Reply to comment on

Grinsted, A.: An estimate of global glacier volume, The Cryosphere Discuss., 6, 3647-3666, doi:10.5194/tcd-6-3647-2012, 2012.

Aslak Grinsted says:

I answer the review comments point by point in indented italics below.

M. Huss comment

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The paper by Grinsted presents an estimate of global glacier ice volume based on the Randolph Glacier Inventory (RGI) and volume-area scaling laws. There is a focus on uncertainties in volume-area scaling, and on how this approach might be improved. The final result of the study is that existing glacier ice volume estimates might be significantly too high. This finding potentially has a significant impact. I appreciate that the problems and uncertainties of volume-area scaling are addressed in this study, but I have also noted a number of – in my opinion – important issues that should be taken into account for a revised version of the paper.

Hopefully my revisions are convincing.

Methods:

The description of the methodology to estimate ice volume needs to be significantly improved. I was unable to understand which statistical approaches were used to obtain the final results. Table 2 provides several different scaling laws but the details are not described in the text. Obviously, regressions were performed with area, elevation range, length and continentality. However, results of these regressions are neither shown, nor discussed.

I now describe the misfit functions being minimized in the text in more detail, and include a new figure to illustrate the difference between my method and the traditional log-log scaling. I now clearly write that I use the min.absdev with constrained constant estimator in the final volume estimates. I have a table which lists the scaling laws in the order they are applied to RGI.

Statistical performance: I am missing a clear description of the statistical performance of the fitted regression curves. How well do the fits perform?

A test of the statistical performance of the pure area-volume scaling when applied to the Huss and Farinotti (2012) data is now included.

Which variables should be included in the multiple regression to obtain the best results?

The table with multiple regressions is ranked according to their performance in the cross validation. The first scaling relationship for each group is the best for that group. These are the actual relationships that I apply in the listed order to obtain the final estimate. The text has been revised to make that more clear.

It also might be worth thinking about showing correlations of area with thickness instead of volume (see e.g. Cogley, 2012). Looking at Figure 1 it seems as the correlation is excellent. However, this is largely explained by the fact that V already contains A, and the actual spread of thickness predicted by area is suppressed.

I agree that showing volume rather than thickness makes the correlation appear more impressive than it really is because the volume estimate is not independent of the area estimate. However, in the context of this paper it is actually the volume we want to estimate, and it is the volume misfit which is the important quantity. For this reason I believe it is better to volume in this paper.

Thickness data uncertainty: The entire study is attached to the measurements of mean thickness of roughly 200 glaciers. Therefore, the uncertainties in these input data should be critically discussed. Many of these thickness values are several decades old and volumes were partly calculated from extrapolating observed thickness of just a few profiles. Basically, no study has yet 'measured' the volume of a whole glacier... I expect that the large uncertainties in these 'ground-truth' data could be very critical to the fitted regression curves.

I now include a test of the sensitivity of the regressions to 5% standard errors in volume and 3% standard errors in area in a surrogate calibration dataset (Huss and Farinotti, 2012). It does not appear to be very sensitive to this added noise, as I have a reasonably large area-volume database to calibrate against. The procedure is also robust to higher levels of noise. The dominating uncertainty is the subdivision/glaciercomplex issue and noise in the calibration data set is comparatively much less important.

I agree that it is hard to quantify the level of uncertainty in the total volumes. Macheret and Zhuravlev (1982) compare volumes derived from flight lines with those from surface RES surveys and note: "Differences did not exceed 8%." - Another anecdotal example is from the Columbia glacier where McNabb et al. (2012) estimate 294 km^3 (ice equivalent) for the pre-retreat (1957) geometry. This can be compared to Brown et al. 1986 who published another estimate of 291 km3.

Glacier areas derived from the RGI: The total regional glacier areas given in Table 1 do not agree with the latest evaluations by Arendt et al. (2012), i.e. the producers of the RGI. This issue should definitely be corrected in a revised version of the paper.

The reason for this difference is because I did not use the pure RGIv2 database. I also included GLIMS and WGI data. I.e. For some glaciers, RGIv2 areas were discarded in favor of GLIMS data because of the richer meta data of the GLIMS database. Further, I have been in contact *with Anthony Arendt who has uncovered some issues with the areas originally reported in RGIv2. I now use corrected RGIv2 areas as provided by Arendt. The update has not changed the global estimate dramatically.*

Glacier complexes: The issue of the separation of glaciers in the RGI is already shortly discussed by the author. The problem is however not resolved. Many shapes of the RGI contain glacier complexes, i.e. many individual glaciers that are perceived as a single one. This has a huge impact on volume calculated using scaling. In order to apply scaling-laws, individual glaciers should be separated first.

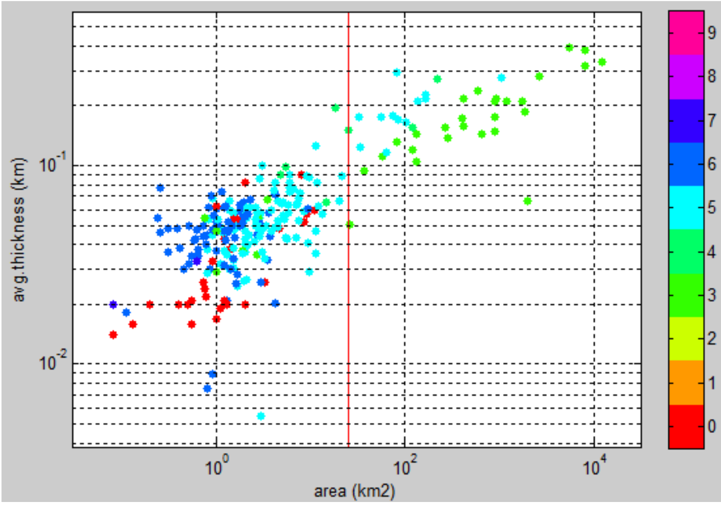
This is clearly the major uncertainty in my estimate. I follow Huss and Farinotti, and investigate the impact of the subdivision of Arctic Canada South glacier complexes between RGI v1 and RGI v2, and the impact of treating the Aletsch complex as a single complex. It can lead to a +80% bias. The question is how large an impact that has on the global estimate. I have designed a Monte Carlo test using data from a virtual world (taken from Huss and Farinotti, 2012) which allows me to estimate both the standard uncertainty of the scaling methods, but also the bias arising from applying non-complex relationships to complexes. I estimate the bias to be in the order of +5cm in the final global estimate.

There is also a potential negative bias arising from RGI having subdivided ice caps into multiple units. This is illustrated with the Devon ice cap example.

Ice caps: Where does the threshold of 25 km2 between glaciers and ice caps come from? Whereas a value like this might be appropriate for the Arctic, it is completely unrealistic in all other regions: In Alaska, High Mountain Asia, and other alpine mountain ranges there are numerous glaciers larger than 25 km2, but not a single ice cap. As it is shown by the author, the selected exponent has an important impact on the calculated volume. So the division of glaciers and ice caps should be addressed in detail in order to keep track of the uncertainties.

The 25 km2 threshold I use is not a distinction between ice caps and glaciers, but just a threshold between large and small ice bodies. I have varied it between 1km2 and 250km2 and my total volume estimate varied by less than 3 cm. So, the exact choice does not matter much for the analysis. I have added a bit more detail in the manuscript, including the motivation for choosing exactly 25km2. (The threshold cannot be chosen greater than about 250 km2 because I need enough data points for a calibration while withholding 75% of the data for cross validation.)

The 25 km2 was chosen base on plots like this showing the primary classifications of the data in the volume database:



Judging from this plot there does not appear to be very much difference between a glacier and an ice cap for small areas.

Regarding the uncertainties: In the new Monte Carlo test I can mimic the estimation procedure (including using the 25 km2 rule). This allows me to get a much better handle on how uncertainties accumulate into the final global estimate. This does not show how much error is from the threshold choice alone, but it give me an estimate of the final total error.

Figure 2: I note that the values for c in the scaling relation used by Radic and Hock (2010) stated in this figure do not agree with the Radic and Hock-study.

This is because I use different units for A and V. I use km2 and km3, whereas Radic & Hock (2010) use m2 and m3. I have also now included a whole host of other scaling relationships to compare with including your own from the 2012 paper.

Other approaches: The last sentence states that 'more sophisticated approaches' should be used to reduce the uncertainties in volume-area scaling. In that sense the author might consider discussing the recent study by Huss and Farinotti (2012) that calculates global glacier ice volume based on the RGI without relying on volume-area scaling, and also presents a comparison to previous studies

Thank you. The paper has been revised to take this excellent piece of work into account.

References

Arendt, A., et al. (2012), Randolph Glacier Inventory: A Dataset of Global Glacier Outlines, version 2.0, digital media.
Cogley, J. G. (2012), The future of the world's glaciers, in The Future of the World's Climate, 2nd ed., edited by A. Henderson-Sellers and K. McGuffie, pp. 197-222, Elsevier, Amsterdam.
Huss, M. and Farinotti, D. (2012). Distributed ice thickness and volume of all glaciers around the globe. Journal of Geophysical Research, 117, F04010, doi:10.1029/2012JF002523.
Radic, V., and R. Hock (2010), Regional and global volumes of glaciers derived from statistical upscaling of glacier inventory data, J. Geophys. Res., 115, F01010, doi:10.1029/2009JF001373.