

Interactive comment on “Brief Communication: Global glacier mass loss reconstructions during the 20th century are consistent” by B. Marzeion et al.

Anonymous Referee #2

Received and published: 10 August 2015

In this Brief Communication the authors present a re-assessment of different approaches to estimate the 20th century mass loss of glaciers around the globe. In contrast to the original papers about these methods, the results are now reconcilable. This is a highly welcome finding and provides higher confidence in all of the reconstructions. The update in the estimates has become possible due to the availability of new data sets (e.g. Randolph Glacier Inventory v4.0 and additional length change measurements).

The paper is relevant, it is very well written and should be rapidly published in TC. I do not see any flaws or major problems in the analysis. Some minor issues however remain to be resolved, or need a better description.

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General comments:

1. 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 did the estimates change with the re-assessment?
2. 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.
3. 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).

Detailed comments:

- 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.
- 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.
- Page 3814, footnote: Just the glacier length feedback is mentioned here. It is, how-

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ever, 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.

- Table 1: I suggest to restructure Table 1 with a clear separation of previous results (Leclercq, Marzeion) and revised estimates. The best available estimates presently available (Cogley, Leclercq, Marzeion, Gardner) should be displayed together and not be mixed with the now obsolete numbers.

- Figure 2: Wouldn't it be easier to read this figure if results were displayed in specific units ($\text{kg m}^{-2} \text{ yr}^{-1}$) 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.

Interactive comment on The Cryosphere Discuss., 9, 3807, 2015.