

Interactive
Comment

Interactive comment on “Global glacier retreat: a revised assessment of committed mass losses and sampling uncertainties” by S. H. Mernild et al.

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— SUMMARY —

The paper presents a simple estimate for the mass loss glacier and ice caps (GIC) are committed to, would the current climate persist. The estimate is based on analysis of observed accumulation area ratios (AAR) and mass balance, as well as some simple theoretical consideration based on scaling theory. The paper is cleanly written and easy to follow. The methodology strikes in its simplicity and is accompanied by a clean estimate for the associated uncertainty. The supplementary material is insightful and nicely presented. All in one I have only a few comments and some minor questions on the manuscript, mainly addressing technicalities.

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— GENERAL COMMENTS —

1) For the upscaling, GIC areas from Radic and Hock (2012) are used. Wouldn't it be suitable to use the Randolph Glacier Inventory by Arendt et al. (2012), which is becoming the reference?

2) Practically all of the methodology is explained in the supplementary material, and not in the main text. This looks very much “high impact journal”-like. Personally I don't like this way of presentation very much, since it forces to flip back and forth between the two texts. I would encourage the editor(s) to have a thought on whether “The Cryosphere” wants to go towards this style as well.

— SPECIFIC COMMENTS —

P1990 L6: As you correctly note in the supplementary material, the theoretical coefficient stated by Bahr et al., 1997 is $\gamma=1.375$, and not $\gamma=1.36$, as you state here. Please reword the sentence.

P1991 L5-6: How many of these “GIC [...] for which AAR methods are not applicable” exist? And how do you cope with them in the final estimate? They are never mentioned again. . . The uncertainty estimate should account for these GIC as well, shouldn't it?

P1991 L9-10: At this stage the question arises how the “fractional change in area and volume” will be computed. From the context it becomes clear later, that you are using the equations at P1990 L4, but this is never stated explicitly. . .

P1992 L18-21: Please point at the supplementary material, in which more information about the resampling experiment is given. It may also be appropriate to state that the necessary assumption for such a procedure is that the variance within individual regions is not dependent from the regional average. . .

P1993 L26-27: At this stage, it is not clear where this statement is coming from. . .

P1994 L10: The number “38%” implies that an average γ of 1.34 was used. At

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this stage it is not clear where this number is coming from. Point at the supplementary material, where more information is given or (better), give the information in the main text.

P1994 L27: Sorry but how can a trend that contains “zero” in its confidence interval ever be “significant”?? What hypothesis are you testing?

— COMMENTS TO THE SUPPLEMENTARY MATERIAL —

L S173: Why are you using the estimate by Radic and Hock, 2010 here? It is certainly not a big deal, but it would be more consistent to use the total GIC area stated in Jakob et al., 2012 (should be stated in the supplementary there).

L S203: Why “12” and not any other number? This is arbitrary, isn’t it?

L S214-S216: For region 3, Fig. S5 shows correlations coefficients higher than 0.5 even for distances > 1500 km. How can this not be significant? And why would you like to prove “not significant” at all? Isn’t what you need an observation that is representative for a large region, i.e. an observation that correlates well also over large distances? I believe the “not” at L216 simply being at the wrong place. . .

L S362-363: Honestly I have not understood how the red dots are obtained. Apparently a standard deviation has been used (L S363) but it is not clear of what. Please clarify (by the way: the clarification should be given in the text, and not in the caption of the figure).

— STYLISTIC COMMENTS —

P1988 L26: Remove the reference to “Huss and Farinotti, 2012” – the number of glaciers given therein is simply the result of “Arendt et al., 2012” who are correctly cited.

P1989 L1: Remove “therefore”.

P1989 L5: Say “currently” (or something similar) before “raising sea level”.

P1989 L23: Well, actually even “<<1%”, isn’t it?

P1993 L15+21: What are “direct-plus-geodetic estimates”?

P1993 L25: “These two potential biases”? What would the “bias” from GRACE be?

P1994 L9: Replace “;” with “.”.

P1994 L21: Place “e.g.” before the citation.

L S53: “ α ”, not “ α ”.

L S104: “ γ ” not “. γ ” (“period” at the wrong place).

L S255: Please tell again where the number “0.70” is coming from, so the reader doesn’t need to look for it again.

L S340: State “, Swiss Alps” after “Silvretta glacier”.

L S367: Add “for the period” before “1971-2010”.

L S370: Replace “the point $y=0$ ” with the “point $(x,y)=(0,1)$ ”.

Sheet C: The header contains some “copy-paste error” in row 1 (years 1982 and following shouldn’t be displayed)

— COMMENTS TO TABLES AND FIGURES —

Fig. 4: What is causing the very negative value for 2001-2005 in method 1? Can confidence intervals be displayed for the other estimates beside method 3 as well?

Fig. 5, caption: “Grey firn areas generally lie in the ablation zone”. This sentence doesn’t make much sense to me: Firn (= “snow that has persisted at least one ablation season”, according to the “Glossary of glacier mass balance etc.”), can not lie in the ablation zone by definition. . .

Tab. S2 + S3: Please merge Tables S2 and S3 into a single one.

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Interactive comment on The Cryosphere Discuss., 7, 1987, 2013.

TCD

7, C560–C564, 2013

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