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To the Editor of The Cryosphere, Professor Jon Ove Hagen

We thank the three reviewers for their careful, insightful critiques of our manuscript, "*Global glacier retreat: a revised assessment of committed mass losses and sampling uncertainties*" (TC2013-55). Our response is attached, with reviewer comments in blue and author replies in black.

Thank you for your help. If we can be of any further assistance, please feel free to contact me.

Best regards,

Sebastian H. Mernild and co-authors

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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.

— 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?

AUTHORS: The RGI dataset in its current form is not suitable for area-volume scaling, because its digital glacier outlines in many cases delineate large glacier complexes rather than individual glaciers. (For an illustration, please see Michael Zemp's 2013 EGU poster, specifically the inset map of Novaya Zemlya: http://www.geo.uzh.ch/~mzemp/share/scratch/EGU-Poster/http://meetingorganizer.copernicus.org/EGU2013/EGU2013-2809.pdf .) Upscaling the areas of glacier complexes would overestimate the volume. However, Radić et al. (2013) did some additional analysis to separate the RGI polygons into individual glaciers, thus allowing aggregate volume estimates (see Table 1 of their paper). In the revised manuscript we replaced the regional area and volume estimates of Radić and Hock (2010) with those of Radić et al. (2013).

This replacement changed some of the global mean estimates in the text and in spreadsheets F and G. Compared to Radić and Hock, the RGI areas are larger for Greenland (which has a relatively low observed α) and smaller for Antarctica (which has a relatively high observed α). For this reason the RGI areas imply slightly higher global area losses (32% instead of 30%), but the overall conclusions have not changed.

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.

AUTHORS: We eliminated the supplementary text file, leaving only the Excel file in the supplementary material. Some of the supplementary text was moved into the main manuscript; other text was converted to appendices containing the relevant equations (e.g., for means and standard errors); and some less essential text was removed. Also, we moved four supplementary figures to the main text and one to the appendices.

— 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.

AUTHORS: Done.

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?

AUTHORS: As stated in the text, several dozen additional glaciers were excluded, mainly glaciers with fewer than 10 total observations. Since we did not have reliable estimates of α for these glaciers, we regarded them as unobserved, like most of the world's glaciers. Our uncertainty estimates depend on the number of observed glaciers, so the uncertainties are simply a bit larger than if we had been able to include some additional glaciers.

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…

AUTHORS: We inserted "p_A" and "p_V" after "area" and "volume", respectively, to makes this more clear.

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…

AUTHORS: We inserted a reference to Appendix B, which contains more information (formerly in the supplementary material).

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

AUTHORS: We added a reference to Fig. 8 (formerly Fig. S4), which provides a basis for this statement.

P1994 L10: The number "38%" implies that an average /gamma of 1.34 was used. At his 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.

AUTHORS: We pointed to Appendix B, where the details are given. Appendix B states, "Regions with relatively few ice caps (less than 1% of the total number of GIC in the regional inventory) were assumed to have most of their volume contained in glaciers. For these regions we assumed $\bar{\gamma} = 1.36 \pm 0.02$, where the error corresponds roughly to the difference between the observed value of 1.36 for valley glaciers and the theoretical value (Bahr et al., 1997) of 1.375. For regions where at least 1% of the GIC are classified as ice caps, we assumed $\bar{\gamma} = 1.31 \pm 0.05$ to reflect an uncertain partitioning of volume between glaciers and ice caps." These are the values used in Sheet G of the Excel file. Since the estimated global volume loss and its uncertainty are relatively insensitive to the exact value chosen for γ in each region (provided

$1.25 < \gamma < 1.375$), we decided to leave these details in the appendix rather than move them to 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?
AUTHORS: We are testing the hypothesis that the trend is significantly different from zero. Using a *t*-test based on the number of data points and the correlation coefficient, we can confirm this hypothesis with 99% confidence. This statement of confidence cannot be derived from the standard error. The stated error range is 1.96 times the standard error, consistent with other ranges given in the text. We added text to state that the significance of the trend is based on a *t*-test.

— 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).
AUTHORS: In the revised manuscript we removed this estimate and the associated reference to Jacob (2012). We thought it more appropriate to use the recent Gardner et al. (2013) paper as evidence that the directly observed mass balances may have a negative bias.

L S203: Why “12” and not any other number? This is arbitrary, isn’t it?
AUTHORS: Yes, a number > 10 is arbitrarily chosen, but the error does not decline significantly for subsample sizes of more than 10 glaciers. Any number from 11-14 would not make a large difference in the error estimate. We now make this point in Appendix B.

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…
AUTHORS: We think that the reviewer missed our point here, and therefore we improved the explanation of why we are doing this correlation analysis (see Appendix B). Of the three methods for determining the global imbalance of glaciers, method 3 is the most robust, but still suffers from some shortcomings in that it assumes measurements within a region are representative of the region as a whole. This is likely the case for smaller regions (e.g., Svalbard) but not for larger regions (e.g., Greenland, Antarctica, and Arctic Canada) that experience a range of climates. Given sufficient spatial sampling, it would not be a problem to assess the error associated with this assumption. However, when regional AAR for large regions is inferred from 1-3 records that are often located near one another, the subregion with significantly correlated records may not be representative for the entire region.

Thus we needed to simulate a situation where a hypothetical large region contains AAR records from glaciers with large spatial separations (~1000 km), whose AAR records are not significantly correlated. Such test regions then yield representative errors for the large regions with sparse records in reality. Our goal with the correlation analysis presented in Fig. 6b (formerly Fig. S3b) was to demonstrate that in our test regions (with a sufficient number of data points), AAR records separated by large distances **are not significantly correlated** with one another. Although there are a few exceptions of correlations > 0.5 for glacier >1500 km apart, the overall result is that the correlation among AAR records is weak for large distances. Thus our test regions are representative for the error assessment and appropriate for determining errors for large regions with sparse AAR measurements.

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).
AUTHORS: We improved the explanation of this method in the text (see Appendix B). We calculated regional mean α for each region using a subsample size of 1 to 14 glaciers within a region. For subsamples of 1 glacier: regional α is equal to α from each glacier, and therefore this subsample gives the largest range of possible regional values. For subsamples of 2 glaciers: all combinations of 2 glaciers are used to calculate the arithmetic mean for regional alpha. For subsamples of 3 glaciers: any combination of 3 glaciers is used… and so on up to 14 glaciers. In Fig. 6 (formerly Fig. S3) we present the maximum range of those results (i.e. derived regional α minus the reference α – where the reference α is the arithmetic mean from all glaciers in the region, e.g. 20 glaciers). So, the range is greatest for subsample size 1 and slowly decreases as we approach a subsample size of 14 glaciers (and would reach zero for the sample of 20 in this case). The standard deviation of these values is calculated for each subsample size, and we present the 95% confidence interval (1.96 * standard deviation) in Fig. 6.

— 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.
AUTHORS: We removed the reference.

P1989 L1: Remove "therefore".
AUTHORS: Done.

P1989 L5: Say "currently" (or something similar) before "raising sea level".
AUTHORS: Done.

P1989 L23: Well, actually even "«1%", isn't it?
AUTHORS: We changed "fewer" to "much fewer".

P1993 L15+21: What are "direct-plus-geodetic estimates"?
AUTHORS: These are the estimates of Cogley (2012), combining glaciological (direct) measurements with geodetic measurements. We added a reference to Cogley (2012) to make this more clear.

P1993 L25: "These two potential biases"? What would the "bias" from GRACE be?
AUTHORS: We replaced the GRACE reference with a reference to Gardner et al. (2013), who found that direct local measurements are likely biased toward more negative mass balances.

P1994 L9: Replace “;” with “.”.
AUTHORS: Done.

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

L S53: “α”, not “$alpha$”.
AUTHORS: Done (in the title of what is now Appendix A).

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

L S255: Please tell again where the number “0.70” is coming from, so the reader doesn’t need to look for it again.
AUTHORS: We added parenthetically that this is “the global mean value estimated for 2001–2010, given in Section 3”.

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

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

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

Sheet C: The header contains some “copy-paste error” in row 1 (years 1982 and following shouldn’t be displayed)
AUTHORS: Nothing is wrong here. The “Freeze panes” were erased from Sheet C, which created the confusion.

— 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?
AUTHORS: The negative 2001–2005 mean value is explained in Section 2: “Method 1 (the mean of all observed GIC) gives a post-2000 mass balance more negative than the published estimates, suggesting a bias due to high melt rates in over-represented low-mass regions.”

We chose not to add confidence intervals for the other estimates because these intervals would be based on the questionable assumption that the observed glaciers are globally representative. The resulting confidence intervals would be misleadingly narrow.

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: : :

AUTHORS: During years like this one, with an anomalously negative mass balance, firn from previous years can lie in the ablation zone. To make this more clear, we rewrote the caption as follows: "…The 2008 glacier mass balance is -1653 kg m^{-2} yr^{-1}, and the AAR is 10%, with net accumulation limited to small white patches of remaining snow. Grey firn areas (i.e., snow from previous years) generally lie in the ablation zone, as does the bare (blue) ice."

Tab. S2 + S3: Please merge Tables S2 and S3 into a single one.
AUTHORS: We deleted Table S2, since the information is shown graphically in Fig. 7 and the numbers are given in Sheet D. We also deleted Table S3, since the information in this table appears in Column D of Sheet H.

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Mernild et al. use observed accumulation area ratios to (i) determine future glacier mass losses caused by present climate, but not yet realized, and (ii) to estimate the influence of the sampling bias (most observations of glaciers in regions with a small fraction of global glacier mass). It was a pleasure to read the paper, as it is well written and presents the concept in a very accessible way.

The authors also do a great service to the community by including a complete and very well-described data set in the supplement. But the comprehensive supplement also comes at a cost, since the readability of the paper is somewhat lowered by frequent references to (especially figures in) the supplement. In some places I also had to refer to the supplement without explicitly being pointed there. Generally speaking, I would have preferred more strict limitation of the text in the supplement to the description of the data, including the figures and description of procedures relevant to the main manuscript within the manuscript itself (or perhaps in an Appendix).

AUTHORS: We agree, and we significantly rearranged the manuscript. Please see the reply above to D. Farinotti (general comment #2).

However, I don't see this as a major problem, and I only have a few questions/ suggestions below.

Specific questions/suggestions:
• Fig. 3: I think it would be good if somewhere in the paper the mass glaciers are committed to loose would be in a Figure. Wouldn't it be possible to add a vertical axis on the right side to this plot, showing globally committed mass loss – based on one of the three averaging methods (probably method 3)? This would also be helpful in giving an impression how strong the temporal variability of this number might be, and to what degree the difference between this and the BDM estimate is due to the more recent reference time or due to the generally increased data basis.
AUTHORS: We think it is best to leave Fig. 3 as it is, with annual averages from methods 1 and 2. The data are insufficient to apply method 3 before 2001. The committed area loss (though not the mass/volume loss) is a simple linear function of α and can be read from Fig. 3, giving the reader a sense of temporal variability.

• To estimate future values of α the past trend is extrapolated to the future. The number of glaciers with α =AAR=0 increases over time, but α < 0 is not possible. From this, I would expect (just roughly speaking) that the sensitivity of α to temperature increase is smaller for higher temperatures. (Or in another way: Doesn't this approach contradict the rational of excluding glaciers with AAR=0 or AAR=1 from the analysis?) I am not sure if and how this impacts the extrapolation, but I would appreciate if the authors could discuss this.
AUTHORS: This is true if the trend is extended for many decades. Over 35 years, however, most existing glaciers, including 93 out of 96 in the 2000-2010 data set, will continue to have $\alpha > 0$ (assuming the trend does not accelerate). The three glaciers projected to have $\alpha = 0$ by 2040 are in low-mass regions (Gries and Careser in Central

Europe, and Riukojietna in Scandinavia) and thus have a negligible impact on the extrapolation. We added text to this effect in Appendix A.

• The authors are using Radic & Hock (2010) for both dividing the regions into high mass and low mass, and for the regional calculations. Is there a reason not to use the more recent, and more complete Randolph Glacier Inventory? I doubt it would change the results much, but given the effort that went into creating that data set, why not use it.

AUTHORS: Please see the reply above to D. Farinotti (general comment #1).

• page 1990, line 6: shouldn't it be $\gamma = 1.375$?

AUTHORS: Yes, we corrected this.

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Mernild et al (2013) provide a utilitarian approach to assessing global alpine glacier mass balance. The WGMS has generated plots of AAR versus mass balance for many glaciers in the Glacier Mass Balance Bulletin series that indicates the utility of AAR for mass balance assessment. Currently the ubiquity of satellite imagery will allow for AAR determination on most glaciers. The advantage of AAR over ELA is that the ELA is hard to assess on many smaller alpine glaciers and is a less universal number. The AAR method also allows assessment for individual glaciers that GRACE does not. ICESat will likely not be as useful for annual mass balance assessment when it comes online.

The title uses the word Global glacier retreat, but retreat is not the key parameter, mass balance or volume is and key input is AAR, both mass balance and AAR should be it title. The comments below are mainly minor, generally asking for more details that will better support the methods and results.
AUTHORS: We agree that readers could be misled by the word "retreat" and changed to the more general title "*Global glacier changes: A revised assessment of committed mass losses and sampling uncertainties.*" Since the title mentions committed mass losses, it implies more negative AAR and mass balance.

Specific Comments:
1990-1: This statement is true, except for glaciers with a frequent AAR of 0, indicating no accumulation zone and no point at which retreat can yield equilibrium (Pelto, 2010). The number of disappearing glaciers is significant and this response for glaciers without a consistent AAR above 0 must be briefly acknowledged.
AUTHORS: We added a sentence acknowledging this extreme case.

1991-6: What is the breakdown of linear relationship, for example 20% from 0.1 to 0.3 etc.
AUTHORS: We are not sure we understand the comment. If the reviewer is referring to the significance threshold, we would reply that $p < 0.1$ is arbitrary but standard, and that most glaciers in the data set easily surpass this threshold, with 111 of 144 glaciers having $r^2 > 0.7$. Readers can refer to Sheet B in the supplementary Excel file for details.

1991-14: Reword- "indicating that the observed GIC are further from balance than previously.
AUTHORS: Done.

1993-25: I do not understand how the second item is a bias. GRACE is just a separate measurement platform that experiences its own issues. This has been noted in the Himalaya but also by Gardner et al (2013). For the Himalaya GRACE has yielded three different values in the last three years -47 Gigatons/a (Matsuo and Heki, 2010), -4 Gigatons/a (Jacobs et al, 2012), -26 Gigatons/a (Gardner et al, 2013).
AUTHORS: We removed the GRACE reference in this paragraph and replaced it with a reference to Gardner et al. (2013), whose results imply that directly observed glaciers may have a bias toward more negative mass balance.

1993-25: At some point you should also contrast your results to the just published Gardner et al (2013) paper, Table 1 in their paper particular has some useful comparative material. The Gardner et al (2013) assessment should also be added to your Figure 4.

AUTHORS: We agree. We cited this paper several times in the revised text: in the introduction, in Section 2 (as a third published estimate of global mean glacier mass balance), and in Fig. 7 (formerly Fig. 4). Our upscaled mass balance (using method 3) for the past decade is 100 to 150 kg m^{-2} yr^{-1} more negative than the Gardner et al. estimate of 350 ± 40 kg m^{-2} yr^{-1} for 2003–2009.

1996-16: This point deserves more attention. For example the NIWA end of summer snowline survey identifies the snowline elevation on 50 glaciers, these observations can then be used to derive AAR for all the glaciers. Also the daily availability of so many satellite imagery sources allows for identification of AAR on most glaciers annually, this can yield such a glacier specific rich database. The use of transient snowlines observations to help identify ELA and hence AAR is also worth a brief mention.

AUTHORS: We agree that there is considerable potential for expanding the AAR database using aerial and satellite imagery. We think the text expresses this point adequately: **"**Direct mass-balance and AAR measurements are inherently labor-intensive and limited in coverage. AARs can be estimated, however, from aerial and satellite observations of the end-of-summer snowline (e.g., Fig. 9 and Rabatel et al. (2013))." Readers interested in more details can refer to the new Rabatel et al. manuscript, to which we added a reference.

Figure 5: Better illustrate the firn, bare ice and retained snowpack.

AUTHORS: We modified the caption to read, "…The 2008 glacier mass balance is −1653 kg m^{-2} yr^{-1}, and the AAR is 10%, with net accumulation limited to small white patches of remaining snow. Grey firn areas (i.e., snow from previous years) generally lie in the ablation zone, as does the bare (blue) ice."

Figure 6: Does not exist yet. It would be useful to provide a good example of a larger glacier too just like in Figure 5, such as Lemon Creek Glacier, AK or Wolverine Glacier, AK, side by side with satellite image of AAR on the same glacier.

AUTHORS: We would prefer not to add another photo to the paper. However, we added a reference to Rabatel et al. (2013), which includes a Landsat TM5 image (Fig. 2 in that paper).

Supplement-data file: Why does the data table contain glaciers that are not used, why not remove these?

AUTHORS: All 144 glaciers in the data table are used for calculations cited in the manuscript.