

**Reply to reviewers comments of referee #2 on “An assessment of uncertainties in using volume-area modelling for computing the twenty-first century glacier contribution to sea-level change”, August 2011**

Dear Referee #2,

Thank you very much for commenting on our manuscript and your constructive and valuable response to our work, which we think has really improved the manuscript. You will find the point-by-point response to each of the substantive comments below.

Kind regards,  
Aimée Slanger and Roderik van de Wal

*General comments:*

The paper discusses the uncertainties in one approach of modeling GIC contribution to sea level rise in the 21st century. This approach relies on glacier mass balance sensitivities, here derived as a function of precipitations. As shown in the results, the uncertainties in the global projections are highly sensitive to the uncertainties in mass balance sensitivities. My impression is that this uncertainty assessment needs better quantification than is currently presented.

The organization of the paper and use of language are proficient. Nevertheless, there are some issues that are not clearly explained and need more elaborations or details provided (such as, how the volume-area scaling is applied in the regions with incomplete glacier inventories). More details about these and some other concerns are provided below, including a few specific comments. I recommend the paper for publication after the revisions are applied.

*Specific comments:*

Page 1657, Ln 16: In Bahr et al (1997) the volume-area scaling is derived theoretically and the empirical relationship was only used for validation. Thus, not all methods use empirical relations derived for small set of glaciers. Please correct.

We have changed the sentence to: ‘These methods use empirical relations derived for a small set of glaciers, which are extended to a global scale. This is supported by theoretical analyses by Bahr et al. (1997).’

Page 1957, Line 17: It can be added to the sentence: the required mass balance changes may be obtained: : : by applying a simplified mass balance model (Radic & Hock, 2011)

*Added.*

Page 1959, Lines 17-18. While volume-area scaling for mountain glaciers is empirically derived from a set of glaciers (or validated), this has not been done for the ice caps. The volume-area scaling for ice caps is derived from Paterson (1994), using a simplified circular based ice cap.

The reference is added: ‘For ice caps,  $\gamma$  is set to 1.25 and  $c$  to  $1.7026 \text{ m}^{3-2\gamma}$ , assuming an ice cap with a circular base (Paterson, 1994).’

Therefore, the scaling parameters for ice caps have not actually been tested against the real data (as is the case for mountain glaciers) and I would suggest to also applying sensitivity experiments for the ice caps as it is done here for the glaciers.

We have performed these tests, and they show that changes in the scaling factor of the ice caps of  $\pm 25\%$  lead to changes of  $\pm 0.5\%$  in the total contribution of GIC, which

is very small compared to changes in the glacier constants. A line is added to Sect. 2.1: 'These ice cap values are kept constant throughout the study, because variations in  $c$  of  $\pm 25\%$  were found to lead to very small variations of  $\pm 0.5\%$  in the sea-level contribution.'

Additionally, how much do the results (global projections) change if all glaciers (including ice caps) are treated as mountain glaciers in the volume-area scaling?

To see how the choice for the glacier/icecap ratio influences the resulting GIC contribution for the 1990-2090 period, we have performed a few extra tests using various glacier/icecap-ratios, with the following results in terms of GIC contribution over 1990-2090:

1. Standard run; 11% icecap vs. 89% glacier:  $0.149 \pm 0.022$  m SLE
2. Keep WGI-XF ratio in upscaling: 19% icecap vs. 81% glacier:  $0.149 \pm 0.021$  m SLE
3. Treat all GIC as glaciers, 100% glacier:  $0.150 \pm 0.022$  m SLE
4. Treat all GIC as icecaps, 100% icecap:  $0.144 \pm 0.019$  m SLE

From this we can conclude that the decision to treat ice as glaciers or ice caps has a relatively small influence, 0.06 m or 4 % in the most extreme case – case 3 vs case 4. The choice to treat the upscaled ice as glacier -case 1- instead of keeping the WGI ratio the same –case 2- has only a very small influence on the final GIC contribution.

This is added to §2.2 as follows: 'It is assumed that the entire upscaled area consists of glaciers, which slightly changes the ratio of glaciers and ice caps. However, tests show that this influence is negligibly small.'

Page 1660, Equations 3 & 4 These functions are originally derived for a set of glaciers using local precipitation observations. The authors here only use AOGCM's temperature and precipitation, which are unable to represent the local climate. Since downscaling of AOGCMs is not mentioned in the text I assume the data is directly taken from the interpolated gridded AOGCM?

Yes, this is mentioned in the sentence following Eq.3 and 4.

If so, this should be mentioned here as well as the implications of this in the results and the uncertainties. The authors mention later on Page 1675 in the conclusions that AOGCMs temperature and precipitation is possibly not representative for the glacierized area, however, this needs more attention.

We have added the sentence: 'This introduces an uncertainty in the GIC contribution to sea-level change which will be treated in Sect. 4.'

Since their results are very sensitive to the defined mass balance sensitivities (as functions of precipitation) the bias in precipitation needs to be quantified. In Chapter 4.1., the guess of 20% error in precipitation may still be on the low bound for most of the grid cells where the glaciers are located. I think that the proper quantification of these biases would strengthen the currently presented error analysis (see Jarosch et al, 2010, Journal of Climate, as an example where even the reanalysis data heavily underestimates the precipitation in higher elevations). I would suggest comparing observed precipitation vs gridded precipitation from AOGCM for the glaciers with available precipitation observations in order to get better assessment of error bounds and the propagation of this error in the mass balance sensitivities and global estimates.

Van de Wal and Wild (2001) carried out a similar comparison and showed that the bias is not onesided, and the errors tend to cancel each other out. Additionally, we

would like to mention that the precipitation is incorporated into the mass balance sensitivity. The precipitation uncertainty of 20 % has a smaller effect -12%- than the mass balance uncertainty of 20% -17%- . Hence, the uncertainty of the mass balance sensitivity studied in §4.1 incorporates a precipitation uncertainty that is larger than 20%. So we believe that we have made a reasonable estimate of the mass balance uncertainty.

Page 1661, Line 11 'upscaled' version: :: better use: 'extended' or 'updated' version  
**Changed to 'extended'.**

Table 3, Should be mention in the captions what D and E are.

**Good point. A description has been added.**

Page 1663, Line 10. Locations of GIC in Word Glacier Inventory (Cogley, 2009) are specified. It is only the choice of this method not to use the locations of the individual glaciers.

**True. To emphasize this in the revised version, the sentence has been rewritten:  
‘This procedure is necessary as the locations of the upscaled GIC in R10 are only known by region.’**

Page 1663, Section 3.1. To my knowledge a crucial point here is not explained and that is: how is the total ice volume estimated for the regions that do not have a complete glacier inventory? For example, Radic&Hock 2010 showed that 9 regions have incomplete glacier inventory (see Figure 2 in R10). They also presented a method for upscaling the regional volume but noted that 'We circumvent the need to know the number of both mountain glaciers and ice caps per region by upscaling glacier volumes as a function of glacierized area missing in WGI-XF.' However, if the size distribution (the upscaled number of glaciers per size bin) is not known, how is the volume-area scaling applied? To my knowledge, volume-area scaling is meant to be applied on each glacier individually and therefore the total (upscaled) number of glaciers per region (and per size bin) should be known. If the volume-area scaling is applied only on the glaciers from the inventory (data from R10), what is done for the remaining glaciers that are missing in the inventory in order to get the global estimates? Please provide more details on this issue.

**The reviewer is right, this is not accurately explained in the manuscript. Indeed, in Radic and Hock (2010), the size distribution is only known for the 9 regions with a complete inventory. However, for the volume-area model information is needed on the number of glaciers and their area per size bin. To obtain this information for the incomplete regions, we have used equation (5) of Radic and Hock (2010), and the total upscaled area per region as provided in their Table 2. A step-by-step explanation of the method:**

1. For each incomplete region, the glaciers and ice caps are divided into size bins, giving the number of glaciers and icecaps and their average area per size bin.
2. By summing up the glacier area (number\*area) and the icecap area (number\*area), we calculate the total area per size bin before upscaling.
3. Using equation (5), with  $m=M-2$ , we calculate the upscaled area needed per size bin to reach the total upscaled area given in Table 2. In this step, glaciers and ice caps are still combined.
4. We now assume that all the added area is glacier area, and that the average glacier area per size bin, calculated in (1), does not change. We now divide the upscaled area per size bin by the average area per size bin, resulting in an upscaled number of glaciers per region and size bin.

In the manuscript, we have added a short version of the above to section 2.2: "Of the remaining 17 regions, 7 regions have an incomplete glacier inventory. To complete

these regions, an upscaling procedure is performed as described in Radic and Hock (2010). Then, to obtain information on the new number of glaciers per size bin, we divide the upscaled area by the average area per size bin before upscaling. It is assumed that the entire upscaled area consists of glaciers, which slightly changes the ratio of glaciers and ice caps. However, tests show that this influence is negligibly small. "

Page 1664, Section 3.2. In the estimates of past sea-level contribution is the precipitation in equation (5) kept constant?

Yes.

If so, are the mass balance sensitivities also kept constant?

Yes.

It would be interesting to see how much variability in mass balance sensitivity there is when allowing the precipitation to change, using for example, precipitation from AOGCM (20th century runs).

This would indeed be interesting. However, it would require quite large changes in the set-up of the model. Additionally, the results would probably not lead to large changes in the GIC contribution, as in Sect. 4.1.3 it was shown that different options for the imbalance before 1990 do not have a very large impact on the future GIC contribution. We therefore chose not to perform this experiment now and leave it to future work.

Also, in the future projections it would be interesting to use only temperature input (neglecting precipitation change) and see how that impacts the global projections.

Neglecting the precipitation changes results in a GIC contribution of  $0.150 \pm 0.020$  m SLE, which is marginally different from the GIC contribution when precipitation changes are included.

Page 1666, Section 4.1. As mention in my earlier comment, the error of 20% in precipitation might be on a lower bound. This should be better investigated, especially since it is the dominant uncertainty in the projections (see my previous comment on this).

[See response to previous comment.](#)

Page 1667, Lines 14 -20. This explanation is not clear to me. First of all, the definition of equilibrium state would be necessary here.

We have rewritten these sentences to clarify that the equilibrium state meant here is disappearance of the glaciers.

In Figure 4, the authors are presenting the change of total volume (in the size bin) in time and this is not equivalent to volume evolution of a single glacier in response to climate perturbation.

The volume-area model uses size bins, with information on the amount of glaciers and their average area to calculate the volume changes. This means that we do not have this information on each glacier individually, but only per size class. To simulate the evolution of 1 single glacier, the volume evolution as shown in figure 4 should be divided by the number of glaciers in that class, thus leaving the shape similar to that in figure 4.

Secondly, wouldn't the reason for different response in  $V_i$  and  $dV$  to variations in  $c$  be found in cancellation of biases, ie. for some individual glaciers the ice volume and volume change would be overestimated and for some underestimated meaning that for the large enough sample of glaciers some of these biases would cancel out? This cancellation of biases would

be different for initial volume and volume change, due to non-linear nature of volumearea scaling.

The over- or underestimation of glacier volume and volume change would be important when considering individual glaciers, and when looking for the best estimate of the contribution to sea-level change. We agree that the cancellation of biases is different for  $V$  and  $\delta V$ . However, we do not see how this would cause a difference in the response of  $V$  and  $\delta V$  to changes in  $c$ .

Finally, Raper & Baithwaite (2006) and Radic & Hock (2011) used scaling relationship (coupled with mass balance model) in a way that would allow each individual glacier to reach new equilibrium (in the changed climate) and therefore they required glacier hypsometry as part of the input data. I do not see how the discussion here relates to their reasoning about the new equilibrium state, and/or how this approach deals with changes in glacier hypsometry. Please include this into your discussion.

**The volume-area model as used in our study can reach no other equilibrium than total disappearance, which is a problem when considering small temperature changes on a long period. However, in this case we apply a significant temperature change over a relatively short period, in which you would not expect the large glaciers and ice caps to reach a new equilibrium state. As shown in Raper and Braithwaite (2006), equilibrium will only be reached in a couple of 100s of years. We therefore think that the volume-area approach gives a reasonable result for the global GIC contribution. A few sentences discussing this have been added to §4.1.2.**

Additionally, I do not see why the uncertainties due to variations in the scaling exponent ( $\gamma$ ) are also not tested in this study. Radic et al (2007) have tested this on generic glaciers and their results (in terms of individual volume evolutions) might not be directly comparable to this approach. It would be interesting to see in the light of this study how sensitive the results are to variations in  $\gamma$ .

**Variations in gamma lead to similar results as the tests for  $c$  described in section 4.1.2. We have removed the reference to the Radic et al., 2007 paper, and instead added the following:**

**'We have performed a similar sensitivity study on the other parameter in Eq.(1),  $\gamma$ . Available estimates for  $\gamma$  are 1.375 (Bahr, 1997), used in this study, and 1.36 (Bahr et al., 1997). The range over which  $\gamma$  is tested is based on the difference between the two estimates: 1.345 to 1.405 with steps of 0.015. From this we also find that  $V_i$  is more sensitive to the choice of the scaling exponent  $\gamma$  than  $\delta V$ , which is fairly insensitive to variations in  $\gamma$ .'**