The Cryosphere Discuss.,
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The Cryosphere Discussions

Interactive comment on "Brief communication: *Ad hoc* estimation of glacier contributions to sea-level rise from latest glaciological observations" *by* Michael Zemp et al.

Anonymous Referee #2

Received and published: 30 October 2019

This is a nice, simple, and clear paper, but I am not convinced about the uncertainty budget (p5 line1-5). This needs to be fixed or argued better.

You are assuming that there is a very simple relationship between βavg and B. Basically a line with slope of 1 and an intercept that you estimate. I would like to see supplementary scatterplots of βavg vs B, with that line you estimate. One plot per region, with one point for every calibration year. You could also add the two ad-hoc estimates with error bars. This would be valuable because it instinctively gives you an idea if the slope=1 is a reasonable assumption, how large the scatter is, and give you an idea if you trust the ad-hoc estimates.

- paper structure, abstract, and conclusion: good.

Minor comments:

The equations are so simple that they are almost unnecessary. I would rather have an illustrative visualization explaining the method (one of the scatter plots).

p1 line 16: Why not the estimates to the abstract.

p3 line 30: Here you essentially assume that the arithmetic average of all the β's is representative of the glaciers throughout the region. However, I imagine that β will correlate with e.g. glacier size, and that the sample is not representative in terms of size (and other characteristics). I don't expect this to introduce a major bias, but the possibility should probably be considered when estimating the errors. I propose that you explicitly mention the assumptions at this point.

equation 2 and 3 (and...): It is good to be clear, but many of these equations are perhaps a bit over-verbose. These two eqns. basically just define what a mean value is.

p4 line 10: Is it a stable glacier sample? What do you mean by stable?

p4 lines 15-20 and equation 6: I assume that the 1/1000 is a unit conversion. Please remove this. The method is independent of units and we trust you to do the conversions correctly when you report the results.

p4 equation 7: Please remove the 10^6 unit-conversion multiplier.

p4 line 25: This is on the other hand important to specify.

p4 line 27: There is only one region with n.a. in table 1 — Please specify which region at this point.

p5 line 1-5: This method seems rather arbitrary. Is it really mathematically justified? E.g. this method has no consideration of how representative the sample is. Consider

also the case where the sample is complete (has the entire population). In this case you this error term should be zero (right?). What would your current 1.96x std method give?

Would it not be better to estimate the uncertainty from the residuals. e.g. using a jack-knife approach. You could withhold the data for one of the years in the 2007-2016 and see how well your method can predict the with held data. That would give you 10 residuals for each region. From these you can estimate both an RMS error and a mean bias. It would even work if your sample has only a single glacier.

p7 line 13-14: I would add "... using this method." to this sentence. Maybe another statistical treatment than your proposed method would have better performance.

equation 6: Does it have any practical effect to allow for changing area rather than just using a constant? It would make it even easier to get ad-hoc estimates, if you did not need new estimates of S.

p3 line 27: Is the sentence starting with "Over the calibration period ..." necessary? Isnt it clear it must be so, given the definition?

p2 line 4: "))"

p1 line 27: "AR 5"->"AR5"

p6 line 9: There is a limit to how far you can extrapolate the empirical calibration. The proposed scatterplots would help diagnosing the problem.

Figure S2: Please expand on the description in the caption and page 6 line24-26. It is not sufficient to understand what has been done.

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