**Response to Anonymous Reviewer**

Thank you for the careful review. We apologize for the construct that appears as a straw man. This was not intentional, and in the closing paragraph of the introduction we have now cited several references (among many) where a lower bound in glacier size is used in a GIC inventory. These GIC inventories are central to sea level studies. We wish to be clear that nobody summarily dismisses the small glaciers in these inventories – in most cases they have assigned a lower bound due to practical considerations such as time and money. In a revision, this practical consideration can be prominently mentioned in paragraph 2 of the introduction as well as in the conclusions.

To avoid any misunderstandings, the apparent straw man can easily be removed and the text can be rewritten accordingly. To this end, section 2.2 would be combined with section 2.3, removing the discussion of the largest size bin. References to bins would also be eliminated from the introduction (fourth and fifth paragraphs removed) and the conclusion (the second and third sentences would be replaced in the first paragraph).

The apparent straw man is not central to the paper's conclusions, and our point should still be clear – there are consequences of choosing a lower bound cutoff in glacier size.

**Response to detailed comments:**

*22: in response to melting of many smaller glaciers. While the world's...*

The "And" will be removed.

*24: Why are the smallest glaciers most likely to be overlooked? Is it because of the potential for confusion on imagery with snow patches - seasonal and perennial?*

To give this prominence, this would bementioned in the second paragraph of the paper.

"It is entirely possible that the smallest glaciers' sea-level contribution could be underestimated, in large part for practical reasons which make a catalog of the smallest glaciers expensive, time consuming, and error prone due to difficulties of separating small glaciers from snow patches (Bolch et al., 2010)."

This would also addressed in significant detail in paragraph 5 of the section on "Scaling relationships."

*27: Is there evidence that their contribution has been summarily dismissed? If there is, cite the evidence. If this cannot be done, this reads like setting up a straw man so you can knock it down.*

Sorry about that. See introductory comments above. The text would be revised, and references would be cited in the closing paragraph of the introduction.

*29: Delete "Moreover" - can't really start a paragraph with this phrase.*

This will be revised.

*42: Define "glacier" early on. Later (line 75) it becomes clear that ice sheets are excluded - but this is not obvious at this point.*

Ice sheet exclusion will be mentioned earlier in the paper, once in the abstract and again in the introductory paragraph.

*69: Are total mass and total volume equivalent in fact? I wonder whether the proportion of an ice mass that is firn, as opposed to ice might vary systematically with volume - or with the climate in a region - or with the long-term trend in mass balance in a region. If any of these were the case, the relation between volume and mass might be somewhat more complex than this statement implies.*

That is a good point, because clearly firn has a lower density. While there should be a small difference, we would argue that the difference must be small enough to be negligible. Volume-area scaling is well-established from theory and data and (to date) shows no marked variation with volume, climate, or other parameters. If there was a significant difference between mass and volume, then the volume data would be unlikely to match the theoretical physics which is derived from mass (and momentum) conservation of ice rather than firn. I do not know of any research that makes this distinction between firn and ice when estimating sea-level rise contributions from GIC.

*105: I believe Shook and Gray were referring to patchiness within a disintegrating seasonal snowcover, not to perennial snowpatches in mountainous landscapes that might be confused with small glaciers. If so, is it reasonable to assume the two types of snowpatch would have similar number-size distributions? I would also say " powerlaw number-size distribution" in line 105.*

Another good point. This is addressed adequately we hope in Bahr and Meier (2000). The basic physics remains the same (processes of ablation and accumulation) and can be described by percolation physics. From the related statistical physics, the associated scaling exponents are "universal" and will be the same for a wide variety of phenomena independent of detailed differences between the processes. Not only will the scaling exponents be the same for perennial snow patches, seasonal snow cover, and glaciers, but the same exponent will also apply to such disparate phenomena as pore sizes in sandstone and polymer gelation. Like much of statistical mechanics, the macroscopic behaviors become independent of the underlying details. Although oversimplifying somewhat, the theory shows that only a broad brush assumption of underlying randomness is necessary. A somewhat dated but excellent book on the subject is *Introduction to Percolation Theory*, 2nd Edition by Stauffer and Aharony (also referenced in Bahr and Meier, 2000).

"Power law" will be changed to "power law number-size distribution."

*109: Not obvious what this flowshed algorithm does or what it was used for in this context. Was it used to split icefields into constituent glaciers – and is this a legitimate thing to do in the present context. The Clarke reference is missing from the list of references at the end of the paper.*

The referenced ice mask (Bosch et al., 2010) indicates ice extent but not ice divides. The flowshed algorithm (referenced in Clarke et al) does split contiguous ice cover into separate glaciers as defined by flow. We will clarify this in the text by adding "This model splits contiguous ice cover into separate glaciers as defined by flow to derive the sizes and numbers of glaciers…" Separate flow is at least one of possible definition of separate glaciers, so it is probably sufficient in this context.

Apologies for the missing Clarke reference – it will be included. Clarke, G. K., Anslow, F., Jarosch, A., Radic, V., Menounos, B., Bolch, T: Subglacial topography and ice volume for western Canadian glaciers from a bed stress model and mass balance fields. J. Climate, submitted.

*111-112: Not obvious to me why this regional subdivision of British Columbia Glaciers was needed for the purposes of this paper.*

The analysis could be done either way, but the thinking in this case is that additional data is better than less. Separating gives more information and demonstrates that the relationship is valid not just regionally but also within subregions.

*113-114: What is the implied significance of this statement - is the flowshed algorithm doing this differently from other methods at the smallest glacier sizes - and if so, what? Is the implication that the apparent deviation in the size distribution at the small end of the size distribution is a methodological artefact? Please clarify what is intended here.*

We will add a clarification. "The model predicts a mean scaling exponent of beta= 2.18 +- 0.1 in substantial agreement with data and theory. This agreement seems unlikely if the theoretically derived power law is incorrect. At the very least, a power-law fit is a reasonable approximation to the data and is sufficient to estimate total volume errors in the following analysis."

*115-120: I had the sense that the direction of the underlying argument was lost in this part of the text*

In addition to adding a new sentence at the end of the preceding paragraph (see above comment), we will also add a clarifying phrase at the beginning of the next paragraph. "However, instead of assuming an appropriate distribution of mass at the smallest sizes, …" This should make it clear that while there is evidence that a power law is appropriate at all glacier sizes, we do not assume this is the case. Instead of choosing a distribution, we derive results for both possible distributions.

*124: sufficient for what purpose?*

The text will be revised as part of combining sections 2.2 and 2.3 to remove the straw man.

*167: glacier sizes (ie delete s from glaciers)*

This will be changed.

*Section 2.3: The authors really need to refer to published examples of calculations that have been made assuming that only the largest glaciers matter to demonstrate that there is a real debate here. Again I had the sense that we might just be discussing a straw man, rather than a genuine issue.*

This will be corrected as discussed above.

*201: What about regions that contain a mix of glaciers and ice caps? How should they be handled?*

An excellent question that would be best avoided altogether by choosing regions that keep glaciers and ice caps separate if at all possible. However, as shown in Figure 2, the recent Randolph Inventory selects regions of the world that have a mix of both. In that case, a revised scaling exponent $\gamma$ could be estimated from a combination of glacier and ice cap volume-area data. If the number of ice caps (or glaciers) in a region is small compared to the number of glaciers (or ice caps), then $\gamma$ is unlikely to change significantly. On the other hand, if the numbers of ice caps and glaciers in a region are similar, then because most regions have many glaciers, the preferred solution of constructing separate distributions may be reasonable. In Table 1, we choose scaling exponents based on whether a particular region is dominated by ice caps or by glaciers.

We will add text that summarizes this point: "For global analyses or regions that contain both ice caps and glaciers, separate scaling analyses can be applied to each (e.g., Bahr et al, 2009). Ideally, regions should be selected so that glaciers and ice caps are not mixed. However, when that is not possible, a revised scaling exponent $\gamma$ could be estimated from a combination of glacier and ice cap volume-area data. If the number of ice caps (or glaciers) in a region is small compared to the number of glaciers (or ice caps), then $\gamma$ is unlikely to change significantly. On the other hand, if the numbers of ice caps and glaciers in a region are similar, then because most regions have many glaciers, the preferred solution of constructing separate distributions may be reasonable."