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The Cryosphere Discussions

Interactive comment on "An estimate of global glacier volume" *by* A. Grinsted

Anonymous Referee #1

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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 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.

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.

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 kmˆ2 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. 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. 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.

Detailed comments:

(1) 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."

(2) Page 3650, Line 26: The symbol n is often used for Glen's flow law. The Greek symbol gamma 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.

(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.

(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. 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.

(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.

(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.

(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.

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

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

(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.

(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 guestimate (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.

(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 kmˆ2 are not in the inventories (depending on the region).

(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 100kmˆ2. 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 kmˆ2. 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 kmˆ2. 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 gamma).

Another possibility is that the contiguous ice masses which have not been subdivided are significantly biasing the total volume of large glaciers.

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

(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.

(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."

Interactive comment on The Cryosphere Discuss., 6, 3647, 2012.

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