Reply to the reviews of our manuscript "Brief Communication: Global glacier mass loss reconstructions during the 20th century are consistent"

We would like to thank Hilmar Gudmundsson for obtaining the reviews, and we would like to thank both reviewers for providing their constructive and very helpful comments on our manuscript. We were able to address all their points (as detailed below).

Response to reviewer 1:

Specific comment:

1. **Comment:** *Section 3.2 should be expanded from a short paragraph to ∼3 paragraphs. The authors should start by pointing out the regions where the match between different methods is good (and has been previously), explaining why. They should then discuss locations where mismatches have now been resolved (this is the current focus of the section and it should be retained). They should finish by explaining where the match remains poor, and why. It would be helpful to make suggestions here (e.g., we don't know why, or more/better mass balance measurements or further updates of RGI, are required in region x, climate data needs more evaluation in region y, satellite data have limited usefulness in region z providing an indication of the greatest needs and priorities). What are the other sources of uncertainty and mismatch? Dealing with surface debris cover? Iceberg calving? Some of the discussion that I propose here may be speculative and the authors can make this clear ("we speculate..") but providing this short "road map" will help the field to move forward. I am confident that the authors have the knowledge to write this short section.*

   Reply: We expanded Sect. 3.2 significantly along the lines suggested by the reviewer.

Technical Corrections:

1. **Comment:** *Title, abstract and paper body. The authors refer to Global glacier mass loss. Loss should be replaced with change. Even though the overall pattern is of glacier mass loss, the rate of loss has changed through time (and regionally, there have been periods of mass gain).*

   Reply: Done – except for those occasions where we really mean a mass loss.

2. **Comment:** *Pg 3808: Line 1. Use "Recent estimates..." and remove "that were published in recent years".*

   Reply: Done.

3. **Comment:** *Line 19. "However" and "this" shouldn't be used at the start of a new paragraph. This sentence (lines 19-21) needs to be rewritten.*

Reply: Done.

4. **Comment:** *Pg 3809: Lines 4-6. Avoid starting a sentence with "But", and clarify the meaning of this sentence. It is confusing.*

   Reply: Done.

5. **Comment:** *Lines 26-29. Please split into two or more sentences.*

   Reply: Done.

6. **Comment:** *Pg3811: Line 8 (and elsewhere in paper): "Pentadal" is not in common usage in the English language. It would be straightforward to replace this with "5-year" and it wouldn't take up much more space.*

   Reply: Done.

7. **Comment:** *Pg3811: Line 18. "Note that the results presented in this paper as Leclercq". Rewrite in active voice. "Note that the results that we present as Leclercq 2011..."*

   Reply: Done.

Response to reviewer 2:

General comments:

1. **Comment:** *No numbers/results are given in the text. Although everything is put together in Table 1 and Figure 1, I have the impression that it would facilitate the reading of the paper, if some key results / some key comparisons between previous and updated estimates were given in the Results section. E.g. by how how did the estimates change with the re-assessment?*

   Reply: We agree that readers will be interested in individual differences resulting from the updates. It is, however, hard to foresee what numbers will be most interesting, and picking examples seems arbitrary to us. For this reason (but also because of its more general usefulness) we added a data supplement, including the data shown in Figures 1 and 2, in both their old and updated versions.

2. **Comment:** *In agreement with the first reviewer I also suggest that the analysis/the discussion of the regional differences should be extended. This might point out the regions on which to focus in future studies.*

   Reply: Please see our answer to reviewer 1.

3. **Comment:** *The results indicated that the model yielded too high mass loss for certain regions due to an oversensitivity. It would be interesting to shortly discuss whether this pattern might also have affected the future projections performed by the same model (Marzeion et al., 2012).*

   Reply: We agree. There are small differences in the projections, but they are very small and negligible compared to the dominant uncertainty,

which for projections is the spread of the climate model ensemble used to force the glacier model. We added this to the discussion.

Detailed comments:

1. **Comment:** *page 3813, line 12: I do not fully agree with this statement. The RGIv4.0 still uses the ASTER GDEMv2.0 for the Russian Arctic and Svalbard, i.e. the same source of information as Marzeion et al. (2012). For Greenland, the GIMP DEM is used. I suggest that this somewhat fuzzy description is clarified here (also by using the reference to Arendt et al., 2014). Nevertheless, this does not explain why the maximum elevations turned out to be so different for Russian Arctic and Svalbard.*

   Reply: Below, we show an example of a problematic case from the Russian Arctic (Graham Bell Island in Franz Josef-Land). Obviously, there are great errors in both directions in the DEM. The too low elevations do not affect the elevation range, but the too high elevations do. While we did perform sanity checks based on the elevation distribution when we automatically extracted elevation information for Marzeion et al. (2012), we did not catch cases like this one, as the areas affected were too large to be considered errors by our routine (note also that the elevation histogram of this glacier does not look unreasonable for elevations greater than 600 m, while the true elevation maximum of the island is 509 m.). In RGIv4.0, spatial filters are applied to remove noise before the elevation distribution is determined (pers. comm. from Matthias Huss, who is calculating the elevation data for RGI). Because of the scatter of the elevation errors, the spatial filtering is better suited to remove them than the histogram-based filter we applied for RGIv1.0. We explicitely mention the spatial filtering in the revised manuscript.

2. **Comment:** *Page 3813, line 16: Can the authors explain this 48% increase in area? I thought the same inventory (Rastner et al., 2012, TC) was included in both RGIv1.0 and RGIv4.0? However, the latter version of the RGI includes all connectivity levels, whereas the earlier versions did not include connectivity level II. If this is the case, this issue should be handled consistently in the re-assessment.*

   Reply: This was in fact due to the connectivity level 2 glaciers; we corrected this in the revised figures.

3. **Comment:** *Page 3814, footnote: Just the glacier length feedback is mentioned here. It is, however, counteracted by the mass balance elevation feedback (which is not explicitly included in the model). Thus, part of the effect is offset. This should also be acknowledged here.*

   Reply: We moved this into the main text and expand a bit here, because this issue is also relevant for the reviewer's question regarding the projections. We added a statement regarding the mass balance elevation feedback, as suggested by the reviewer.

4. **Comment:** *Table 1: I suggest to restructure Table 1 with a clear separation of previous results (Leclercq, Marzeion) and revised estimates. The*

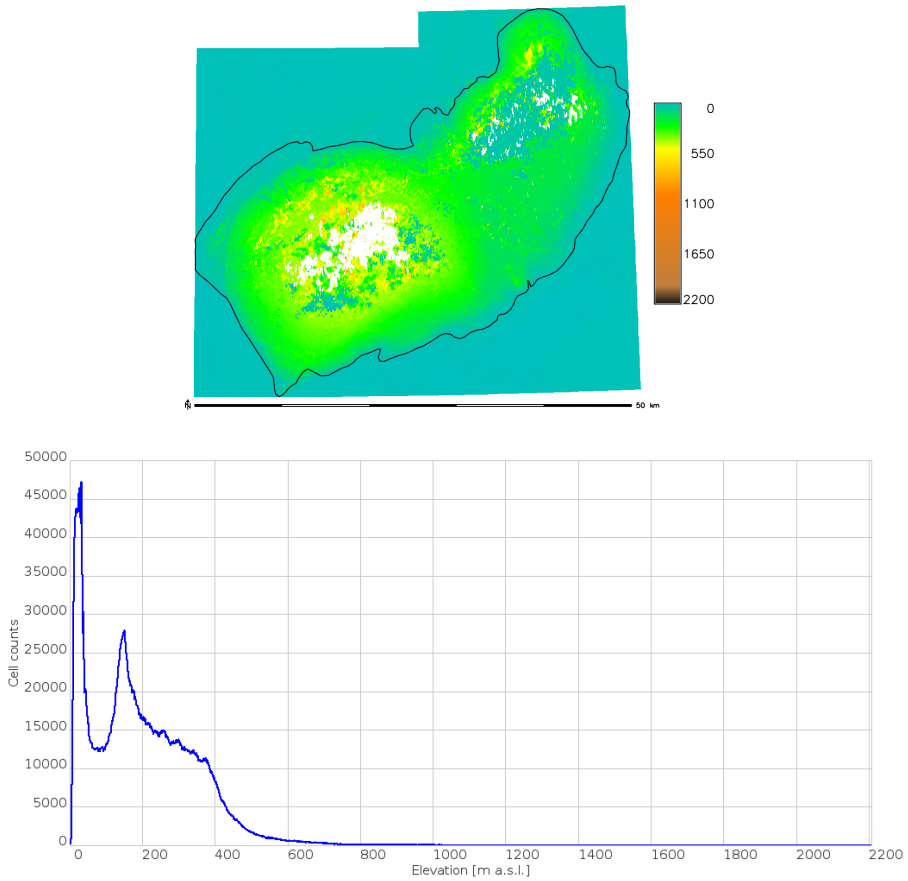

Figure 1: Example from Graham Bell Island, Franz Josef-Land. Upper panel: Black line: Glacier outline from RGIv1.0. Background shading: ASTER GDEMv2.0. White: missing data. Lower panel: distribution of elevations within the glacier outline from ASTER GDEMv2.0. The true elevation maximum of the island is 509 m.

best available estimates presently available (Cogley, Leclercq, Marzeion, Gardner) should be displayed together and not be mixed with the now obsolete numbers.

Reply: Done.

5. **Comment:** *Figure 2: Wouldn't it be easier to read this figure if results were displayed in specific units (kg m-2 yr-1) instead of mm SLE yr-1? This would allow using the same y-scale for all panels and promote the comparison of the individual regions.*

   Reply: We see both advantages and disadvantages to displaying this in specific units. The reviewer mentions the most important advantage – the most important disadvantage we see is that it becomes harder to understand how the reduced mass loss of the Marzeion et al. model shown

in Fig. 1 is regionally distributed. The differences of the glacier surface areas – both from the different RGI versions and the different temporal evolutions – further complicate the comparison based on specific units. For these reasons, we prefer to keep the Figure as it is.