Reply to reviewers comments of G. Cogley 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 Prof. Cogley,

Thank you very much for commenting on our manuscript and your constructive and valuable response, which we think has really improved the manuscript. You will find the point-by-point response to each of the substantive comments below. The stylistic comments are all applied in the new manuscript, many thanks for pointing them out.

Kind regards,
Aimée Slangen and Roderik van de Wal

General Comments
This paper has a self-explanatory title. It addresses several, but not all, of the uncertainties that are likely to affect projections of sea-level changes due to glacier mass loss over the 21st century. The leading sources of uncertainty are found to be the specification of mass-balance sensitivity to climatic forcing, the inadequacy of basic information about individual glaciers, and (potentially) failure to allow for the fact that glacier responses lag behind the forcing. Lesser contributions are made by the volume-area scaling factor (the scaling exponent not being considered) and by discrepancies between the climate models from which the forcing is taken. Sources of uncertainty that the paper does not consider, although they are mentioned in the Conclusion, include "scenario uncertainty", the behaviour of tidewater glaciers, and "measurement uncertainty" in a sense broader than that implied by the paper's comparison of two different glacier inventories. The authors estimate that the sources that they study combine to give a total uncertainty of about ±30% for the glacier contribution to sea-level rise over the period 1990-2090.

I found the paper to be well written and well executed, interesting and valuable. I recommend that it be accepted for publication subject to revision by the authors in light of the detailed comments below.

Substantive comments:
P1656
L7 Clarify. Most volume-area formulations have two parameters (sometimes three). "scaling constant" should be something like "fitted parameters". We learn later that in fact only one of these parameters is being investigated, however; perhaps change "scaling constant" to "scaling factor", although not all readers will grasp that a factor is a number that multiplies another number.

Agreed, we have changed 'scaling constant' to 'scaling factor' throughout the manuscript.

L14 Give the total sea-level contribution, perhaps by adding "or ...%" after "0.027 m".

Added 'or 18%'.

L16-23 The paper might be strengthened by adopting a more general classification of the sources of uncertainty: glaciological measurement uncertainty; glaciological analytical uncertainty; climate-model uncertainty, measured by the spread of outputs from different models; and scenario uncertainty, measured by the spread of outputs from different runs of the same model.

Thank you for this suggestion, this indeed gives the paper a better structure. We have altered Sect. 4 accordingly:

- 4.1: Glaciological analytical uncertainty
    - 4.1.1 Mass balance sensitivity
    - 4.1.2 Scaling factor c
    - 4.1.3 Imbalance in 1990
- 4.2: Glaciological data uncertainty
    - 4.2.1 Choice of inventory
    - 4.2.2 Data uncertainty
- 4.3: Climate model uncertainty
- 4.4: Emission scenario uncertainty

Reasonably enough, the paper does not consider scenario uncertainty, but it should at least be mentioned as a part of the larger picture.

You are right, this was missing. A section has been added, describing emission scenario uncertainty (§4.4).

Of more concern is the lack of explicit treatment of measurement uncertainty, other than through the comparison of inventories.

Good point. We have made some extra calculations and described the results in section §4.2.2.

P1659

L17 Parameters held constant for ice caps: perhaps this is reasonable,

Changes in the scaling factor of the ice caps of ±25% lead to changes of ±0.5% in the total contribution of GIC. This is very small compared to variations in the glacier scaling factor, which is 9%. We therefore chose not to take vary this factor.

but surely most of the mass will be lost from the ice caps, which hold most of the initial mass, rather than from the smaller glaciers? See comment on P1663 L20 below.

See reply to comment on P1663 L20 below.

P1660

L8-9 There are problems here. What are the standard errors of the five parameters in equations 3 and 4?

The relations were taken from Zuo and Oerlemans (1997), where no standard errors were provided. Therefore, in section 4.1 the mass balance was tested using these relations, and it turns out to be quite sensitive.

Are these uncertainties factored into the assessment of other uncertainties?

In section 4, each uncertainty in the assessment is calculated without considering the other uncertainties. All the uncertainties are combined in section 5.

Is P to be understood as Ps and Pns in 3 and 4 respectively?

No, P is annual precipitation on the glacier.

The description leading up to the equations is puzzling, because it says that the sensitivities are functions of the temperatures but the equations say that they are functions of the precipitation alone.

We have rewritten the sentence and hope that the explanation is clearer now.

L16 How many samples are there in the ensemble? 144?

No, there are 12 climate models, which together form the ensemble. The sentence has been rewritten to clarify this.

P1662

L19 Model outputs are not “data”.

Changed into ‘values’.

P1663

L20 The figure of 90% surprises me. Presumably it derives from the V-A scaling calculations, but we are not told what proportion of the total area is accounted for by ice caps or what their mass balances are. Over 100 years, I would expect the ice caps to account for much more than 10% of the contribution to sea-level rise.

The areas of glaciers and icecaps are taken from the WGI-XF inventory, and initially the area ratio is 19% icecaps vs. 81% glaciers. However, not all regions are complete and therefore need to be upscaled, which is done according to Radic & Hock (2010). Because we do not know whether the upscaled area contains glaciers or icecaps, we decided to treat all upscaled area as glacier. Of course, this leads to a ratio change in the upscaled regions, which results in a new area ratio of 11% icecaps vs. 89% glaciers, which is what was used throughout the study.

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 ± 0.022 m SLE
2. Keep WGI-XF ratio in upscaling: 19% icecap vs. 81% glacier: 0.149 ± 0.021 m SLE
3. Treat all GIC as glaciers, 100% glacier: 0.150 ± 0.022 m SLE
4. Treat all GIC as icecaps, 100% icecap: 0.144 ± 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.'

P1664

L23-24 It is technically correct that Cogley (2009b) adopted 785 000 km2 for total glacierized area, but Leclercq et al. (2011) used 704 000 km2 (see last paragraph of their section 2), the result of an update of the Cogley estimate for glacierized area in the periphery of Antarctica.

Thanks for pointing this out. The value has been changed.

P1665

L3-7 I do not understand this reasoning. It seems to be over-interpreting uncertain numbers, and adds little to the argument of the paper.

Agreed. The sentences have been removed.

L10 "in the lower range": not an accurate summary of Figure 2, in which R10 and W01 start higher than Leclercq, are lower from about 1920 to 1990, and end somewhat higher.

Sentence taken out, the figure is more accurately described in the next sentence 'R10 and W01 ..before 1865'.

P1666

L5 "to relate mass-balance sensitivity to precipitation".

Changed.

L12-13 Delete "for the future scenarios". All the scenarios in the SRES sense are the same, and the sensitivity does not vary with time within each simulation.

True, deleted.

P1667

L15-21 The time scale is usually understood as the time for 1 – 1/e of the total change to be realized; "hundreds of years" sounds like the time for a much larger fraction. And I do not understand why any size class should reach a new equilibrium before 2100, unless by complete disappearance as in Figure 4b; this point should be made more accurately.

True, sentence changed.

L24 Give the calendar date at which t = 0.

T_0=1865, added to text.

L26 Equation 5 suggests that the units of V are radians. Presumably this V is a fraction of some reference value, which should be stated. It would also be prudent to use a distinct symbol, say V*.

This is indeed confusing. We have changed it to V* and added that V* is dimensionless, being the ratio V_t / V_0

P1668

L5 The paper switches indiscriminately between V as glacier volume, as in Figure 4, and V as sea-level contribution, as in equation 6. One of the definitions should be chosen and adhered to.

In most of the paper V and dV are expressed as the sea-level contribution and given in sea-level equivalent, and otherwise it is mentioned in the text.

At what date, if any, does the δV/ δt of eq.6 pass through a maximum? This question is of some interest in the context of water resources, because the answer represents "peak meltwater".

Equation 5 is only valid for t < D, because for t=D, Vt/V_0=0. D is also the time where Eq.6 passes through a maximum, thus the melt speed increases until the glacier disappears.

P1671

L1-6 This comparison of two inventories accounts well for the differences seen in Figure 6, but I am not sure of its relevance. The W01 inventory resolves the glaciers with the greatest areas only poorly, and is plainly rather unrealistic as a basis for estimating volumes. For example, it has about three quarters of the total Central Asia area (Figure 7) in glaciers with sizes of 512-1024 km2, of which there are fewer than 10. I suspect that many of the glaciers in its bin 9 are in fact "glacier complexes" (patches of ice taken from small-scale maps?).

The relevance of this section is that we wanted to show the progression of the data in the glacier inventories. Indeed, the W01 inventory was less complete in the sense that for instance in Central Asia the glaciers could not yet be classified separately and instead one large glacier complex was used. We show that these differences are of influence on the calculation of the GIC contribution to sea-level change.

P1672

L5-10 This is not expressed accurately. Except for Patagonia, the Southern Hemisphere will see a sea-level rise greater than average and the Northern Hemisphere a rise less than average. According to Figure 10a, only the high Arctic will see an actual drop.

Sentence rephrased: '.Figure 10a shows that, except for Patagonia, the Southern Hemisphere will experience a sea-level rise greater than average, the Northern Hemisphere a rise less than average and parts of the Arctic region will even experience a sea-level drop from the contribution of GIC.' .

L8 Delete "and". This tends to contradict the claim at P1663 L20. Most of the Arctic ice, by area, is in ice caps.

Changed.

L14-16 Again, not a very accurate description of Figure 10b. I would say that the differences are "moderate" in Pakistan and Bangladesh and "substantial" in Patagonia and the high

Arctic.

Sentence has been rephrased: 'Regions with substantial differences between the two inventories are Patagonia and the high Arctic, where the largest differences in sea-level pattern can be found close to the largest melt sources.

P1673

L19 Table 4 would summarize the study better if it had a "No imbalance" row, perhaps below the Total uncertainty row.

Thanks for this suggestion, a 'no imbalance' row is added.

P1674

L7-8 It should be mentioned somewhere that there are other ways of building the imbalance into projections, for example by modelling the equilibrium-line altitude and/or accumulation-area ratio explicitly.

Added to §3.2: 'Other possible methods to account for the imbalance of with climate are by modelling the accumulation area ratio (e.g. Bahr et al., 2009) or the equilibrium-line altitude (e.g. Raper and Braithwaite, 2006).'

P1675

L8-11 The text might also add that an elevation-dependent correction of the climate-model outputs would strengthen the volume-area scaling model. Such corrections are very uncertain for precipitation, but less so for temperature, for which they could be made glacier by glacier for most of the entries in WGI-XF.

Added: 'Improving the climate models with an elevation-dependent correction will significantly reduce the uncertainty in calculating the GIC contributions and is therefore a crucial action for future work.'

L12-18 These numbers for "scenario uncertainty" are substantial with respect to those in Table 4, and reinforce my impression (P1656 L16-23) that the paper ought to say more about it. For example it could be mentioned near the last two sentences on P1676.

We now consider emission scenario uncertainty, a section has been added (§4.4).

In the same vein, I suggest also that it would be appropriate to add near this one a paragraph about "measurement uncertainty". Two inventories are better than one, but they do not amount to a systematic analysis of the shortcomings of the glacier database.

We have added a section on measurement uncertainty (§4.2.2).