemp et al. 2015; Sakakibara et al., 2013 and Truessel et al, 2013.

2) The authors stated that few lake- calving glaciers have been studied worldwide. I'm afraid they need to have a better literature review including many more papers about this type of glaciers. Only in Patagonia (mentioned by the authors) there are studies on freshwater calving glaciers Upsala, Spegazzini, O'Higgins, Nef, Leones, Grey and Tyndall, among many others.

Thank you for the comment, please see above. We have added references to several more relevant studies to the Introduction and Table 1. Several Patagonian glaciers, including Upsala and Nef, are now listed in Table 1 in the Discussion, and we have changed some the language to reflect the diversity in Patagonian glacier studies. We would again like to emphasize that we are focusing on lake-calving glaciers where both calving dynamics (and/or retreat) as well as mass balance (and/or ablation) have been observed over the same time periods. While many of these studies have investigated calving fluxes and retreat rates, only a few (those in Table 1) contextualize those frontal changes with ice losses from surface ablation.

3) The last paragraph of the introduction is almost a repetition of the text previously presented. Maybe they can delete this part.

This section (lines 65-75) has been re-written to define the terms we are using and to emphasize that ablation from surface melt and from calving will be directly compared.

Study area and retreat history

1) I think a better Location Figure is needed. The Figure Number 1 has not enough information for a reader not well familiarized with the study area.

The inset map of Figure 1 has been updated to more clearly show the location and context for Bridge Glacier, including southwestern BC elevation data.

2) I think this chapter is mixing results with a description of the study area. For example; how did you estimate ELA since the 1970's? No methods, no reference etc. This must be moved to results. The frontal changes are not quoted; therefore I understand that these results were obtained by this manuscript. If this is correct, I suggest moving all of this to results. Before that, you need to discuss in methods how you measured these changes, the estimated errors, the used databases, etc. Figure 2 also needs to be moved and improved (add co-ordinates, scale, North etc. Figure 3 also needs to be moved to results

Thank you for your comment, the reviewer is quite correct. We have now moved all of our 'results', including Figure 3, to the appropriate sections in Results.

We have also added a North arrow and scale bar to the Landsat images in Figure 2.

3) They talk about over deepened basin. Again, this is a result of this manuscript? Did they measure bathymetry? Or is a result that needs to be quoted from a different paper?

Bathymetry was measured in study, and our description of this has been moved to Results. However, we stress that the over-deepening can be inferred without bathymetry; based simply on the shape of the lake and the size of the icebergs currently visible in the lake.

4) In Page 5 line 14 and 15, says: ". . .cannot be fully explained by regional climate. . .". This is a strong conclusion and must be moved out of "study area". This entire paragraph (lines 14-21) includes conclusions and must be justified by quoting a paper from the specialised literature.

Has been re-written:

"This retreat is substantially greater than what has been inferred from regional climate indices" (Note: this finding is echoed in Stahl (2008) - which has now been cited with this sentence).

<u>Field methods</u>

1) I suggest separating AWS, from Bathymetry, from mass balance, from satellite images, ice dynamics, etc., using subtitles.

We appreciate this suggestion, and have re-structured our Methods chapter to separate the individual field and modelling components, with appropriate subtitles.

2) The location and use of AWS needs to be better justified. Maybe you didn't have access to other locations or there is a hypothesis underlying this location. The same about the bathymetry. How were designed the tracks?

This section has been re-written to clarify the locations and methods chosen.

3) Figure 1 can be improved and quoted here to show the location of cameras and AWS. For example, in line 28, page 6, you mention TLC, and 1.5 km east. I needed to look very carefully and calculate distances in order to locate the cameras.

Figure 1 has been revised, and TLC "1.5 km east" is now added to the map as 'Lake TLC'. Additionally, Figure 5 (which shows the lake bathymetry) now also contains the TLC locations (and velocity vectors derived from the TLCs).

4) In page 6 line 28 you talk about "Floating terminus". This asseveration needs to be better justified. I presume you concluded this, but in this case you must describe in results how you did it. In the study area section you mentioned large calving events as explanation. Again, this is a result of your work of investigated by somebody else? I think you must give more attention to the explanation of both issues (tabular icebergs and floating tongue) in discussions after describing

your own results.

Thank you for this comment. We have re-written and added a new photo in Figure 4 that we hope more clearly shows the inflection point on the glacier surface which we argue indicates flotation. We also added the following text for clarity:

"During the melt season, large tabular icebergs calved and showed limited mobility, suggesting that the glacier is at or near the boundary criterion for flotation. There is a notable inflection point (Figure 4) roughly 500 m from the end-of-season terminus, where the surface slope becomes flat or slightly reclined, which has remained stationary since 2012, and where we assume that the terminus transitions from grounded to floating.

Modelling Surface melt

1) In this section you are mixing different methods, some of them partially described in the previous section (use of Landsat images for example). I think you need to reorganize this and the previous section.

Thank you for your points on organization. We have re-organized this section extensively - see comments above.

2) In Point 4.1 you again mix method descriptions with results (we estimate that ice loss is less than 10%...)

See comments above.

3) I think you are confusing here the term "ice loss" with ablation, which is not the same and need to be changed everywhere in this manuscript.

Thank you. For clarity we have re-worded ice loss as ablation, and now define all terms in the final paragraph of the Introduction.

4) In Point 4.2 Net radiation. I don't see if you calculated direct short wave radiation per pixel per day. I presume you considered declination angles and change the zenith angles day by day during the melt season.

Incoming Shortwave radiation was calculated considering declination, shading, etc. following Oke (1987). Full details are found in Chernos (2014). We felt that including these details in the manuscript would be redundant and/or cumbersome given that they are commonly applied calculations in energy balance models.

5) Did you calculate distributed albedo or you only use albedo from the AWS? This is clearly a limitation in the model. Did you use the photographs from the fixed cameras to estimate distributed albedo? This is something you can try.

Albedo was estimated from the on-glacier AWS, and was held constant for each day. Although this limits the model's representativeness over the whole glacier, given the model is only applied over bare ice, simplifying the albedo in this way is not expected to have an appreciable impact on the modelled volume of ice melt.

6) There is a problem when using LWR from outside the glacier and apply this to the glacier. Humidity is not the same out and on top of the ice. At least you must discuss this limitation.

Although incoming longwave radiation is expected to vary on- and off-glacier, the relatively small difference in humidity between the Glacier and Lake AWS (approximately 10% higher humidity on-glacier), we expect that any difference in incoming longwave radiation between the two sites is relatively small. Much more comprehensive studies by Shea 2010 (PhD Thesis) found "no systematic difference... over all sky conditions".

7) In page 10 line 24, you assume that terrain T° is equal to air T°, but later on you assume that the ice is at melting point.

By "terrain temperature" we are considering only non-ice terrain (i.e., rock, vegetation). Terrain temperature is only used to calculate incoming LWR from surrounding terrain (1-skyviewfactor).

8) In page 11, line 7 and 9 you say that the glacier is at melting pressure point, then you are dealing with a temperate ice. If this is correct, I have serious doubts on the asseveration that the lower tongue is floating. Normally, when temperate glaciers approaching flotation are collapsing due to the presence of water and crevasses within the lower tongues. This is something you must at least discuss and address.

While floating temperate ice tongues have been shown to be unstable, often leading to disintegration and dramatic retreat, the possibility of a floating termini made up of temperate ice remaining intact is not unprecedented in both freshwater and tidewater glacier systems. Some of the most prominent examples include glaciers that these authors and reviewer Roman Motyka have worked on: Yakutat and Bering glaciers in Alaska, Tyndall and Upsala glaciers in Patagonia. Boyce (2007) documents Mendenhall Glacier's calving rates, where an unstable floating terminus remained intact for approximately 2 years. Similarly, temperate lake-calving Yakutat Glacier (also Alaska) sustained a floating ~3 km terminus for over a decade (see Trussel 2013).

While we are aware of the more recent calving models that invoke the propagation of water-filled crevasses to the waterline or along the full ice thickness to the bed as one mechanism for generating longitudinal stresses and inducing calving (see Benn et al., 2007; Nick et al, 2010; Todd et al, 2014), these models require the presence of many open crevasses close to the ice front in order to cause 'collapse'. If, as at Bridge or Yakutat or Tyndall, the surface slopes decrease in the lower reaches and there is not an ice fall close to the terminus that would cause extensional strain, then many of the crevasses created up glacier will anneal, and will not generate a locus for calving.

Many examples of temperate floating tongues, where the height above buoyancy criterion has not been met but the calving front has not collapsed, and can be seen wherever large tabular bergs are found in lakes and fjords.

9) In line 11, page 11, you mentioned the use of 2.5 mm for ice, but you don't give a justification. This parameter is critical and must be well supported. What about sublimation?

The value was taken from Munro (1989,2006) and Pellicciotti (2005), and is within the range of "a couple of mm" suggested by Hock (2005) over glacial ice. If a glacier surface is melting, the vapour loss is evaporation and not sublimation, because the loss occurs from the water film coating the ice surface or snow grains, not the solid ice surfaces. We do not believe the air is dry enough in the region for significant rates of sublimation.

10) Line 12, page 12. What do you men for "standard temperature lapse rate". I mean, this number (-6 °C/km) is not standard. Depends on the region, and hopefully you can calculate this by measuring at different altitudes. What is happening when precipitation is solid and air T° is <0°C? Are you using a threshold?

Thank you for this comment. The lapse rate is from Stahl et al. (2008), based on calibrating a model to predict both glacier mass balance and streamflow for the Bridge River catchment. The sentence has been re-written to reflect this.

During the study period no precipitation occurred at T < 0C (on-glacier temperatures were only below 2C for 3 hours, and never below the melting point).

11) In line 22 page 12. Please describe the used method. Clausius - Clapeyron?

Have added:
"Saturation vapour pressure was calculated using Teten's formula (Murray 1967)". More information about the methods can be found in Chernos (2014)

12) 4.4 Melt contribution. Please quote a proper paper for the use of this equation and parameters. Did you use an altitudinal gradient for precipitation or is constant?

We cite Hock (2005), where the equation and parameters are discussed in full. We used a constant precipitation gradient. With our two rain-gauges, we did not find a significant altitudinal or E-W gradient during the field season.

<u>Modelling calving flux</u>

1) The Calving flux as stated assumes that the ice is floating, but the equation in fact assume that the ice is near flotation, not necessarily floating. I already asked before for the temperate condition of a floating tongue, so again, this is something you must address more carefully.

Given there have been several observations from the time-lapse cameras of tabular calving events that show some movement across the lake immediately following calving, combined with our estimate of height of freeboard (above the waterline) and the *measured bathymetry, we feel confident that the terminus is near or above the threshold for flotation.*

2) Page 14. Notable inflection point (Fig. 5). To say the true I don't see this notable point in the figure. Improve the photo or explain better.

See above comments. We now include a new photo in figure 4 which we hope better illustrates the change in slope where we are asserting floatation and the grounding line.

3) I have serious doubts on the floating condition issue and the statement "it is clear that the terminus became ungrounded. . ." How did you estimate this from the images? This is again conclusion and not methods, and I'm afraid this is not well justified.

We have re-written: see our response to the comment in Field Methods #4

4) The height of the ice wall in Fig 5 shows in places that the ice is clearly grounded. This is ratified by the bathymetric map (Figure 6), where the water depths near the front are quite shallow (even less than 20 m water depth). There is only one section with a bit more than 100 m water depth, that seems to me is located at the large crevasse indicated in Figure 5.

The measured median water depth from our transect near the terminus is 91 m. We feel the median water depth is an appropriate estimate for terminus given the steep bathymetry on either side, and the relatively flat 'U-shaped' bathymetric cross-section anticipated at the flux-gate.

5) I don't understand the phrase in lines 13-14 in page 14 and the conclusion about calving rates prior 1991. The ungrounded condition has been permanent since 1991? How much changed the ice elevation in this period? How did you include ice elevation in the calving fluxes since 1991? Only assuming that was floating? These questions arise from the lack of proper description of results and proper discussion. We are supposed to be in methods and modelling.

Thank you, please see previous comments. We have re-written our methods, and have re-organzied to improve clarity.

<u>**Historical surface melt**</u>

1) I presume DEBM is distributed energy balance model. If yes, say so.

Yes. Amended in text (lines 7 - abstract).

2) I think there is a problem with the units here. Shea et al 2013 is talking about values of 5.17 to 7.25 mm w.eq./m, and you are talking about b1= 6.62 m w.eq./m . With your gradient the mass balance is amazingly out of any possible range. By the way, the data in Figure 7 seems to have an exponential and not lineal trend. Discuss this.

Correct. This was/is a typo; it should read "mm (w.eq.)/m"
Regarding the exponential trend we observed, given we have no way to verify whether other years followed this same exponential pattern and that the linear rates derived from Shea 2013 have proven successful at modelling summer balance, we feel the best way to account for this uncertainty is to include it in our estimate of the uncertainty in the ELA (which we set at 22%/, or 75 m). (pg 2929, line 15-16).

3) Page 15 lines 9-11. ELA determination. This is a good example of the organization problems in this manuscript. Several pages before you gave the results of ELA changes (Figure 3), and only now you describe how you measured this.

Thank you for your comment - we have extensively re-organized our Methods, and moved much of this section to the appropriate sections in Results. Please see above comments.

Results, Discussion and Conclusions

After all the above comments, I think the authors must re-write most of the previous text, especially by re-organizing these sections, otherwise the following parts will not be very clearly understood. A new version is needed before going into more details that need to be presented in the following chapters.

We are grateful for all the suggestions made to date, and thank the reviewer for their perseverance through our structurally challenging manuscript. We have amended and re-organized the entire paper as suggested, and hope that the new organization has significantly improved the paper.

We look forward to any recommendations that you would be willing to provide for the remainder of the text.