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Interactive comment on "An upper-bound estimate for the accuracy of volume-area scaling" *by* D. Farinotti and M. Huss

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We are pleased by the positive and constructive feedbacks, and thank both the anonymous referee and Surendra Adhikari for their reviews.
In the following, referee comments (RC) are listed, and the according author response (AR) is stated directly afterwards. Answers are given in the same order as the comments recieved from the referees.

Comments by Reviewer 1 (anonymous)

RC: *Title: I am a bit confused by the use of the term "upper-bound". I associate this term by something like "the maximum value". The paper however presents re-*

sults from idealised experiments, stating that the uncertainty for real applications will be larger. The values presented are therefore minimum values and the term "upper-bound" seems misplaced. But perhaps there is a strange twist in my mind? At least the authors could consider whether rewording might avoid confusion.

AR: We understand the possible confusion related to the use of the wording "upper-bound". In fact "upper-bound accuracy" and "lower-bound uncertainty" can be used synonymously! Presupposing that a high accuracy value indicates a "better" accuracy, the accuracy yielded by best-case (i.e. idealized) experiments can be interpreted as an "upper-bound", in the sense that every real-world application will lead to a lower accuracy – so our reasoning. Conversely, presupposing that a low value of "uncertainty" is equivalent to a high value of "accuracy", the same idealized experiment will yield the lowest possible values of uncertainty, i.e. a "lower-bound uncertainty". In the revised manuscript, we will try to make this clear with the following sentences, hopefully avoiding confusion:
Abstract: *"Here, a series of resampling experiments based on different sets of synthetic data are presented in order to derive an upper-bound estimate (i.e. a level achieved only within ideal conditions) for the accuracy of its application. For real-world applications, a lower accuracy has to be expected."*
Introduction: *"In this contribution we [...] perform a series of synthetic experiments providing an upper-bound estimate (i.e. an estimate that is only reached in an ideal case, in which all assumptions are fulfilled) for the accuracy which can be expected when volume-area scaling is used for estimating (1) the total volume, (2) the total volume change, or (3) the total area change of a given glacier population."*
Section 2: *"The upper-bound estimate for the accuracy is derived by considering a synthetic set of data for which the assumptions necessary for volume-area scaling are imposed a priori. In an application with real-world, non-synthetic data, these assumptions will not be fulfilled to the same ideal degree, and a lower accuracy has, thus, to be expected."*
Conclusions: *"By considering different sets of synthetic data explicitly constructed in or-*

der to fulfill the assumptions that underlie volume-area scaling, the derived confidence intervals for the stated accuracies represent an upper-bound, i.e. a level of accuracy that will not be reached in applications with real data."

RC: *2305, 7-8: The two randomly selected glaciers both show a considerable volume reduction from their initial state to the equilibrium state. Do all glaciers loose this much volume or is this a coincidence? If this is a general feature, the mass balance gradient and ELA/AAR are ill-defined.*

AR: The relatively large volume loss occurring before reaching the first equilibrium state is not a coincidence but indeed a consistent feature. The reason for that, however, is not an ill-defined ELA/AAR parameter set (the influence of the mass balance gradient on the volume loss is minor), but the fact that current glacier geometries are out of balance with current climate. In fact, we imposed present-day AARs (taken from the compilation by Dyurgerov et al., 2009) to present-day geometries. The observed volume loss can therefore by interpreted as a "volume loss present-day glaciers are committed to" because of current climate. The reviewer comment let us, however, note that the stated AAR value was incorrect (should read AAR=0.44). We will correct the stated AAR value and include a note on the volume loss in the figure caption:
Section 3.2: *"The ELA is chosen such that the given surface geometry yields an Accumulation Area Ratio (AAR) of 0.44, as observed on the worldwide average (Dyurgerov et al., 2009)."* Figure 2, caption: *"The pronounced volume loss occurring between states "1" and "2" is the consequence of forcing present-day glacier geometries with present-day AARs, and reflects the imbalance between current glacier geometries and climate."*

RC: *2306, 7-9: Perhaps the authors can state explicitly that $n_{M',t1}$ and $n_{M',t2}$ are not equal.*

AR: This will be stated explicitly in the revised manuscript (note that in the revised manuscript, the notation M' will be replaced by Q in response to Reviewer 2):
Section 3.2: *"Subsample M will be composed of $n_{M',t1}$ (V, A)-pairs referring to time t_1 (subsample M'_{t1}), and $n_{M',t2}$ (V, A)-pairs referring to time t_2 (subsample M'_{t2}), with the condition $n_{M'} = n_{M',t_1} + n_{M',t_2}$, and, in general, $n_{M',t_1} \neq n_{M',t_2}$."*

RC: *2306, 18: The reference to (2.3) slightly confused me, I first thought that it referred to Section 2.3. To avoid confusion, the experiments could be indicated with letters instead of numbers, e.g. A.1, etc.*

AR: We apologize for the confusion. Experiments 1, 2, and 3 will be renamed to Experiments A, B, and C, respectively.

RC: *2315,10-13: But should not the values obtained from transient geometries be used for transient applications, the other values may not be representative...*

AR: We believe this comment being due to a misunderstanding: The parameter values stated at the mentioned lines are derived from the set of "real-world" (V, A)-pairs. In that sense, they actually refer to a transient state. That said, the parameters are only required in Experiment 1 (now called Experiment A) in which no distinction between steady-state and transient-state is made anyway.

All further comments given by Reviewer 1 concerned typographic corrections and will be adopted as suggested.

Comments by Reviewer 2 (Surendra Adhikari)

RC: *I point out a simple error made throughout the manuscript: authors constantly use the phrase "coefficients of the scaling relation", which in my opinion should be "parameters of the scaling relation", because c is the coefficient and γ is the exponent (or power; not the coefficient) of the VA relation depicted in Eq. (2).*

AR: We will change the wording from "coefficient" to "parameter" in the revised manuscript.
We wouldn't, however, term the choice of the wording "coefficients" an "error". In fact, we like to think about volume-area scaling as a linear regression model between the logarithmic transformed glacier volume V and glacier area A, i.e. $log(V) = a_0 + a_1 \cdot log(A)$. This formulation is equivalent to $V = c \cdot A^{\gamma}$, with $c = exp(a_0)$ and $\gamma = a_1$. In this case, a_1 would clearly not be called an "exponent". It may be worth noting that in statistics, the wording "coefficients", rather than "parameters", is commonly used for a_0 and a_1 (e.g. Cox and Snell, 1981; Draper and Smith, 1998), although equivalent in this case.

RC: *Section 3.2: I don't see the logic of having experiments on "transition between steady states". Theoretically, the subset of glaciers in the initial steady states (time t_1) and that in the final steady states (time t_2) should behave in an exactly similar manner. In other words, for sufficiently large number of VA pairs, the scaling parameters associated with initial or final steady states should converge.*

AR: We are not sure to correctly understand the comment here: We fully agree with the reviewer that in theory, given a sufficiently large glacier sample, the parameters estimated for the scaling relation should be the same for both the states t_1 and t_2. This, however, is not the point of Section 3.2. In fact, we never questioned this statement! This section attempts to quantify the accuracy of volume-area scaling when used for estimating volume CHANGES, given that changes in area are known. This is a differ-

ent application than estimating the accuracy with which the total volume of a glacier population can be determined (discussed in detail in Section 2), and does not explicitly address the numerical values of the scaling parameters. In our opinion, separately investigating the steady-state and transient case is necessary since there is no obvious reason for why the transient and the steady-state case should yield the same results. This point is acknowledged by the reviewer in a separate comment as well, and in fact, the results are not the same (see Fig. 3)!

RC: *The differences that you are depicting between "single" and "multiple" scaling experiments are mainly due to the fact that in former one you have twice as many VA pairs to constrain the scaling parameters.*

AR: Also in this case, we have some difficulty in correctly interpret the reviewers' comment, as the wording *"single"* and *"multiple" scaling experiment* was not used in the manuscript. If we understand correctly, the reviewer addresses the difference between the experiment in which a constant set of parameters is used, and the experiment in which parameters are assumed to vary in time. The reviewer is completely right in pointing out that some of the differences depicted are associated with the different sample size from which the parameters are estimated. This, however, is clearly acknowledged in the manuscript (note that, compared to the original manuscript, the sentence has slightly been reworded in response to a later comment of the reviewer):
Section 3.2: *"This observation seems to contradict earlier findings that indicate time varying coefficients (e.g. Adhikari and Marshall, 2012), but can be explained by (a) the standard errors associated with the estimated coefficients, which are mainly a function of the absolute number of (V, A)-pairs available for the estimate itself, and (b) the consistency of the estimated coefficients for the two points in time, which is given when assuming constant coefficients, but not when these are time-varying."*

In addition, it must also be noted that because the experiment is performed 1000 times, we can virtually exclude that the values of one of the parameter sets for time t_1 or t_2 shows a systematic deviation from what would be estimated from a larger sample: A bias is certainly present if one particular realization is considered on its own, but this is only reflected in an increased confidence interval for the final result – not in a systematic deviation.

RC: *More interesting experiment would be a transition between initial steady states and final transient states, as considered by Adhikari and Marshall (2012).*

AR: As stated by the reviewer, this very experiment has been performed before by Adhikari and Marshall (2012). In the present paper we did not want to replicate this study but go a step further.
When designing the experiments, our reasoning was that two cases should be considered: (1) The "ideal" case, i.e. the case in which the glaciers are in steady state at both points in time, and (2) the "close to reality"-case, i.e. the case in which the glaciers are out of balance at both points in time when the measured area is known. To us, the "mixed case" (i.e. glaciers in the steady state at the beginning but out of balance at the end) seems to be a combination that is neither likely to be particularly relevant for practical applications (considering the wide spread of response times of "real" glaciers, there has hardly been a timing in history in which all glaciers have been in balance at the same time), nor to be easily interpretable from the theoretical point of view.

RC: *Even for "transition between transient states" experiments, more systematic way of comparing performances of "single" and "multiple" scaling parameters is possible. In the present analysis, you have constrained "single" set of scaling parameters by lumping VA pairs at different transient states altogether. I think "single" scaling parameters should represent for initial (t_1) or final (t_2) transient states one at a time. This way, you would have equal number of VA pairs for constraining both "single" (you will get two*

solutions here, associated with each transient state) and "multiple" scaling parameters. This might lead to the different conclusions about the importance of "multiple" scaling parameters. Conclusions could be a function of "degree" of transition (i.e., how far the glaciers are off the steady states).

AR: Also for responding to this comment we hope to correctly understand the reviewers' use of the wording *"single"* and *"multiple" scaling parameters*. The approach in which the data from the two points in time t_1 and t_2 are merged for estimating the scaling parameters (i.e. the approach that assumes constant parameters) represents the most likely scenario in real-world applications. In fact, currently available measurements of glacier area and glacier volume stem from a wide range of different years, and all of them are referring to glaciers out of balance by a certain (not uniform) degree. What users are then forced to do when estimating scaling parameters, is exactly to "lump" these data together – as done in our "constant parameters" scenario.
On the other hand, if sufficient data were available, there might be a theoretical possibility of attributing the available data to two different points in time, and to estimate separate parameter sets (our "time-varying parameters" case). In this case, however, the number of (V,A) data pairs from which the parameters can be estimated is very unlikely to be exactly the same. To speak in the terminology defined for Experiment 2 (will be called "B" in the revised manuscript) this is the reason for which, in general, we decided to allow $n_{M',t_1} \neq n_{M',t_2}$, and not to force $n_{M',t_1} = n_{M',t_2}$, as the reviewer is suggesting. Note, however, that whilst performing the experiment 1000 times, the option $n_{M',t_1} = n_{M',t_2}$ is not discarded a priori, and some realizations may satisfy this condition.
Concerning the overall number of (V,A)-pairs that are used for estimating the parameters, it should be noted that the sum $n_{M',t_1} + n_{M',t_2}$ is the same for both the "constant parameters" and the "time-varying parameters" case. This was imposed exactly to avoid the possible comparability problems the reviewer is mentioning and that would be an issue if a different number of (V,A)-pairs would have been used for estimating the parameters in the two different cases. In this sense, we are confident that our results

are robust.

RC: *Section 3.4: Experiments presented in this section are interesting, but I don't think these make useful statements and deserve publication. Main reasons include: (i) the assumption that the scaling law parameters must be constant in time may not be entirely true (at least until there is sound mathematical justification available), (ii) this method demands unnecessarily large number of dataset (e.g., glacier area in two time stamps, corresponding change in volume) than the VA method itself and hence prone to poor performance, (iii) reasonable choice of c and gamma themselves is needed (but I think you can initialize these with positive c and gamma; after all, you are optimizing these), and (iv) this method yields no better results, despite the associated complication described above. I advise summarizing the whole section in a few sentences without actually describing the equation and experiments in details. Last paragraph of the section (p. 2341; l.1-10), after a bit of rewordings, should be just enough.*

AR: We will remove section 3.4. in the revised manuscript. The reviewer is right in saying that this additional experiment is not strictly necessary for understanding the main content, and will contribute to shortening the manuscript as requested.
However, as we partially disagree with the reviewers' statements, we would like to give a short reply here: (i) As discussed in the manuscript, the "constant parameters" assumption is almost unavoidable in practical applications, and thus widespread in the literature. Addressing this case would, thus, be definitively of relevance. (ii) This method may indeed require a larger data base, but has the advantage of being a data base much more readily available! In fact, the reason for why we were intrigued about the idea, was that the method doesn't require data on total glacier volume which are very difficult to obtain and uncertain. This would arguably be of great advantage compared to the current way of estimating the parameters. (iii) It is not correct that a "choice" of c and γ would be required. The method allows estimating these parameters directly. However, as stated by the reviewer, the method turns out to be inefficient (in

the statistical sense), and can, thus, be considered of minor interest.

RC: *Title: The title of the manuscript should be revised: (i) include the phrase "maximum accuracy" instead of "upper-bound of accuracy", and (ii) be explicit to write "glacier volume-area scaling".*

AR: See also the reply given to Reviewer 1: We believe that the wording "upper-bound accuracy" is correct and will keep it unchanged. However, we will add the specification "glacier" in the title.

RC: *p.2297, l.8: What value of c and γ did you use to generate v_{true} of n_{true}=171000 synthetic glaciers? I don't think you have specified these.*

AR: We used $c = 0.033$ and $\gamma = 1.36$, as suggested by Bahr (2011). This will be stated explicitly in the revised version:
Section 2.1: *"For the experiment, we chose $c = 0.033$ and $\gamma = 1.36$ (Bahr, 2011), and $\sigma_{\mathrm{V,true}} = 0.3$, based on the analysis of the results by Huss and Farinotti (2012)."*

RC: *p.2297, l.18: Is M a subset of T ? I think so; specify clearly.*

AR: No, M is not a subset of T. Loosely speaking, T is the set of "true (A, V)-pairs", whilst M is the same as set T, but with some noise added in order to mimic actual measurements. In the manuscript this will be formulated more clearly as follows:
Section 2.1: *"A corresponding set M of synthetic values, pretended to represent measured values, is then generated by adding white Gaussian noise to the "true" values of set T according to [...]"*

RC: *p.2298, l.15: Use Q or other symbol, instead of M' so that you will have $p \in T$ and $Q \in M$? Also define whether M' is subset of P? I think yes (refer l.23).*

AR: (1) We will change the notation as proposed, i.e. Q instead of M'. (2) No, M' (Q from here on) is not a subset of P. P is a subset of T, as the reviewer has correctly noted, but Q is a subset of M (and not of P). We believe that the confusion arose because it was not clear that M is not a subset of T (see the answer to the reviewer comment for p.2297, l.18). In order to prevent similar confusion in the revised manuscript, we will additionally state that $n_\mathsf{Q} \leq n_\mathsf{P}$.

RC: *p.2299, l.6: What is M'^C? I don't think intersection symbol is appropriate here.*

AR: (1) For any subset Y ($Y \in X$) of a population X, Y^C is commonly used for indicating the complement of Y within X, i.e. "the remaining part of X". We will introduce a note in the manuscript for clarification:
Section 2.2: *"Estimate the volume $\hat{V}_\mathsf{R}$ of the "remaining" subsample $R = Q^C \cap P$ of $n_\mathsf{R} = n_\mathsf{P} - n_\mathsf{Q}$ glaciers (i.e. that fraction of the glacier population for which no measured volumes are available; Q^C indicating the complement of Q with respect to the whole set M) [...]"*

(2) Once clarified the meaning of Q^C, the intersection symbol should look appropriate.

RC: *p.2299, l.19: May be show a 30% accuracy line in the figure, to facilitate reading.*

AR: The sentence which induced the question reads
"According to this result, the total volume can be recovered within $approx$30% at the 95%-level of confidence if a subsample of at least 200 glaciers is available for estimating the coefficients of the scaling relation".
Correctly translating this observation graphically in Fgiure 1a would mean highlighting the area for "200 glaciers and less" at a height of "about 30%". The reasoning may be

somewhat subtle but we think that inserting the line as suggested would not be correct.

RC: *p.2299, l.23 onwards: It is perhaps the uncertainty noise has a normal distribution with zero mean? Too long a sentence to understand for me. Lines 1-2 on the next page is too puzzling to grasp. Please rephrase.*

AR: Not exactly, to be precise. The crucial point is neither the normality of the "noise", nor its zero mean. Loosely speaking, the point is about the "spread" introduced by the noise, compared to the "natural spread" the "true values" (i.e. the values without noise) have on their own. We agree, however, that the concept is somewhat involved, and will simplify the sentence as suggested by the reviewer:
Section 2.2: *"Note that, in first approximation, this statement holds true independently from the uncertainty of the measured values (different lines in Fig. 1a), as long as the scatter introduced by the uncertainty in the measurements remains below the variability imposed by $\varepsilon_{\mathsf{V,true}}$ (see Eq. 2). This is the case because the measured values are assumed to deviate from the true values by following a normal distribution with zero mean (Eq. 3), but would not apply in case of systematic deviations of the measurements."*

RC: *p.2302, l.5 onwards: "For example..." I don't think this statement can be made based on Fig. 1b. If I am correct, be explicit to include "(results not shown)" at the end of the statement.*

AR: We believe that the figure was misinterpreted: In Fig.1b, any point on a same line (i.e. the black, red, or green line) yields the same accuracy (i.e. 40%) in terms of total recovered volume. In that sense, any statement linking two points lying on the same line can be formulated in a similar way as the sentence the reviewer is addressing. In the example, it is easy to note that the point (x=0,y=50) (where x and y denote the abscissa and ordinate of Fig. 1b, respectively) lies on the same line (namely the red line) as the point (x=75,y=200). Including "(results not shown)" would, thus, be incorrect.

RC: *p.2304, l.1: How do you control ELA in ice flow model, such that you get AAR=0.57?*

AR: Note that, as stated in response to Reviewer 1, the correct number should read AAR=0.44 (and not 0.57).
Given the mass balance function (Eq. 4) and knowing the glacier hypsometry, determining the ELA is straight-forward. We define the ELA as the altitude that yields 44% of the glacier area with positive mass balance, and 100%-44%=56% of the glacier area with negative mass balance.

RC: *p.2304, l.4-14: Bring this discussion up on the page 2303 (l.17) where you first talk about the ice-flow model.*

AR: Here, simply the values chosen for various parameters in the ice-flow model (there is no discussion at this stage) are given. We decided to first describe the function and the parameter values of the model forcing, and then to address additional model parameters that were left unaltered. The latter are basically of no further interest for the simulations.

RC: *p.2306, l.14: For sufficiently large number of steady-state VA pairs, I don't think you would obtain different set of scaling parameters no matter at what time (i.e., t_1 or t_2) glaciers attain steady states.*

AR: The reviewers' statement may be true for the steady-state case, but has been shown to be inappropriate in the transient case (Adhikari and Marshal, 2012). In our

study, we didn't want to impose "constant parameters" a priori, since our experiments addresses both cases.

RC: *p.2306, l.16: I don't think intersection symbol is appropriate here. Again, what is superscript C for?*

AR: See the answer in response to the comment for p.2299, l.6.

RC: *p.2307, l.14-15: This sentence is incomplete. You have used all data set to determine "time independent" parameter. Adhikari and Marshall (2012) compare "time varying" parameters vs. those based on steady-state VA pairs.*

AR: Thanks. We will reformulate the sentence into:
Section 3.2: *"This observation seems to contradict earlier findings that indicate time varying coefficients (e.g. Adhikari and Marshall, 2012), but can be explained by [...]"*

RC: *p.2308, l.10: The discussion 2 should be mostly based on Fig. 3b instead of Fig. 3a.*

AR: The discussion listed as point "(2)" is referring to the steady-state case (this is clearly stated), which is shown in Fig. 3a, and not Fig. 3b (Fig. 3b is showing the transient case).

RC: *p.2309, l.8: But, the point is that the so-called constant parameters you have used are based on the VA pairs of glaciers at both time stamps. If you use VA based on one time-stamp and assume it constant, results and conclusions may be other way around.*

AR: It is true that we calibrated the parameters by merging the data of the two points in time. This is, however, the most likely (if not even the only viable) scenario in a real-world application: Data regarding volume and area will be available for different points in time, and until no easy method for directly measuring glacier volume is available, these data will need to be used all together for estimating scaling parameters. This is the reason why our sentence starts with the specification "for practical applications [...]". The sentence, however, was slightly modified in response to the next comment.

RC: *p.2309, l.8: "a positive effect on the accuracy"? You mean increase in accuracy? Rephrase it.*

AR: The sentence will be reworded:
Section 3.2: *"for practical applications, assuming constant scaling coefficients increases the accuracy with which the true volume change can be recovered."*

RC: *p.2311, l.14: "between steady-state geometries than between transient states". Plot Fig. 4a and 4b in a single plot. This makes comparison easier.*

AR: The figure will be re-drawn as suggested.

RC: *p.2311, l.20: To depict this more clearly, use same x- and y-axis range and draw a diagonal in Fig. 4.*

AR: Fig. 4 will be re-drawn in response to the comment given for p.2311, l.14. The ranges of the axis are the same already.

RC: *p.2311, l.22: "within a factor of two"? 100% uncertainly may correspond to a factor of half as well, right? To avoid potential confusion, just write "uncertainty %" instead of*

a "factor" here and elsewhere.

AR: We agree that the wording "factor" is prone to create confusion. We believe however, that stating "100% uncertainty" alone is not sufficient either (to us, "100% uncertainty" sounds like nothing would be known at all). We therefore propose the following formulation:
Section 3.3: *"If, for example, the individual volume changes are known within the magnitude of the signal itself (100 % uncertainty), the total area change can be recovered within [...]"*

RC: *p.2311, l.26: Write "(or with an uncertainty...)".*

AR: See answer given for comment to p.2311, l.16.

RC: *p.2314, l.15-16: "the previously estimated confidence... ...confidence intervals need widening" is too difficult to understand. I would suggest rephrasing it.*

AR: We will reword the sentences into:
Section 4: *"For applications with real-world data, the confidence intervals estimated so far are thus expected to be systematically too narrow. For assessing by how much these confidence intervals need widening, the three experiments are repeated using measurements taken from two different data sets."*

RC: *p.2315, l.23: Worth citing Adhikari and Marshall (2012) here, who demonstrate that the stable values of scaling parameters are obtained only for large sample size (> 100).*

AR: The reference to Adhikari and Marshall (2012) will be introduced one sentence earlier than suggested and with the following text:

Section 4: *"These numbers are to first order consistent with the findings by Adhikari and Marshal (2012), who, analyzing a synthetic set of glaciers, found that "ca. 200 glaciers are required to produce stable solution[s] of scaling parameters"."*

All further comments given by Reviewer 2 concerned typographic corrections and will be adopted where considered appropriate.

References not included in the original manuscript

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Draper, N., and H. Smith (1998). *Applied Regression Analysis, 3rd edition*. Wiley, N.Y.
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