The Cryosphere Discuss., 9, C1133–C1138, 2015
www.the-cryosphere-discuss.net/9/C1133/2015/

The Cryosphere
Discussions
Open Access

Interactive comment on "The relative contributions of calving and surface ablation to ice loss at a lake-terminating glacier" *by* M. Chernos et al.

Anonymous Referee #2

Received and published: 3 July 2015

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 measurements 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 addressed 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.

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 accumulation 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

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. 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. 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. 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. 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. 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.

Introduction

1) I suggest that in the introduction they quote more recent and more closely related to the study area papers when giving examples. 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. 3) The last paragraph of the introduction is almost a repetition of the text previously presented. Maybe they can delete this part.

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. 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 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? 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.

Field methods

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

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. 2) In Point 4.1 you again mix method descriptions with results (we estimate that ice loss is less than 10%...) 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. 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. 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. 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. 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. 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. 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? 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? 11) In line 22 page 12. Please describe the used method. Clausius - Clapeyron? 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?

Modelling calving flux

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. 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. 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. 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. 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.

Historical surface melt

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

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.

Interactive comment on The Cryosphere Discuss., 9, 2915, 2015.