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The Cryosphere Discussions

Interactive comment on "An assessment of uncertainties in using volume-area modelling for computing the twenty-first century glacier contribution to sea-level change" *by* A. B. A. Slangen and R. S. W. van de Wal

Anonymous Referee #2

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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-are scaling is applied in the regions with incomplete glacier inventories).

More details about these and some other concerns are provided blow, including a few specific comments. I recommend the paper for publication after the revisions are applied.

Specific comments:

Page 1657, Ln 16: In Bahr at 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.

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)

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

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? If so, this should be mentioned here as well as the implications of this in the results and the uncertainties. The authors mentions 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. 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.

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

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

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.

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-are 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-are 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.

Page 1664, Section 3.2. In the estimates of past sea-level contribution is the precipitation in equation (5) kept constant? If so, are the mass balance sensitivities also kept constant? 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). 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.

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).

Page 1667, Lines 14 -20. This explanation is not clear to me. First of all, the definition of equilibrium state would be necessary here. In Figure 5, 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. Secondly, wouldn't the reason for different response in Vi 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 volume-area scaling. 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. 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.

Interactive comment on The Cryosphere Discuss., 5, 1655, 2011.