

Reply to review of

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.

Anonymous Referee #1

This paper is timely – new estimates of total GIC volume are very important to the ongoing efforts to predict sea level rise. The paper is easy to read and the analysis is described clearly. The results are unique and significantly different from previous studies of the same topic, and for this reason, the paper deserves careful attention. In many places, additional citations would be helpful. In particular, this manuscript's scaling exponents depart significantly from previous work; therefore, a thorough list of previous volume-area regressions would be helpful in the introduction. A summary table of regression values from previous studies (exponents and constants) would also help place this paper's work in context with others. The primary justification for the new exponents is an improved regression technique.

I have added a table with published scaling laws. I have also included a new figure which hopefully helps to explain both the methods and why I get different exponents to previous studies.

I heartily endorse an improved regression of the data, but the choice and justification may need some additional thought. There is a great deal of literature describing appropriate ways to find power-law scaling exponents (e.g., Clauset et al, 2009, referenced below), and the author's selection is not discussed in that context. Again, citations would be helpful.

In order to justify the particular methods I use, I have now included a test to investigate the performance of different calibration methods when tested against data from a virtual world (I use the Huss and Farinotti 2012 data as the truth in this virtual world). This gives much stronger support for the regression technique that I finally employ. This has also allowed me to improve the regression technique further (by not having k as a free parameter). I believe this test provides much stronger support for the approach that I use.

One of the main arguments of the paper is that when you calibrate an empirical relationship, then you must consider the purpose for which you plan to apply it. You should optimize the skill with respect to performance measures that are considered most important for the task at hand. This is why I optimize it with respect to absolute volume deviation. My goal is not to

have the smallest percentage error on any particular volume estimate, but to design a relationship that results in the smallest error in the total volume.

Clauset et al. is an excellent paper, but while some of the considerations are useful, then it is only directly applicable when the power law you are fitting is describing a distribution. In my case I have volume on the y-axis rather than a frequency or a probability density. The dangers of log-log regression that Clauset et al. list in their appendix A do not apply in this case. Clauset do not describe cross validation (but endorses it). This is the approach I use in my model selection. Another important difference between Clauset and my study is that he is talking about real power laws, whereas I am talking about empirical fits. Although I use the term "scaling law", then I don't actually think of it as a law, but rather as a useful approximation which works well up to certain point. -Probably because it is pretty close to a law as derived by Bahr.

The conclusions of this paper depend on the quality of the volume and area data for the 210 glaciers and 34 ice caps shown in Figure 2. That list is short enough to warrant publication of the data as a table, and would help others to both evaluate the quality of the data and to attempt their own regressions. I would consider publication of this data essential to the publication.

Graham Cogley has collected this data and I believe that he is still adding to it. So, for that reason I would much prefer that potential new users went to Graham Cogley for the data rather than using the particular set of data available to me at this time. However, I will add the contents of this file as supplementary material

I am concerned that there is such a large discrepancy between this paper's very high estimate of the percentage total volume of glaciers greater than 100 km² when compared to Bahr and Radic (2012, The Cryosphere). Normally, I wouldn't be too concerned with such a discrepancy, except that in this case the author is already proposing significantly different scaling parameters.

While my ~85% is greater than your calculation below based on Bahr and Radic (2012), then it is very compatible with the 86% estimated by Huss and Farinotti (2012). Also if you read off from Bahr and Radic's final graph then you get 70-75% which is not that far away from the estimate we propose.

The difference this seems to simply be a feature of the area distribution in the RGI, rather than a consequence of my scaling laws. Indeed applying other scaling laws with larger exponents would place an even greater fraction of the total volume in the large ice bodies.

The Bahr and Radic analysis can be applied to the scaling parameters derived in this paper, so the discrepancy may indicate a calculation bias in this paper that is over-weighting large glaciers (relative to

small glaciers) by assigning them too much volume and/or by assigning small glaciers too little volume. This would be consistent, for example, with assigning too large of a value to the scaling constant.

I do not follow how you are able to conclude which study has a bias based on the disagreement alone. I do not dispute that my scaling fits might have a bias, but I do not think this affects the discussion I have here. My scaling laws have smaller exponents than most other published estimates. This would act to place a smaller volume fraction in the largest glaciers. So this cannot explain the difference.

However, the issue of RGI containing glacier complexes for some regions could lead to a positive bias in the largest ice volumes. I now estimate the bias in the total ice volume due to this glacier complex problem to be in the order of 15% of the total volume. This seems too small to explain the difference though. I believe that Huss & Farinotti's approach is able to give much better volume estimates for glacier complexes, than what I would get from glacier scaling, and they also get ~85%.

Regarding over-weighting large glaciers: I have now added a simple weighting to the least.abs.dev misfit function to reduce the effect of a sampling bias in the volume database. (As was suggested as a refinement in the old version of the manuscript). This has led to larger exponents, but they are still below published estimates. The global volume estimate is unchanged.

Figure 3 does seem to show a significantly higher value for this constant when compared to previous regressions in other studies. The estimate of total SLE depends on the relative volumes assigned to the few large glaciers versus the many more numerous small glaciers. I have no way of evaluating this discrepancy quantitatively in the context of this work, but it is a concern. At the very least, I'd recommend noting (and possibly discussing) the discrepancy somewhere in this manuscript.

When I use a smaller exponent then I would have to compensate with a greater constant to maintain the fit. When this is taken into account then my scaling relationships are actually below most other published studies. (see the new figure 2).

Detailed comments:

- (2) Page 3648, Line 18: This line mentions that the volume-area power law is "empirical but physically reasonable." I would instead say that the power law is "both empirical and physically derived."

Thank you. Revised to "physically justified".

(2) Page 3650, Line 26: The symbol n is often used for Glen's flow law. The Greek symbol γ is frequently used for the volume-area scaling exponent. For clarity and consistency with previous

publications, gamma would be preferable. To avoid my own confusion in this review, I'll use the traditional n for the flow law exponent and gamma for the volume-area scaling exponent.

I have changed to gamma.

(3) Page 3651, Line 3: This line mentions that the theoretical power law exponent of gamma = 1.375 is derived from perfectly plastic ice. Not so. The scaling exponent is derived for arbitrary choices of Glen's flow law exponent n (see equation 7 in Bahr et al, 1997). The specific exponents of gamma = 1.375 and gamma = 1.25 are derived using $n = 3$.

Thank you. This misrepresentation has been fixed. However I note that Bahr et al. (1997) assume a 'square-root' shaped ice cap profile when getting 1.25 which is essentially boils down to assuming perfect plasticity.

(4) Page 3651, Line 4: This line mentions that volume-area scaling has been derived in Bahr et al (1997) for linear glaciers and circular ice caps only. In fact, there is no restriction on the geometry.

Thank you. The misrepresentation has been fixed. Bahr's scaling considerations are very general. The particular exponents of 1.375 and 1.25 are consistent with particular idealized geometries and perfect plasticity, although this is not actually assumed in the closure choices.

The quantities used in Bahr et al (1997) are "characteristic values" of the length, width, etc. These characteristic values summarize in a single number the complex geometries of real-world glaciers. This is exactly the same as the characteristic values of time, length, thickness and velocity that are used to summarize the complex shapes of glaciers in Johannesson et al (1989, J. Glac.) when evaluating the response time of glaciers. This is also the same way that non-dimensionalizations of other problems in physics and engineering handle complex geometries.

(5) Page 3651, Lines 8-11: The author implies that there may be inadequacies with the theoretically-derived volume-area scaling relationship because it assumes a fixed scaling constant k . Actually, neither Bahr et al (1997, JGR) nor Bahr (1997, WRR, vol 33) make that assumption. In fact Bahr (1997, WRR) shows that k has a distribution of possible values (with a well-defined mean) – see Figure 6a. The author should cite more explicitly those publications which perhaps erroneously make the assumption that k is a constant, and then note that the derivation from the physics does not make this assumption.

I do not mean to imply that there is anything wrong with the theoretical foundations. I now clarify that Bahr 1997 considers a distribution.

I also do not think that it is wrong using a constant k in the scaling law, as long as you realize this is an approximation that will not work well for every glacier.

(6) Page 3651, lines 12-14: Yes, it is good that this is stated explicitly. Scaling laws are best applied to large sets of glaciers and not to individual glaciers. This is not widely acknowledged but should be, and I appreciate seeing that here.

(7) Page 3651, line 16: It would be best to cite specific references that use regressions to derive gamma and k. In fact, this manuscript derives radically different values for the scaling exponent, so a review of previous regressions would be most helpful. It's also worth noting that not all papers use a regression to derive a value for k. For example, consider the way Bahr (1997, WRR) treats the scaling constant. In that paper, the constant is shown (from data) to have a distribution of possible values with a well-defined mean (with what looks like a roughly normal distribution); that derivation assumes that the scaling exponent gamma is fixed. Other papers (e.g., Slangen and van de Wal, 2011; Radic et al, 2008) have also looked at the effect of variations and errors in k to assess potential problems associated with deriving its value from a regression.

I have added a new table which shows a list of earlier published relationships. Several of the papers I list in this table 1 use log-log regression to estimate the parameters (although sometimes regressing against average thickness rather than volume). Bahr 1997 is an exception.

I have updated the misfit function to put less weight on large glaciers (to partially account for sampling bias in the calibration data set). This has rendered the exponents less 'radical' (but still smaller than many published studies). The impact this has had on total volume and volume distribution is minimal however.

(8) Page 3651, lines 22-23: I agree that errors in the largest glacier volumes are more important when calculating the sea level equivalent (SLE). However, the errors in the larger glaciers are mitigated somewhat by the fact that there are very, very many small glaciers. Therefore, a scaling error at small glacier sizes is multiplied by tens of thousands when calculating the SLE. This tends to level the playing field somewhat between the large and (numerous) small glaciers. When calculating the SLE of glaciers and ice caps, it's not quite as biased toward large glaciers as the author implies. Bahr and Radic (2012) discuss the role of many small glaciers.

The small glaciers are clearly also very important, but they receive an undue share of the weight when you do log-log regression. Consider the 50% of the volume held by small area glaciers, and the 50% of the area in the large glaciers. These two groups should receive equal weight. However, there are hundreds to thousands of times as many in the bottom half compared to the top half. (The exact number depends on whether I look at the volume database, or the volumes I estimated for the RGI or WGI/GLIMS).

That said... The least abs. dev. estimator probably gave too much weight to the largest glaciers/ice caps, because there appears to be a sampling bias in the volume database. There seems to have been a preference to measuring the largest glacier units. I have now introduced a weight in the misfit function to reduce the effect of this sampling bias (as alluded to in the previous version).

(9) Page 4, lines 6-17: The author makes a very good point that least-squares regressions can be fragile. It's always good to check the physics, and this manuscript performs a more sophisticated regression where "volume misfit" is minimized. The author's goal here is a good one. The proposed regression technique is resistant to outliers in the data, and a citation of this important and valuable feature of the regression should be added. However, techniques of "least absolute deviation" have other shortcomings that can cause unstable regression slopes when there are small errors in the glacier area (the errors do not need to be large). In other words, the "least absolute deviation" technique could potentially derive non-unique and unstable solutions to the volume-area exponent. In particular, the regression slope (exponent) could change dramatically with small changes in glacier area data. Under normal circumstances I would not be too concerned with this possibility, but this manuscript does derive radically different scaling exponents when compared to most previous studies. As part of a possible explanation, the potential for this instability (and non-unique solution) needs to be examined very carefully in this manuscript. Frankly, I doubt that there is any instability, but it would be much better to explicitly rule it out. Other possible power-law regression techniques are outlined in many references. For example, see Clauset, A., Shalizi, C. R., and Newman, M. E. J.: Power-law distributions in empirical data, SIAM Review, 51, 661–703, doi:10.1137/070710111, 2009. This reference uses the similar maximum likelihood estimator, and they discuss the slope instability problem in great detail. Their approach and conclusions could be very useful in this paper.

I have added two references about least absolute estimators. I also evaluate the performance of the various regression techniques in two ways: I do cross validation, and I now also test the skill in on a surrogate dataset where I know the truth (I use the Huss and Farinotti 2012 set of areas vs volume). If there is any instability then the model will not have good cross validation statistics and will be weeded out by this procedure.

(10) Page 3652, line 17: The Akaike Information Criteria is not explained and needs a reference.

I have changed this model selection criterion to a simple mean squared prediction error of log V against withheld data. I do not need a penalty for high-order models (as in AIC) because I am doing out of sample validation. Whatever model does best in an out of sample validation should be considered the best regardless of the number of parameters. This simplification of the model selection criterion has only minimal impact on the results, and was found to be more robust in tests (less chance of over-fitting).

Note this change in model selection criteria does not influence the total volume much. The new simpler model selection criteria allows for more complex statistical models even if they only give slight improvement in the cross validations.

(11) Page 3653, line 26: The correct units are $3-2\gamma$, not $3 - 2k$.

Thank you. Typo fixed.

(12) Page 3654: line 16-17: The author says the total volume is concentrated in the largest ice masses. By his wording here (and elsewhere in the paper), he is implying that the largest of the very largest GIC contain the bulk of the mass. But Bahr and Radic (2012, The Cryosphere) show that about 50% to 60% of the total mass is in all of the remaining glaciers (depending where the cutoff is assigned). This is acknowledged later in the text. So this statement on lines 16-17 is not quite wrong, but it's also not quite precise. A little more precision is warranted in this statement.

I have revised the sentences. At this point in the paper I do not think that the specific numbers are very important. I simply want to highlight that a very large fraction of the total volume is concentrated in a few very large ice bodies. If you do log-log fitting then you will have extremely many points in the small size classes, and the fit will be constrained primarily by them.

Later in the paper (in the final figure) I quantify the volume in the largest glaciers and I get ~85% in the largest glaciers using RGIv2 (size threshold =100 km²). This is higher than Bahr and Radic 2012, but very compatible with Huss and Farinotti 2012 who gets ~86%.

(13) Page 3654, line 18: The author acknowledges that the Randolph inventory contains ice masses that combine multiple glaciers in a single outline. But then he says this will lead to a 20% error and does not actually separate the ice masses. This 20% is a guesstimate (evaluated from only one Devon Ice Cap example) and should not be treated as +/- error because it is a systematic overestimate. Not separating the contiguous ice masses into separate glaciers will always lead to an overestimate of volume. However, the amount of the overestimate can vary considerably. For example if the ice outline is separated into two glaciers that contain 10% and 90% of the original area, then the error is only 9% (when $\gamma=1.375$). If separated into two glaciers of equal size (50% and 50% of the total area), then the error is 33%. All possible errors between 0% and 100% are possible. The choice of 20% needs additional justification; or perhaps a better alternative to drop this arbitrary error value. The author can then note that that "not subdividing ice masses" will always give an upper bound on the SLE. This would be consistent with the author's argument that the total GIC SLE should be lower.

I have included a new more solid estimate of the standard error and the potential for bias. The bias arising from not sub-dividing glacier complex, can be huge (+80%). However, on the global total I estimate the RMSE to be 20% and bias to be +15%. (I suppose I was lucky with my earlier guesstimate.)

There may however also be another negative bias as illustrated by the Devon ice cap example. The ice cap volumes will be too low if they generally have been subdivided into many small units (as was the case for Devon).

(14) Page 3656, line 2: This paper finds the total volume of all the glaciers in the inventories, but does not find the total volume of GIC on the Earth. It's a small semantic difference, but an important one. Many glaciers that are smaller than 1 to 2 km² are not in the inventories (depending on the region).

This has been clarified in the conclusions.

(15) Page 3656, lines 15-17: Using Figure 3 and $\gamma = 1.13$ for glaciers and $\gamma = 1.19$ for ice caps, the author estimates that 85% of the total volume is in glaciers larger than 100km². Bahr and Radic (2012, The Cryosphere) give a technique for determining the total volume in all glaciers larger than a specified size. If we use $\gamma = 1.13$ in Bahr and Radic's 2012 analysis, then we find that only 58% of the mass should be in glaciers larger than 100 km². If we use $\gamma = 1.19$ in Bahr and Radic's 2012 analysis, then we find that only 44% of the mass should be in glaciers larger than 100 km². Reality is a mix of glaciers and ice caps, so the correct value should be between roughly 44% and 58%. That's a huge departure from this manuscript's result of 85%. The departure is difficult to explain, but the difference does not depend on the scaling constant; the constant k is not part of the derivation in Bahr and Radic (2012). Also, Bahr and Radic's 2012 analysis does not rely on the underlying physics and only takes scaling laws as "fact." In other words, the scaling laws can be either empirical or theoretical, and the volume-area scaling exponent can be assigned any value; the result is not dependent on using the theoretical value.

This disagreement (<58% versus 85%) is possibly indicative of a flaw in the manuscript's analysis. One possibility is that the scaling constant k in this paper is too large for glaciers. For a given and fixed scaling exponent (e.g., $\gamma = 1.13$) a large value of k would tend to exaggerate the volume of all the largest glaciers relative to all the smallest glaciers. Interestingly, one of the drawbacks of a "least absolute deviation" regression (as used in this manuscript) is that the solution is not guaranteed to be unique. Therefore, it is unlikely but theoretically possible that the "least absolute deviation" regression could give the incorrect value of k (even while giving the correct slope γ). Another possibility is that the contiguous ice masses which have not been subdivided are significantly biasing the total volume of large glaciers.

My estimate of 85% is supported by Huss and Farinotti, and is not sensitive to excluding regions with glacier complexes. I do not think this can be explained by the scaling relationships I apply, but the source of the disagreement must be in the area distributions in RGI compared to the power law area distributions in Bahr and Radic (2012). Further homogenization of RGI would certainly help here.

When I read off Bahr and Radic's final graph, then I get 70-75% of the volume in the glaciers larger than 100 km². I really do not think that is much of a disagreement considering the assumptions that go into Bahr and Radic's number. E.g. A single scaling exponent, a specified maximum area, and not a perfect fit to very large glaciers. Additionally my study may have a positive bias. Bahr and Radic's approach is a great way to estimate the order of magnitude

error you make from excluding very small glaciers. - But I am less sure that you can use it to get very precise estimates of volume fractions below a threshold. Can it really distinguish between 70% or 85%?

(k affects all glaciers equally. A larger value of k would have to be compensated by a smaller value of gamma (otherwise it the fit is lost). A smaller value of gamma would place a smaller fraction in the large glaciers. So k cannot explain why I get a large fraction.)

(16) Page 3661, line 4: "Ice sheets" should be changed to "ice caps."

Fixed.

(17) Page 3665, Figure 2: Please include another table that shows the specific glaciers, volume, area, etc. that were used to construct this figure and the regression. It will help readers evaluate the results.

This will be included as supplementary material.

(18) Page 3665, Figure 2, Line 6: The figure caption notes that the theoretical exponent is too high compared to nature. That may be true, but the author should instead say that "Theoretical scaling law exponents are generally higher than what is derived from the regressions in this analysis."

Sentence rephrased.

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

matthias.huss@unifr.ch, Received and published: 27 October 2012

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 km³ (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.

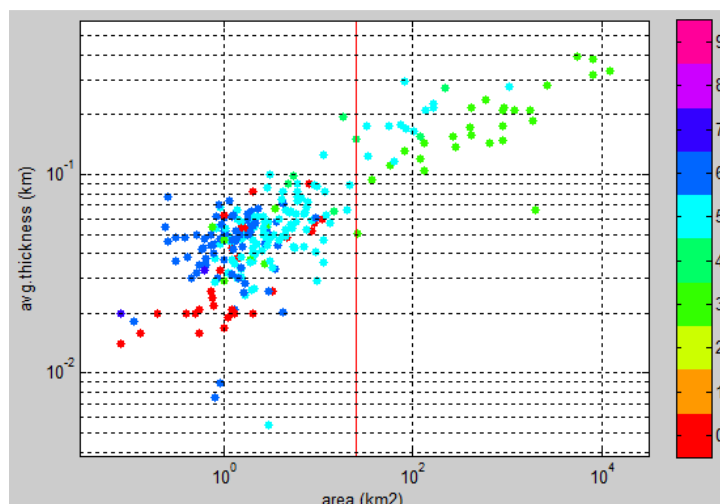
This is clearly the major uncertainty in my estimate. I follow Huss and Farinotti, and investigate the impact of the subdivision of Arctic Canada South glacier complexes between RGI v1 and RGI v2, and the impact of treating the Aletsch complex as a single complex. It can lead to a +80% bias. The question 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

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- 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.

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Aslak Grinsted says:

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

Referee #2 Roger J. Braithwaite

General Comments:

The total volume of water contained in the world's glaciers is important because it sets a limit on the potential sea level rise from glaciers under global warming. Insofar as sea level rise is regularly re-assessed by bodies like the Intergovernmental Panel on Climate Change (IPCC), research on global glacier volume is policy-relevant and, like all policy-relevant research, the emphasis has to be on the best available science at the time of the assessment rather than on the last word. Estimation of the volume of all the world's glaciers and ice caps (excluding the great ice sheets of Greenland and Antarctic) is difficult because of lack of data. In a strict sense, there is no single glacier whose volume has been measured directly, although glacier volume can be assessed with greater or lesser accuracy for about 200-300 glaciers, using field data from the glacier in question. We are referring to these (roughly) 200-300 glaciers when we talk of "observed" glacier volume, although there will still be questions of sampling of measuring sites and extrapolations to unmeasured sites.

It is hard to quantify the level of uncertainty in the total volumes. Macheret and Zhuravlev (1982) compare volumes from sparse flight lines with those from dense surface RES grids and find: "Differences did not exceed 8%." - Another anecdotal example is from the Columbia Glacier where McNabb et al. (2012) estimate 294 km³ (ice equivalent) for the pre-retreat (1957) geometry. This can be compared to Brown et al. 1986 who published another estimate of 291 km³.

I have now estimated the total error from several error sources including observational uncertainties in area and volume. The uncertainties due to errors in A and V are dwarfed by the uncertainties/biases due to differences in how areas are subdivided between inventories.

McNabb et al. 2012, doi:10.3189/2012JoG11J249

A number of recent studies use so-called volume-area scaling to estimate glacier volumes from known glacier areas, e.g. from a glacier inventory. Some people may prefer thickness-area scaling but the final global ice volume will be the same whichever way you go. To apply volume-area scaling, a.k.a. thickness-area scaling, you would need (1) the area of each and every glacier in the world, and (2) a tried and tested equation (or equations) to calculate average glacier thickness or total glacier volume for each glacier from its area. We still have some way to go to achieve both (1) and (2) but the paper by Grinsted is a very

interesting, challenging and timely contribution to the literature. With respect to (1), the paper by Grinsted uses the newly compiled Randolph Glacier Inventory v2 together with updates to the WGMS and GLIMS datasets, which must surely be the best available data on glacier areas. With respect to (2), Grinsted uses volume-area scaling carefully and critically although he should look seriously at suggestions made by Referee #1 and by M. Huss. In particular, Grinsted's comment that the scaling law "remains the only feasible way of estimating the volume of the vast majority of glaciers" may require some modification in the light of the newly published paper by Huss and Farinotti (2012).

I have tried to accommodate the comments from M.Huss and Referee 1. In particular I have revised the manuscript taking the Huss and Farinotti paper into account.

Grinsted refers the reader to Cogley (2012) for a review of earlier estimates of global ice volume. As this review constitutes a chapter in a relatively expensive book, excellent as it may be, I think Grinsted should exploit the open-access character of The Cryosphere to give a brief review of earlier estimates in this paper, e.g. in the form of a table.

I have added a non-exhaustive table of earlier estimates of the scaling parameters. I also have a new comparison plot.

"Low and "high" ice volume estimates:

An earlier estimate of global glacier volume by Raper and Braithwaite (2005a) suffered from the lack of a complete global glacier inventory at that time. These authors could only use available glacier areas, which then excluded local glaciers around the ice sheets of Greenland and Antarctica. The ice volume estimate of Raper and Braithwaite (2005a) was heavily attacked as being too low by Meier et al. (2005) but Raper and Braithwaite (2005b) defended themselves vigorously. A revised version of Grinsted's paper could usefully reference this discussion for the insights that it may still give and for the fact that the global ice volume of Raper and Braithwaite (2005a) is a relatively "low" estimate (0.241 ± 0.026 m sea level equivalent SLE) compared with Grinsted's (0.35 ± 0.07 m SLE). However, if we adjust the Raper and Braithwaite estimate for their lower glacier area total, it would agree reasonably well with Grinsted's estimate, which is itself much lower than the 0.60 ± 0.07 m SLE found by Radic and Hock (2011). An ice volume of 0.43 ± 0.06 m SLE from Huss and Farinotti (2012) is between these "low" and "high" extremes.

I now do the Raper & Braithwaite comparison excluding the peripheral glaciers, and you are right there is very good agreement.

I prefer not to step into the discussion further than pointing out that the discussion exists, and that RB05 agrees with my estimate. On a side note the RB05 assumptions and method could probably be tested against virtual world data where the true answer is known (using e.g. volume estimates from Huss and Farinotti, 2012).

Specific comments:

Abstract second sentence reads: 'I calibrate scaling laws against volume observations of optimized towards the purpose of estimating the total global ice volume'. Perhaps 'glaciers' should be inserted after 'of'.

Thanks.

Page 3650 line 12: 'GLIMS where.' Delete 'where'

Fixed.

Page 3650 line 13: replace 'does contain' should be 'do contain'.

Fixed.

Page 3650 line 18: replace 'determined ERA40' by 'determined from ERA40'.

Fixed.

Page 3650 line 19: other authors have used the annual range (from warmest to coldest month) to express "continentality"

That would yield equivalent results, but I prefer the std.dev.

Page 3650/3651: delete 'This has been...' and start the sentence with 'The only practical...'

Revised.

Page 3651 line 27: 'weighing' should be 'weighting'.

Revised.

Page 3652 line 7: '...that is...' should be '...that it is...'

Fixed

Page 3653 line 8: I am not sure I know what '...imputation models' means.

It is a model for estimating the holes in the data (i.e. volume). I have rephrased this.

Page 3653 line 19: You give the range 0.30 to 0.39 m SLE but Table 3 gives 0.27 to 0.41 m SLE.

Changed, and updated with new numbers.

Page 3653 line 26: There is something odd about the units given on this line!

Thank you.

Page 3656 line 13-14: ‘...probably hundreds of glaciers and ice caps that have been measured, but which have yet to be included...’ .

Fixed.

Page 3663 Table 4: As commented earlier in this note, Raper and Braithwaite (2005a) estimate global glacier volume to be 0.24 m SLE for glaciers and icecaps but they exclude local glaciers in Greenland and Antarctica. Adding the regional values in Table 4, but excluding Greenland and Antarctica, gives an equivalent value of 0.23 m SLE, i.e. Grinsted’s ice volume for the same areas is almost the same as the Raper and Braithwaite (2005a) value. When it comes to applying glacier thicknesses to glacier areas, the two studies are almost identical.

I now make the comparison excluding these two regions, and there is good agreement.

References:

Cogley, J.G. (2012). The future of the world’s glacier. In *The Future of the World’s Climate*, 2nd ed., edited by A. Henderson-Sellers and K. McGuffie, p 197-200, Elsevier, Amsterdam.

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