"Ice volume estimates for the Himalaya–Karakoram region: evaluating different methods" – Final response

MS No.: tc-2013-139

We thank David Bahr and the two anonymous reviewers as well as Wilfried Haeberli for their detailed and valuable feedbacks and comments; they are all very helpful to improve this article.

We modify the manuscript in order to address the suggestions and criticism brought up during the interactive discussion. First, we here list the general changes, specific replies to the detailed comments of each reviewer are attached as a PDF.

General points

The general aim of the study will be redirected from a comparison of the three different volume estimation approaches towards the main goal of obtaining the best possible volume estimation of the glaciers in the Himalayan–Karakoram (HK) region. We agree with the reviewers, that a more thorough and quantitative comparison of the approaches would require more detailed field measurements – which are not available in this region. Therefore, we suggest changing the title of the article to "*Estimating the volume of glaciers in the Himalayan–Karakoram region using different methods*".

Our idea of applying different existing approaches instead of developing a new methodology persists. For peculiarities of the individual aspects, such as theoretical backgrounds and discussions of sensitivities and uncertainties, strengths and weaknesses, we however refer to the related publications. In this study, we focus on results as derived from three different volume estimate methods, which in our view indeed is an asset and provides new insights. This concept obviously was not declared clearly enough and is accentuated in the revised manuscript.

Regarding the scaling parameters applied for Volume-Area (V-A) scaling, the parameter set from Arendt et al. (2006) will be replaced by scaling parameters that are more appropriate for the region. Arendt's parameters were used because they had been applied by Cogley (2011) to the same region and therefore allowed a direct comparison to these results. We agree with the reviewers that scaling parameters obtained from Alaskan glaciers might not be suitable for the Himalayan-Karakoram region, and thus they will be replaced by the area-thickness relation from the Lanzhou Institute of Glaciology and Geocryology (LIGG, LIGG/WECS/NEA, 1988) that is often used for volume assessments for glaciers in the HK region, e.g. by the International Centre for Integrated Mountain Development (ICIMOD, e.g. Bajracharya and Shresta, 2011).

As mentioned above and in the reviews, field measurements that would be required for a comprehensive analysis of the uncertainties, which would then allow for a direct comparison of the different approaches, are not available for the HK region. Furthermore, due to the different nature of the approaches, it is hardly possible to perform a sensitivity assessment that allows for a quantitative comparison between the different approaches. Nevertheless, the comparison of results obtained with different methods will help to reach a general assessment of the uncertainties of the resulting ice volumes.

Furthermore, it will be explained in more detail how the model parameters for GlabTop2 have been defined. To avoid an excessively long methods section that would be unbalanced between the different approaches, these explanations will be provided as supplementary material.

In the reviews of David Bahr and Reviewer #3, our evaluation of V-A scaling was criticized. Vigorous scientific discussions concerning V-A scaling have a longer history and all pro's and con's have been discussed in the scientific literature and other scientific fora. In this publication, however, it is not our intention to enter this debate on principles, but to apply this approach and compare its results to those obtained from other methods. In this comparison of ice volume estimates derived from different approaches, we see the main merit of the paper. We therefore will not go into more details of the individual approaches that have been discussed before already.

Finally, we regret if the impression has been given that our intention was to prejudicially criticize the V-A scaling approach; which certainly is not the case. In the revised manuscript, the wording of the respective sections (mainly subsection 5.2) will be revised in order to avoid such misunderstandings.

Detailed replies to the comments of each review are given in the supplementary PDF.

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REVIEW BY DAVID BAHR

SHORT COMMENT BY WILFRIED HAEBERLI AND REPLY BY DAVID BAHR

Italics: copied from the review
Normal font: Author reply

First of all, we thank David Bahr for the constructive review and explanations. We appreciate the positive judgment of the importance and the novelty of the study and that the reviewer agrees with our conclusions.

In the following we first comment on the main point of criticism, which was also taken up by Wilfried Haeberli in his short comment. Replies to the details that are not related to this main topic, follow below.

General

The review treats to a large extent (5 pages out of 8) subsection 5.2 on conceptual aspects of volume-area scaling. First of all we would like to stress that it was not our intention to prejudicially criticize this approach and hope that this misunderstanding is removed in the revised version of the manuscript.

We fully agree that the volume of an object can be measured in different ways, and most of them do not require measuring the area of the object (which for some objects would even be difficult). However, most ways of volume determination (by weighting, measuring water displacement etc.) are not applicable to glaciers. Applications of ground based gravimeters are possible as well to measure glacier volume independent of glacier area (e.g. Klingele and Kahle, 1977), but are only applied very rarely. By measuring the gravitational field, as done for instance by the GRACE satellites (e.g. Jacob et al. 2012), "only" mass *changes* of glaciers can be derived; however, such measurements are far too coarse for determining volumes on the scale of individual glaciers, and also related to large uncertainties because other factors influence the gravitational field as well (see Gardner et al., 2013).

For most other techniques that are applied for determining the volume of a glacier, knowledge of the glacier area or the glacier perimeter is inevitable. By looking at the references given in the publications from, for instance, Chen and Ohmura (1990) and Grinsted (2013), it seems that the volume data used for the determination of the scaling parameters are mostly based on ice-thickness measurements (mainly by geophysical methods such as GPR, or drillings; see also last paragraph of comment by W. Haeberli). Thus the raw measurements (neglecting that GPR thicknesses actually are an interpretation of electromagnetic runtimes) of these scaling parameters indeed include the large scatter shown in Fig. 8.

Or course it is also possible to interpolate the ice thicknesses between the locations of the measurements without using glacier area as a variable, but the two-dimensional glacier outlines are required to define the area of the interpolation. This argumentation is supported for instance by Cogley (2012) *("V-A scaling is glaciological jargon for the observed tendency of glacier volume to be proportional to glacier area. It is a misnomer, because the independent quantity that is found to be proportional to area is mean thickness (Figure 8.5). Objections to the statistical propriety of correlating area with volume, because volume is the product of mean thickness and area, are therefore without merit.")*. His Figure 8.5 also shows the large scatter when plotting mean glacier thickness against glacier area, very similar to our Fig. 8. Similarly the comment Huss (2012) to the Discussion version of the Grinsted (2013) paper: *"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…"*. Furthermore, in the IPCC AR5 it says, *"From the glacier areas in the new inventory, total glacier volumes and masses have been* *determined by applying both simple scaling relations and ice-dynamical considerations (Table 4.2, and references therein), however, both methods are calibrated with only a few hundred glacier thickness measurements"* (Vaughan et al., 2013).

We basically agree with the reviewer's arguments; the only disagreement is that we believe that the measurements, at least the majority of them, actually are glacier thickness measurements instead of volume measurements. We are aware that this cannot be tracked back to each individual data point used for calibrating V-A parameters, but the citations above show that other studies and assessments support our argumentation as well.

We recognize that discussions on this issue have been held for quite some time. It is neither the main focus of this study nor our intention to enter this debate, rather our idea is to use the different approaches as they are, in order to achieve the best possible estimate of the amount of ice stored in HK glaciers. As a consequence of this, and to avoid the impression of having a "hidden agenda" to blast the V-A scaling approach, section 5.2 will be removed. Therefore we abandon to reply to detailed comments related to peculiarities of V-A scaling that go beyond its application. All other comments are treated below.

Finally, we decided to consistently use Thickness-Area (T-A) relations in the revised manuscript. This actually influences neither the results nor the sensitivity analysis (as pointed out by Reviewer#2 as well). However, we think it is more consistent with the argumentation of the entire paper, and follows the suggestion made by W. Haeberli in his comment.

Details

Pg. 4818, line 4: The historical popularity of volume-area scaling is not just due to the simplicity of the application. It's also because area data has been historically easy to measure and to compile.

Added to the text

Pg. 4820: Some disadvantages of modeling are mentioned, but the inherent dangers of a numerical inversion are not discussed. Huss and Farinotti (2012), for example, average over long wavelengths to avoid large calculation errors at short spatial wavelengths. This should probably be discussed or briefly mentioned (more than in the cursory nod given to slope averaging on pg. 4822, lines 14-15). Improper averaging could lead to incorrect volume estimates or to volume precision that is not warranted. In the conclusion of the paper, the authors also mention thickness distributions as one reason to prefer numerical models over scaling. I agree, but the limitations (or at least inherent dangers) of an inversion should be acknowledged earlier in the paper, particularly when the thickness distribution could potentially be calculated on too fine of a grid.

This will be mentioned in the discussion of the revised version.

Pg. 4824: The sensitivity analysis seems appropriate. However, I have one note of concern. The scaling techniques have three notable sources of sensitivity/error – errors in the area A, errors in the scaling exponent gamma, and errors in the multiplicative scaling factor c. (Of these, it could be argued that the scaling exponent is fixed by the scaling physics and technically cannot be a source of error, but that's not too important here.)

On the other hand, my understanding of the numerical models is that they have many more free parameters. But page 4825 mentions only f and tau. What about all of those parameters mentioned on page 4822 (like hmin, hga, r, and n)? I know they are optimized, but they are *still potential sources of error. And what about the potential variability introduced by the selection of the 50m elevation intervals? Ditto for the parameters in Huss and Farinotti (2012); to count the free parameters, I just reread this paper, and there are very many. I do recognize that each of these model parameters has been selected to give a good fit to available data, but then the same could be argued about the scaling parameters of Arendt et al (2006) and Chen and Ohmura (1990). So if you are allowing these "fit" quantities to vary in the scaling approaches, then shouldn't we consider the potential variability of "fit" parameters in the models? If not, explain why.*

We acknowledge that a number of model parameters are applied to calibrate the GlabTop2 model and some of these parameters do not directly represent physical properties. A good example is the 50 m elevation interval which is here applied to reduce the impact of DEM inaccuracies. However, there are also various parameter where one is not or less free to optimize them for the purpose of model calibration. h_{min}, for instance, represents the minimum ice thickness for the grid cells at the glacier margin. A value needs to be chosen that agrees to expected ice thickness at the glacier margin and averaged over the size of a grid cell. Thus h_{min} approaches 0 with decreasing grid size.

Of course there is always a certain tolerance within which such parameters can be defined. However, it is important to mention that they are not used to tune the model.

In the revised version, we will add supplementary material where it will be explained and illustrated, how parameters like h_{min}, h_{ga}, r, and n of the GlabTop2 model have been determined (see also reply to Review#2).

Regarding the method by Huss and Farinotti (2012) the reviewer is right in stating that there are many parameters. However, none of them is a completely free calibration parameter. All parameters actually describe individual processes of glacier dynamics and were validated on their own with appropriate data (see all details in their paper). Given the limited amount of ice thickness data worldwide it would indeed be impossible to "optimize" all parameters, and would not make sense either. Related to the present study it is important to emphasize that no parameters were recalibrated for the Himalayan-Karakoram region. The entire parameter set is directly based upon the calibration-validation presented in Huss and Farinotti (2012).

In other words, it might be worth noting that an advantage of the scaling technique is that very few parameters are necessary, and that they can be fit to a particular region (as done in Arendt et al, 2006); and therefore the sources of error are more easily controlled and understood in a scaling approach. By the same token, an advantage of the numerical approach is that the extra free parameters give the solution more flexibility.

Agreed, this will be mentioned. However, it should be highlighted again, that these 'extra free parameters' are used in an attempt to correctly represent physical processes rather than only tuning the model to meet desired results.

Pg. 4825, line 21: Ah, wonderful. My faith in glaciology is restored by the sentence "Furthermore, V-A [scaling] relations are designed to estimate the volume of a larger glacier ensemble, but are not suitable to assess the [precise] volume of individual glaciers, which further hampers their comparison with [individual] measurements."

Excellent! Very few publications acknowledge this important shortcoming of scaling approaches, and I am very pleased to see it acknowledged here.

By the way, I recommend inserting the word "scaling" as I have done above – there could be other types of volume-area relationships that do not involve power laws. And I recommend inserting the word "precise" as I have done above; we can use volume-area scaling to give an order of magnitude estimate of a single glacier's volume, even if it is entirely *inappropriate to use scaling to get a more precise estimate for a single glacier. I would also insert the word "individual" as I have done above; because we could still compare sets of volume calculations to sets of measurements. For example, comparisons could be done between probability distributions or between sums of volumes (total or mean volume of all the glaciers in each set).*

Thanks for the support! We agree to include the "scaling" as suggested. However, we prefer to keep the second part of the sentence; assessing the volume of an individual glacier with the accuracy of (possibly) only an order of magnitude should not be promoted. Unfortunately this is already done often enough without considering this low accuracy. And related to "individual measurements", see our general comments above.

Pg. 4830, line 1: Do you mean "Conceptual aspects" or do you mean "Conceptual shortcomings"? You only present shortcomings, but I'm sure there are some conceptual advantages which might be included under the title "Conceptual aspects".

Pg. 4830, section 5.2: Everything had been going so well in this paper, but at this point, the paper's logic really derails. Section 5.2 needs a major rewrite in order for this paper to be publishable. To be blunt, the paragraph starting on line 2 has so many conceptual errors, that I wonder if the authors have an agenda beyond a detailed comparison of the different techniques. Some of these statements can't be made with a straight face, and it's almost like the authors are winking or goading. I hope and assume that is unintentional.

Yes this is definitely unintentional. As mentioned in the general points above, we will remove and rewrite this paragraph to avoid this misunderstanding.

[We acknowledge the detailed feedback given here, but we ignore certain comments, since they are already made and countered in the past, and actually are not relevant anymore after having removed section 5.2

We acknowledge the usefulness of V-A scaling and will therefore use this approach in our revised version as well, without entering such a detailed discussion.]

Pg. 4830, line 11.

"(iii)..., (b) the scaling parameters are determined on the basis of only a few hundred glaciers with measurements at most."

No, that's not entirely true. The scaling exponent can be derived from theory, and two of the three scaling analyses in this paper use that theoretically derived value of gamma = 1.375 (Bahr et al, 1997; Arendt et al, 2006). I will agree that (to date) the scaling parameter c has been determined from data. But I'm not sure that a few hundred measurements can be called insufficient. Plenty of valid regressions have been done with less data. Bahr (1997, Water Resources Research) goes a step further and derives c as the mean of a probability distribution. It's the same data, but it's a different technique for which a few hundred data points is usually considered more than sufficient.

I can entertain the notion that there might be biases in glacier sizes that could impact the calculation of the mean value of c, but you would need to say this and then back it up with evidence. The appendix of Bahr (1997) shows a reasonable distribution of c that would not seem to support a size bias, but no specific tests were done.

Perhaps a more reasonable claim would be that c could vary from region to region. If so, there is insufficient data to establish this regional variability. That might be a real shortcoming, and it might be worth mentioning in this paper.

Ok, this will be changed. It is true that the determination of the scaling parameters should be separated for c and γ. This will be rewritten, considering the related literature. Probably it should be mentioned that the fixed determination of gamma assumes glaciers in steady-state conditions, which does not apply for most of the glacierized regions.

Pg. 4830, lines 14-15. The references on line 14 and 15 are good. They support your point that the scaling parameters vary. But why isn't the same potential variability discussed with respect to the numerical models? Surely there could be regional, temporal, or other variations in f, tau, etc.?

Yes, f and tau probably have different characteristics due to continentality for instance. But of course there is much less literature available regarding the variations of the parameters used in the numerical models. In the HF method, specific functions or direct calculations explicitly account for the variations of the parameters for individual glaciers based on physical relations (cf. Huss and Farinotti, 2012).

One of the advantages of the physically based models is that they allow a more direct representation of the physical processes. In statistical (scaling) approaches, regional or local characteristics such as continentality or slope cannot be considered or if, only by adjusting the parameters applied to the entire data set. As a simple illustration one can imagine two glaciers with different characteristics (e.g. different surface slopes) but identical areas. Using an area depending scaling technique, these two glaciers will always get the same volume or mean thickness, whereas with physically-based models it is possible to obtain different volumes / mean thicknesses by considering, for instance, the surface slope. Regional characteristics such as continentality basically are causing different scaling parameters for different regions in order to represent the different glacier characteristics.

Pg. 4830. For a balanced presentation, please include another paragraph in this section that discusses the "conceptual aspects" of numerical models. The "variety of possible combinations for the [model] parameters" is one aspect. The necessary simplifications from full Stokes models is another aspect (albeit possibly less important, in my opinion). I am sure there are others aspects worth mentioning. Discussing the conceptual difficulties of scaling without discussing the conceptual difficulties of numerical models does not give your paper the appearance of a balanced presentation.

Will be considered in the rewriting of this paragraph.

Pg. 4830. Continuing with the conceptual errors in section 5.2... Figure 8 is used in support of the arguments in section 5.2, but why is "measured" in quotes in the caption for Figure 8 and again on page 4825, line 17? If this is not real thickness data, then is this plot even a fair representation of the thickness-area relationship? Or do you mean to imply that GPR derived thickness is not an actual measurement, but is instead a complicated derivation from measurements? Please explain this.

The quotes should point to the fact that a direct measurement of mean thickness is not possible. Mean thickness is always an interpolated result of individual thickness measurements. This is fine if the mean thickness value is based on a sufficient number of measurements regularly distributed over the entire glacier. However, again due to practical reasons, this is not always the case. Often measurements (GPR profiles) are restricted to flat glacier parts that are easily and safely accessible, whereas steep and crevassed glacier parts are normally not measured (see for instance Binder et al., 2009 and Fischer, 2009). This can introduce a bias, since the easily accessible flat parts of the glacier might have higher ice thicknesses than steep (and crevassed) glacier parts.

I suspect the data in this plot is volume data (compiled by Meier, Bahr, Cogley, Grinsted, etc.) divided by glacier area. I'm not sure because the authors don't say it explicitly. But if so, then this is a plot of V/A versus A. That is an autocorrelation, and it will artificially increase noise (because A is in the denominator). Well, that's ironic!

For this plot we used mean ice thickness values as published by Grinsted (2013) in the supplementary material. The data comes from Cogley (2012) and was collected by G. Gogley and R. Hock, as stated in the figure caption. We did not divide volumes by area, but in the table both mean thickness and volume are given, it is thus not possible to trace back which of the two values is based on measurements and which is derived from a multiplication or division with area, respectively. According to the IPCC AR5 (Vaughan et al., 2013) and Cogley (2012), the thickness values are based on measurements, see also the general comments made above.

*By the way, scaling theory requires that the thickness be *measured* in the same place on every glacier. For example, it can be the average thickness (measured, not calculated), the average thickness on the centerline, the average thickness across the equilibrium line, the single value of thickness measured at the intersection of the equilibrium line and the centerline, etc. But whichever is chosen, it needs to be the same quantity in the same place on every glacier. If the values are not consistently measured in the same place on each glacier, then the scaling relationship is not guaranteed; at best, the scaling relationship would be very noisy, and in a worst case scenario, the scaling relationship would not be identifiable at all.*

We absolutely agree. The rhetoric question is: At which place can the average thickness be measured? This always requires an interpolation/averaging of several measurements. Measuring single thickness values at a specific point is an interesting approach, but to get volumes from out of the areas and these individual measurements (wherever they are taken) would require a correction factor.

*Although I am surprised at how good Figure 8 looks considering that it spans only one order of magnitude (see above), this plot would undoubtedly be less noisy if the mean thickness was *measured* as the same quantity on every glacier. As the authors note, the thickness has rarely been measured in the Himalaya-Karakoram and is interpolated and extrapolated from measurements that were not made in the same place (as the same quantity) on every glacier. Therefore it is probably a mistake to over-interpret the scatter in Figure 8. At the very least, the scatter in this plot needs a much deeper analysis.*

Data used for Figure 8 does not come from the HK region, but is taken from Grinsted (2013), who published the "glacier volume database" used for his publication, which contains data collected by Graham Cogley and Regine Hock that was originally published in Cogley (2012). We will reword the figure caption to make this clearer. We hope the source is clear in the reference given in the caption.

Pg. 4833, line 11. Have you considered using the numerical models to estimate appropriate scaling parameters gamma and c for volume-area scaling in the Himalayas? Would the performance of volume- area scaling be improved if more appropriate scaling parameters were used for the Himalaya? Either way, this would be a very interesting result, and would be a more thorough and in my view essential exploration of the two different approaches (scaling versus numerical modeling).

In fact, until you have appropriate volume-area scaling parameters, can you really claim that the modeling approach is superior? Yes! I think you can make that claim based on the idea that the modeling can be tuned to the specific region without (unavailable) volume data. But if volume-area scaling will give results that are more in line with the numerical modeling *when using the correctly tuned parameters for c and gamma, then I think your claims should be much more nuanced.*

I realize that this constitutes a major change in the manuscript, but it would very much strengthen your results, and your comparisons would be much more definitive.

This is done in the first paragraph of section 5.3 (p. 4830): We applied the region-specific thickness-area scaling parameters provided by Huss and Farinotti (2012) to our inventory dataset. These parameters were determine according to the best fit to their calculated glacier volumes, thus the good agreement with the results from the HF model is given, but it shows exactly that – as you say – the missing input data is the main reason for the differences in the resulting ice volumes.

Pg. 4833, line 20. This line implies that the modeled results are based on ice dynamics while the scaling approaches are not. Consider rewording or removing this. The scaling approach is based on a derivation from the underlying physics/mechanics/dynamics.

Will be reworded.

Pg. 4853, Figure 8: I assume this is calculated from volume data that was collected globally rather than locally in the Himalaya. The global nature of the data should be mentioned because the rest of the paper is dealing with the Himalaya-Karakoram. At first glance, it is also very slightly confusing to have a box that says "V-A scaling relations" on a plot that shows a thickness-area relationship. The dashed lines on the plot are not the specified volume-area relationships but are instead V/A.

Yes, as mentioned above we will reword the figure caption to make this clearer. And (also mentioned above), we will use T-A approaches in the revised version in order to avoid such confusions.

Summary

(1) Please rewrite section 5.2 to remove the inaccurate claims, preferably by dropping the discussion of area-thickness scaling altogether. For a more balanced presentation, also add to this section a discussion of "conceptual aspects" of numerical modeling.

Will be done

(2) Use your numerical models (or other techniques) to tune the scaling parameters c and gamma to the Himalaya. Until this is done, the scaling techniques are not being given a fair assessment. So far, you've only shown that global values and Alaskan values for c and gamma don't work so well in the Himalaya.

This was actually done in the first paragraph of 5.3. We will rewrite this part to make this point clearer

(3) Otherwise, well done. I agree with your general conclusion that the numerical models offer many advantages over scaling. A more definitive and nuanced comparison will enhance your conclusion considerably.

Thank you.

Comment by W. Haeberli

Most of the points raised in the valuable comment by W. Haeberli are treated above already. The only remaining point is related to his suggestion to specify the average basal shear stresses that would be required for the stress-related approaches (GlabTop2 and Haeberli and Hoelzle 1995) to obtain the same average ice-thicknesses as calculated by area-related estimates. This is a very helpful suggestion for an inter-comparison of the different approaches and will be discussed in the revised version as well.

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REVIEW #2

Italics: copied from the review
Normal font: Author reply

First, we would like to thank the reviewer 2 for the helpful and detailed review, which will certainly improve our article.

General comments

1) WHY THE HYMALAYAS?

We agree with the reviewer that the HK region is not ideal for a methodological comparison due to the lack of validation data. However, as outlined in the introduction, knowledge of glacier volumes is very important in this region. For the revised version we redirect, as suggested by the reviewer, the focus from the methodological comparison to the resulting ice volumes (see general points given in the very beginning); hence, the importance of the topic should justify the choice of the region.

2) ABOUT GLABTOP2

A number of issues and concerns about the methodology applied in GLABTOP2 were raised in this review. We try to answer these points below and have accordingly modified the text of our manuscript:

(A) ... seeing that no single word is spend in discussing why a relation that was derived for estimating the mean ice thickness along a central flow line (Eq. 3), should suddenly apply locally! I don't want to exclude that the authors found very good reasons for doing so, but these reasons should be presented!

We recognize that we have not provided sufficient details to make our methodology comprehensible. The reviewer is correctly stating that Eq. 3 is intended to be used along central flow lines and not locally. Points close to the glacier margin often have a very similar surface slope than the glacier center and thus Eq. 3 will result in identical thickness although ice thickness at the margin is certainly different than in the glacier center. The issue in the current version of GlabTop2 is that we do have little control on where random points are picked. Thus we apply Eq. 3 at all random points but subsequently make use of the interpolation of ice thickness to achieve glacier cross sections that are close to a parabolic shape. The mechanism is explained in detail below, and is explained in the supplementary material that will be submitted with the revised version of the paper

(B) ... As far as the authors describe, GlabTop is a deterministic approach, i.e. the result for a given grid cell will be the same at every time the particular grid cell is considered ...

Yes, this is correct, we will state this more clearly

The authors then say that the computations for a particular glacier are repeated 3 times, by randomly selecting 30% of the grid cells at each time. Sorry, but what's the difference with respect of selecting 90% of the grid cells at the beginning?... I really wonder what the author's thoughts are here.

We admit that our argumentation was not detailed enough to make the reasoning behind this approach understandable. We use only 30% of the grid cells because the interpolation of ice thickness is sensitive to the ratio of random cells to margin cells (where h is set to h_{min}). Imagine a glacier tongue that has a surface of constant slope. Applying Eq. 3, we will at any random point receive the same ice thickness, independent on whether the point is located at the margin or in the center of the glacier. If we would use a very high percentage of random cells (e.g. 90%), then the high number of random cells would dominate the interpolation and since all of them have the same thickness value, we would obtain a glacier cross section with nearly vertical side walls instead of a parabolic shape. By comparing theoretical parabolic glacier cross sections to modeled glacier cross sections we determined that the weight of the margin cells and the random cells is optimized at r = 0.3 to achieve glacier cross sections similar to a parabolic shape. A respective figure will be shown in the supplementary material to explain and illustrate this issue more clearly.

The reviewer furthermore asks why we repeat the calculations n times and we try to explain this more clearly in the following. We do random sampling with little control on locations of the sampled points, and thus run a risk that the aforementioned balance of random points to margin points is not given everywhere. Since we need to operate with low ratios of r = 0.3 random points, the latter are often clustered at one location while elsewhere only few or no random points exist. Interpolating over these points results in a "correct" mean ice thickness but the spatial distribution of ice thickness is dominated by the random clustering of the random points. Hence we repeat the calculations n-times with different random sampling and each time interpolate ice thickness. Eventually we average all ice thickness distributions and thereby strongly reduce the effects of the inevitable non-uniform distribution of random points. A similar statement will be included in the updated description of GlabTop2 (Supplement of the revised manuscript).

(C) In order to avoid the problem that the basic equation used by GlabTop diverges for very small surface slopes (see Eq. 3), the authors propose a smoothing implemented through an iteratively growing, square "buffer". The authors are right in pointing at the literature for the reason of such a smoothing, but actually, the literature suggests something very different than the authors actually do! Kamb and Echelmeyer, JoG, 1986, give good theoretical reasons for smoothing the surface topography in flow direction over a length of about 10 times the local ice thickness. I wonder how this can be translated in a squared region defined as the particular region within which the elevation range is 50m!?

We apologize that our explanations were not clear enough to explain our reasoning. Square buffers are centered on a random point and are expanded until the highest and the lowest cell in the buffer are 50 m apart in elevation. Thus square buffers are small (e.g. 3x3 cells) in steep glacier sections and can be quite large in very flat glacier sections. We use the buffers as a simple mean of (1) averaging surface slope and (2) reduce the influence of spurious surface undulations present in the DEM data we rely on. We are fully aware that it would be more correct to average surface slope along the flow direction. However, such a concept was implemented in the first version of GlabTop2. The outcome was spurious variations in ice thickness because calculating local flow direction is hampered by DEM irregularities. Glacier surfaces are usually of a rather smooth character and further experiments have shown that averaging slope over a certain surface area is much more useful to re-establish this character for the purpose of ice thickness assessment. We also considered whether our approach brings the risk of suppressing real variability in surface slope and glacier flow (i.e. as the reviewer states "assume isotropic ice flow within the buffers"). To minimize this risk we always limit the search buffers to the 50 m elevation interval. Wherever the *actual* surface of a glacier shows strong surface undulations, the 50 m criteria will result in small buffers, thus limiting variability of ice flow within the buffer. We are aware that there are situations where even within a 50 m elevation buffer ice flow can be extremely non-isotropic (e.g. ice streams and stagnant ice in close vicinity on flat surfaces of ice caps or the ice sheets), but these are of limited relevance to our simple methodology to assess ice thickness.

3) VOLUME-AREA SCALING. . .

See our replies to the review of David Bahr for the reason of the lengthy discussion and the explanation of the statements made in section 5.2. We are aware of the fact that $V=c*A^g$ is the same as $h=V/A=c*A^{(g-1)}$, for reasons of consistency we will use the latter relation in the revised version of the manuscript.

Detailed comments

P.4813: I suggest adding "Glacier" in front of "ice": You are not dealing with permafrost, lake ice, or other sorts of ice.

Is considered in the suggested new title "Estimating the volume of glaciers in the Himalayan–Karakoram region with different methods" (cf. general points in the beginning).

P.4814: Well, probably not the best reference for supporting the claim: It was not "discovered" by Bolch et al., was it?

Bolch et al. (2012) provide a general review of glaciological studies in this region, therefore we consider it as suitable. Of course this (obvious) statement was also mentioned by others, therefore "e.g." will be added in front. Probably no one "discovered" that glaciers in the HK region have complicated physical access but in the mentioned publication this has been mentioned recently in the context of the topic of the paper.

P.4815: In this context, you may also want to cite the work by Gabbi et al, TC, 2012.

Yes. (We assume the suggestion refers to the publication by Gabbi et al. (2012) in HESS)

P.4815: You are certainly more aware than I am of the "background story" of this particular paper. I would propose you consistently cite Linsbauer's work for the GlabTop approach.

It is correct that Andreas Linsbauer originally presented this approach in Linsbauer et al. (2009). However, this is published in a conference proceeding and the later publications are more detailed and appeared in ISI listed journals, therefore we cited Paul and Linsbauer (2012) for technical and methodological aspects, and Linsbauer et al. (2012) for results of the model. In the revised version, Linsbauer et al. (2009) will be cited at this place in the introduction to have the historically correct order.

P.4815: Consider the formulation "Recently, Huss and Farinotti (2012) used the RGI for deriving the first estimate of the ice thickness distribution for all glaciers around the globe."

Done

P.4816: I would suggest the following formulation: "Using the same glacier inventory dataset as the present study, Bolch et al. (2012) highlighted that estimations of ice volumes in the Himalayas are [...]"

Done

P.4816: Isn't this data included in the RGI by now? If not, why not? (The latter question is for one of the co-authors ;-))

No, glacier outlines from ICIMOD outside Nepal are still not included in the RGI, but could be used for this study. Negotiations are ongoing, but none of the co-authors is in charge of this decision…

P.4816: As said in the general comments: If this is the primary goal, you picked the worst possible region...

We agree that for a pure comparison of different methods, one of the more densely measured regions should be chosen to allow for sound validations. As mentioned in the general points, the main aim of the revised version is to come up with the best possible estimation of the ice volume stored in the HK glaciers.

P.4817: Previously (Page 4816, Lines 3-4) you stated that you used the outlines by Bolch directly. This is a contradiction!

This was not clearly formulated. Both are true: We used the same glacier inventory as Bolch et al. (2012), which is a collection of different remotely sensed glacier inventories, they did not compile their own outlines. Since the outlines represent the fundamental dataset for the all the calculations, we believe it is worth mentioning the individual sources. Will be re-formulated.

P.4817: Well, where did you get the data from then? Nobody is acknowledged in the acknowledgements for that...

S. Bajracharya who is co-authoring the paper provided the data, therefore nobody is mentioned in the acknowledgements for this.

P.4818: Check the syntax and the exact meaning of the sentence: Here you imply that Chen and Ohmura are... parameters!

True. Will be reworded anyway, since the parameter set of Arendt et al. (2006) is not considered anymore in the revised version.

P.4818: include the number of glaciers that were used in this case - similarly as you died for (i).

Is now obsolete.

P.4818: , and... [surface slope is not the only variable, is it?]

True, "vertical glacier extent" was missing here

P.4819: Please provide arguments for this particular choice. [Shape factor set constantly to 0.8]

0.8 is the typical value of the shape factor for valley glaciers (for other glacier types it can be smaller), this has been added, as well as the reference to Paterson (1994). For simplicity we assume the shape factor to remain constant along the glacier and among the sub-regions.

P.4820: Well, for deriving $\pi/4$ you need an assumption about the depth-to-width ratio. State what this assumption is explicitly!

This division is used to account for the semi-elliptical cross profile. Since Haeberli and Hoelzle (1995) used this factor in their publication it was also applied here. Will be explained in the revised version.

P.4819/20: (1) You can shorten this part. The only thing you need to state is that the original relation was derived by considering the mean slope of the glacier centerline (you can even remove Eq. 6, in my opinion), whilst now you derive a mean slope from the DEMs.

During presentations of the topic at conferences we saw that the audience had problems understanding the reason of this problem when it was presented in the suggested way. It needed to be explained that (i) glacier length is/was a standard parameter in the tabular glacier inventories but that extracting the glacier centerline from shapefiles is hardly possible (or at least not straightforward), and (ii) to show how the mean slope exactly is calculated with the two kind of glacier inventories (tabular and 2D outlines). We agree that it could be mentioned in the text, that for tabular inventories mean slope can be calculated "using the arc tangent of glacier length and the elevation extent", but we consider the equation as more elegant.

(2) Why this discretization into size-classes? Why not a smooth solution by fitting a (quadratic?) function to the data you show in Fig. 2?

This would avoid the choice of size-class boundaries, and my annoying question of (a) justifying them, and (b) assess the sensitivity of the choice.

Regarding the total volume, the correction factor for larger glaciers has a much higher influence. Since there are only two glaciers larger than 500 km^2, their difference between α_{DEM} and α_l would largely influence the fitted correction function. Size-class boundaries were chosen iteratively so that the means of each size class are significantly different from each other (s. P.4820, l.12-13). The size-class boundary choice influences the resulting volume for some smaller, individual glaciers but its sensitivity s very low regarding ice volume estimations on a larger scale.

P.4821: Not sure if this description is really necessary. The formulation is rather involved, and nobody would imagine something else, I believe. ["The calculation of ice thickness is grid-based and requires a DEM and the glacier mask as input. In a first step, all groups of glaciers sharing common borders, i.e. glacier complexes, are assigned a unified ID. All following steps are performed for one ID (i.e. all cells of a glacier complex) at a time, disregarding all cells of differing IDs"]

Yes, the second part will be shortened. But the first part of the formulation describes a fundamental step of the methodology (unifying glacier ensembles) and will be retained.

P.4821: (1) Before you start with the technical description, tell briefly that what you are aiming at, i.e. avoiding very small α. This would facilitate comprehension.

Will be done

(2) Please explain why you think that exchanging a parameter (the minimal surface slope) with another (the elevation range that needs to be covered by the buffer), would be an advantage! [this is what your wording "obsolete" implies]. Without having thought the issue true, the cutoff seems at least to have the advantage of being predictable in its outcome...

No parameter was exchanged here, neither GlabTop2 nor the original GlabTop Model by Linsbauer have a slope cutoff criterion, both work with minimum elevation ranges of 50 m that need to be covered. The formulation "and thus makes a slope cutoff (i.e. a minimum local slope considered) obsolete" refers to other models like from Farinotti et al. (2009) and Huss and Farinotti (2012), were such a threshold of 6° is applied. The absence of a slope cutoff criterion indeed leads to strong overestimations of ice thickness on large arctic ice caps where GlabTop2 was also tested. These issues, however, are not relevant in the context of the HK region but we are working on solving these difficulties.

(3) You mention the work by Kamb and Echlemeyer later. As said in the general comments, I would say that what you do here is in obvious contradiction to what their theoretical considerations suggest.

See reply to General Comments (reply to “2. ABOUT GLABTOP2”, point (C)).

P.4821: And what's that? Define it here in the text (and not in the figure caption). [h_{min}]

Done

P.4821: As pointed out in the general comments: Please provide arguments for why a relation derived for the average thickness of a central flowline should be applicable locally.

See reply to General Comments (reply to “2. ABOUT GLABTOP2”, point (A)).

P.4821: This is a minimal ice thickness that you impose for "marginal glacier cells", correct? State it explicitly!

Yes it is the minimal glacier thickness, but not assigned to ‘glacier marginal cells’ (which are within the glacier) but to ‘glacier adjacent cells’ (which are outside the glacier). The thicknesses of ‘glacier adjacent cells’ (h_{ga}) are not incorporated into the final glacier volume (only ‘inner glacier cells’ and ‘glacier marginal cells’ are counted), but it influences the thickness of glacier marginal cells through the interpolation.

*P.4822: See general comments: What is the difference of choosing n times r cells and choosing one time n*r cells?? The answer is simple: There is none! (The answer "there might not be enough grid cells" is really not a good one, since you can easily chose the cells by "draw with replacement". But since your approach is deterministic, this has no added value anyway.)*

This step is important to obtain realistic, near parabolic-shaped cross sections, see reply to General Comments (reply to “2. ABOUT GLABTOP2”, point (B)). This is also related to the point above, regarding thickness calculations of the central flowline and towards the glacier margin. We agree that at first glance this step seems arbitrary, in the revised version of the manuscript it will therefore be explained in more detail, including a figure in the supplementary material.

P.4822 Please state explicitly which assumption are required for translating "average over ten times the ice thickness in flow direction" into "average in a squared region with elevation range 50m"!!

The suggestion by Kamb and Echelmeyer (1986) of considering the “average (slope) over ten times the ice thickness in flow direction” cannot be implemented directly, because the ice thickness is the variable to be calculated. Considering elevation bands of 50 m, as implemented in the original GlabTop version, guarantees, that larger ice thicknesses (glacier parts with smaller slopes) are calculated over larger distances (even though the distance considered might not be exactly ten times the resulting ice volume). For GlabTop2 this concept has been adopted, but is applied to a squared buffer. Tests with glaciers with simple geometries showed, that the buffer has the advantage of being less sensitive to DEM artifacts (see also reply to General Points, “2. ABOUT GLABTOP2”, point (C)).

P.4822: According to what you said so far, this is the "percentage of randomly selected grid cells". How can this "govern the SHAPE of the glacier cross section"?

See reply to General Points, “2. ABOUT GLABTOP2”, point (B).

This [h_{ga}] is only the ice thickness of the marginal grid cells. It doesn't "govern the SHAPE" as such, does it?

h_{ga} is the thickness of ‘glacier adjacent cells’ but not directly of the glacier marginal cells, and therefore has an influence on the shape of the cross sections because the glacier adjacent cells are used in the interpolation. See also the reply to comment on P.4821 above.

P.4822: So the shapes you generate are random! Why would one want to compare a random shape to measurements?? This doesn't makes sense, does it?

Although there is a limited amount of data for validation of modelled ice thickness, the available data still show clearly that the performance of GlabTop2 is generally good and very similar to the approach of Huss and Farinotti (2012). If our calculated shapes were random, such an agreement would be extremely unlikely. Then again we are aware that we need to more clearly explain the reasoning behind GlabTop2 and will provide more detailed explanations in the revised manuscript (see the reply to general points “2. ABOUT GLABTOP2”.

P.4822: Here the consideration of another region would seem a particular good reason: It would be so much nicer calibrating those parameter to measurements, rather than to "theoretical parabolic glacier cross sections". And by the way: (1) How do you perform the calibration? (2) If the aim is to obtain parabolic cross sections, it would be easier to impose this shape in the model itself, rather than trying to "fit" this shape through some model parameters. So your line of argumentation is not convincing.

We admit that the issues related to GlabTop2 pointed out in the review (choice of r and n, squared buffer to determine elevation range, calculating ice thicknesses for central flowline at random points, and the influence of the parameter h_{ga}) require further explanations. This will be considered in the revised version, where more details addressing these issues will be given as supplementary material. There (1) will be described in detail and complemented by a figure.

P.4822/23: See general comments: I really don't get the idea behind this approach. Since your computations at the level of individual grid-cells are deterministic, it would be a lot more efficient to compute the ice thickness at every point (and if this is really unfeasible, at least for 90% of the glacier, as you apparently can do).

See above

P.4823: The wording "is based on" suggests that you performed some modification to the original approach: Describe them!

Changed to “is” (without “based on”).

P.4823: There is a whole series of parameters that are mentioned implicitly here (apparently, for example, there is one controlling the resolution of the computational grid; some controlling "continentality"; some controlling the melt for debris covered glaciers etc.). Please state them explicitly, as you did for GlabTop.

Will be described in more detail in the revised version of the paper.

P.4824: My understanding is that a "shape factor" refers to the cross section of a glacier. I therefore struggle in figure out what a shape factor for a "given point along the glacier" means. Explanations are required.

True, reworded to "Furthermore, the shape factor and the basal shear stress are calculated for each glacier and every point along the glacier, respectively".

P.4825: You must be aware of the recent work by Paul et al., AoG, 2013, which is probably the better reference than Cogley (2011), for using 5% as a typical uncertainty for the glacier area. However, more importantly: Isn't the accuracy which is of relevance here, the one which refers to the splitting (or not) of glacier complexes? I would think so!

Yes, Paul et al. (2013) is added. It is well true that the glacier separations have a fundamental influence on the V-A scaling results (as stated in the discussion, P.4829 l.17-19, and P.4820 l.16-19), and will be mentioned here as well. However, it is much harder to come up with an accuracy estimate for glacier separations, and assessments normally are restricted to "algorithm fails" or "algorithm succeeds" (see for instance Kienholz et al. (2013)). Paul et al. (2013) list reasons for why it is hardly possible to assess the accuracy of a glacier separation method. Therefore we have to use the arguments given to determine the extent of area alterations for our sensitivity assessment.

*P.4825: And what's about all other parameters, such as h_min, r*n, h_ga??*

The determination of h_{min}, r, n, and h_{ga} will be explained in the supplementary material. However, tests showed that their influence on the results is negligible compared to the influence of changes of τ and f. This will be mentioned explicitely.

P.4825: Well, no! This is cheap! At least re-present the most important results of the analysis! ["For uncertainty and sensitivity analyses of the HF model, see Huss and Farinotti (2012)."]

In general, we try not to repeat analyses already done and well-described in other publications, this would add a lot of unoriginal content. The idea is to apply different existing approaches in order to get an estimate of the volume of HK glaciers. The uncertainty and sensitivity tests provided here thus only include aspects that are specific to this application (in the given region with the given input datasets).

In order to meet the reviewer's comment we will however provide some very short results of the sensitivity tests presented in Huss and Farinotti (2012)

P.4825: This is really absolutely disappointing: Why did you select this region then???

The region was selected, because knowledge of ice reserves stored in the HK glaciers are of great importance for several applications, see introduction. But we agree that the lack of validation data is not supporting the choice of this region for a comparison of the method, we therefore redirected the focus and aim of the study towards the best possible estimation of ice volumes of HK glaciers using different approaches.

P.4826: Well, the parameters were derived for Alaska...

Agreed. In the revised version these parameter set is not considered anymore (see also General Points).

P.4827: By the way: If GlabTop uses rasterized outlines, why you did not performed a sensitivity analysis on the effect of glacier area as well? Wouldn't be to difficult to implement...

Implementing this would involve a lot of subjectivity. Of course the outlines could just be expanded or contracted equally in all directions, but this is unlikely the real variety of different interpretations during glacier mapping. Uncertainties of glacier areas are mainly related to glacier debris-covered glacier parts and the separation of individual glaciers (Paul et al. 2013). Thus, whereas for the glacier area (the number) used for V-A scaling, a modification of ±5% can be easily and objectively implemented, but this is not possible for altering the 2D outlines as it would be required for GlabTop2 or the HF model.

P.4827: Please provide arguments for why the ranges f=[0.7,0.9] and \tau_max=[1.2, 1.8] should be plausible. Why not f=[0.5,0.9] and \tau_max=[x,y]?

Based on literature (e.g. Patterson, 1994 and Paul and Linsbauer, 2012 for *f*, and Patterson, 1994 and Huss and Farinotti, 2012 for tau, see also comment by W. Haeberli and Fig. 9), these ranges seem to represent the limits of realistic estimates. More details and references will be given in the revised version.

P.4827: So are you suggesting that the uncertainty of the results yielded by the HF-method is as low as +/-10%? This is rather hard to believe... (especially for a region where you can't assess it - I know, that's the same comment again...)

This is cited from Huss and Farinotti (2012) and based on their sensitivity tests and the effect of DEM uncertainty, see Table S3 in their supplementary material. Of course real uncertainties might always be larger, however, for a large sample of glaciers random uncertainties might (partly) average out.

P.4827: What did you do exactly for the comparisons? Later one guesses that the actual measurements were gridded somehow. Is this actually the case? Explanation is required.

Ice-thickness values as calculated with the two models were taken at the location of the ice-thickness measurement; none of the values was modified. In some cases, the reference values were used as reported directly by the persons who conducted the measurements; in other cases, the measurements and locations have been inferred from the cross-sectional profiles. This is clarified in the revised version.

P.4827: Are you sure that you don't mean "cross sections"? The number of "points" is only a matter of sampling resolution...

We compare our model results to 86 measured ice-thickness values. The locations of these 86 values are more or less equally spread on several (but much less than 86) cross sections. Of course this number depends on the sampling resolution, but that is what we did here.

P. 4827: This is absolutely disappointing! You mentioned x tunable parameters! There is, by all means, no reason for why the average deviation shouldn't be a round round zero! For the GlabTop-approach, increasing h_min by 20m would already help! I'm not necessarily suggesting that you should do that, I'm only saying that the mean deviation has very limited informative character... State (at least) the average of the ABSOLUTE deviation between individual "points" (or grid cells, or elevation bands, or whatever you actually compared).

We provide a validation, not a calibration, thus the idea is to validate the results with field evidences, not to tune the parameters to reach a best fit. Of course, this data could as well be used to tune the models to optimally fit the measurements, but there are three reasons why we did not do this: (1) As described in the methods section, the parameters of both the GlabTop2 and the HF model are – at least to a certain extent – physically-based; tuning them to reach best-fit results, such as increasing h_{min} by 20m, could thus lead to unrealistic values of these parameters. (2) Tuning the model to fit the presented measurements might lead to unrealistic results at other, unmeasured locations. As mentioned several times in this review, the sparse validation material available for the HK region is far from being representative for all the different conditions occurring in this heterogeneous study area. Thus this data is only used for validation instead of calibration. (3) All approaches used here have been elaborated and validated in individual studies. The idea here is to use such existing approaches to get a best estimate of the ice volumes of the HK glaciers. Modifying some basic parameters, however, would require a re-assessment of the performance of the approaches, which is neither the goal nor possible with the sparse data available. In Fig. 7 more values are given beside the mean deviation mentioned in the text. Repeating all these numbers in the text was considered as not very helpful.

P.4828: Where is this number coming from?!? And what does it mean at all? None of the methods you applied will give you "volume=0" for a particular glacier. So what does "100% difference" mean?

Will be revised explained more specifically in the revised version. “More than 100% difference” means, that the resulting ice volume for some region is more than double the estimate from another approach. I.e. 6’455 km^3 for the entire HK region using V-A scaling according to Arendt et al. (2006) compared to 2’955 km^3 obtained with GlabTop2. This is explained on P.4826, L.21/22.

P.4828/4829: Based on our model results, the SLE of the HK glaciers is 16.1mm for the V–A relation by Arendt (2006), and to 7.4mm for GlabTop2 Assuming a density of 900 kg m^3. [And what's about the other methods?]

Here we presented the SLEs for the largest and the smallest resulting total ice volume, in order to show the range of the results. In the revised version, Arendt et al. (2006) will be replaced. In the revised version, SLEs will be given for all resulting ice volumes.

P.4830: Sorry, what are you plotting exactly? Say it in the text: The data in the supplementary by Grinsted (2013)...

Will be revised accordingly.

P.4830: See the review by David Bahr and the general comments.

As mentioned, the entire paragraph 5.2 will be rewritten (see also General Points and reply to the review by D. Bahr).

P.4830: Well, there is only one set of parameters if you consider all measurements that are available so far. Check out the recent Farinotti and Huss, TC, 2013, for the reasons of why using such parameters is a good idea.

Agreed, provided that all available measurements are considered. Nevertheless in literature, including (but not restricted to) the cited articles, there are many examples where different possible parameter combinations are suggested.

P.4830: I don't see the usefulness of this comparison and discussion: OF COURSE calculating the total volume with VA-scaling, with parameters that are derived from one particular model, will give you "similar results" for the total volume as the model that was used for the calibration! That's what linear regression is good for...

Actually, given the relatively large size of the sample that you consider, there shouldn't be any significant difference at all. This is either due to the change of the inventory, or... a wrong parameter estimation in the scaling relation!

It is of course no surprise that using the V-A (or rather T-A) parameters suggested by Huss and Farinotti (2012) lead to the same ice volume as the HF model itself. However, it is not obvious that they are also comparable to the volumes obtained by GlabTop2, which was not used for calibration. This comparison is included to emphasize with an example that V-A (T-A) relations indeed lead to results comparable to results from the other approaches. Calculations with scaling parameters obtained by modeled ice volumes is by the way explicitly requested by Reviewer #1.

P.4831: This sentence implies that the approaches by Marzeion et al., Grinsted, and Radic et al. are different than the approaches you used here. Please explain briefly what those differences are.

We will revise this sentence. The V-A parameter combinations discussed at this place are other examples of possible parameter combinations that are used in other studies to estimate ice volumes on a regional or global scale. We do not consider them as being fundamentally different from the ones used in the results section. But in our view it makes no sense presenting 6 or even more parameter combinations for the results section. We therefore restricted the parameter sets to 3 combinations that were applied to HK region as well in Cogley (2011). In the revised version, the parameter set from Arendt et al. (2006) will be replaced.

P.4831: Not a very important comment, but: Although the glacier length is not included explicitly as a number in the RGI, it shouldn't be too difficult deriving one from the data. It's definitively easier than assigning an ice thickness distribution to all glaciers!

An automated derivation of glacier lengths is not straightforward (cf. Le Bris and Paul, 2013), otherwise we would of course use such a methodology which would allow a direct application of the approach from Haeberli and Hoelzle (1995) without the detour of the slope correction.

P.4832: "Fewer" than what? You hardly used any measurements!

True, changed to “fewer digitized glacier lengths”.

P.4832: This is a "speculation" (the correctness of which could even be assessed rather easily...)

True, will be reworded. Avoiding the slope correction does not necessarily improve the correctness of the resulting ice volumes, but it would allow calculating the slope value directly and in the same way as originally suggested instead of deriving it via the applied size-dependent slope correction.

P. 4833: These results already existed... [“The results are important in the context of improved estimates of water stored in the Himalaya region, both in view of fresh water resources and sea level rise.”]

Yes, estimates of these numbers already existed, as cited in the introduction, but here we provide “improved estimates”, which certainly makes it worth mentioning in the conclusions.

P.4834: Again: WHERE IS THIS NUMBR COMING FROM?? It is certainly inappropriate including it in the conclusions! [“… derivations of up to 100% or even more”]

As mentioned above, this number comes from comparing the glacier volumes obtained with the different methods. It comes from the results shown in Fig. 4 and mentioned, for instance, on P.4826 L21/22. But since this formulation seem to have caused confusion we will reword this to “results deviate by a factor of two”.

P.4834: Well, this is definitively not a "discovery" by Bolch et al., is it?

Not a “discovery”, but this statement (the need for more ice-thickness measurement) is mentioned by Bolch et al. (2012) as one the proposed steps to close existing knowledge gaps on HK glaciers. Since this latest review study of this region mentions the same point as we do, we think it is worth citing it here. However, “e.g.” will be added in front of the citation to indicate that this issues might had been raised in other studies before.

P.4846 (Fig. 1): Of no relevance. [mentioning “as well as by Bolch et al. (2012)” in the figure caption]

We will remove this. It was mentioned to clearly state that this data set was used before.

P.4847 (Fig. 2): Please plot log(area) on the abscissa (or use a logarithmic scale) / Once you have the log(area) abscissa, this plot is superfluous..

Will be done in the revised version.

P.4847 (Fig. 2): Oh! So even more hidden parameters here... / How many? State the number. [“… for selected test glaciers, including the linear regression lines …”]

Well they are not at all “hidden”, but extensively discussed and explained on P. 4819, L. 19 to P.4820, L. 13. Looking at the figure we thought it is superfluous to mention that one regression was calculated for each of the three classes… but we will mention this number for the sake of clarity.

P.4848 (Fig. 3): Remove the semi-transparent blue color of the blue glacier outline.

We prefer to keep this transparent overlay because it (i) helps to separate ‘inner glacier cells’ from ‘non-glacier cells’ and (ii) illustrates the applied vector to raster conversion (cell center criterion. There would have been other possibilities for this conversion, i.e. that a grid cell is counted as glacier if only a small fraction is covered by the vector outlines, or if the majority (but not necessarily the center) of the cell is covered by the polygon).

P.4852 (Fig. 7): In the plots, the unit is missing for these values.

This measure has no unit since we divided the RMSE of the thickness differences (in meters) by the mean of the measurements (in meters as well). We will convert this number to %.

P.4853 (Fig. 8): Here and in your analyses: Include the regression line that you get by fitting the scaling parameters to this particular data set!

Is done and discussed extensively by Grinsted (2013). In the context of the present paper this does not make much sense, because we do not want to introduce new scaling parameters and least of all based on data that largely comes from other regions. The idea of this figure was to show the large scatter of measured mean ice-thicknesses for a given glacier area.

P. 4853 (Fig. 8): Plot area, not mean-ice-thickness.

?? Plot glacier area on both axes ??

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REVIEW #3

Italics: copied from the review
Normal font: Author reply

First, we would like to thank the reviewer 3 for the review, which will improve our article.

General comments

The study…

(1) displays a bias (the authors openly oppose the volume-area scaling while even their very own results do not provide evidence for it)

We would like to stress once again that we did not have a hidden agenda to criticize any of the approaches or similar. We regret that this impression came up and try to avoid this interpretation by rewriting the respective parts of the manuscript.

In our view our discussion and findings are based on material presented in the study. However, as mentioned in the general points at the beginning, we will redirect the aim of the study towards the focus of resulting ice volumes using different approaches, and rewrite the discussion related to V-A scaling (mainly section 5.2).

(2) fails to evaluate and present uncertainties of each method they use (and therefore strengthen their intercomparsion method)

Will be addressed in the revised manuscript, see replies to detailed comments below and also in the General Points in the beginning. However it is important to note, that – due to the different nature of the three approaches – it is not possible to consistently assess the uncertainties of the three methodologies in a way that allows a direct and quantitative comparison of the uncertainties.

Regarding the parameters for V-A scaling, for instance, the selection of parameters to be included in such an analysis and the uncertainty range of these parameters is a matter of subjectivity. As mentioned in the review, parameter combinations derived from Alaska, such as the ones suggested by Arendt et al. (2006), might not be suitable for this analysis (and won't be considered in the revised version). However, where to draw the line between suitable and unsuitable involves a considerable amount of subjectivity.

Furthermore, in the revised version, we will add supplementary material to explain the reasoning behind the parameters and parameter settings of the GlabTop2 model. Finally, the focus of the revised version will be redirected towards the main goal of obtaining a best estimate of the ice reserves stored in the HK glaciers. This will then finally allow giving a range within which the "true" volume of these glaciers are likely located.

(3) does not provide any new insight (the pros and cons of each method have been already discussed in more detail in the previous publications to which the authors refer)

To our knowledge there are very few (if at all) studies that make direct comparisons of results of different glacier volume estimation approaches. The combined application of the different approaches to produce an informed and transparent estimate of total ice volume for a critically important mountain region, in particular regarding the importance of the water resources stored in the glaciers, is, in our opinion, indeed providing new insights going beyond of what is already available in the literature. See also general comments of the review of David Bahr. In the revised version with the re-directed focus, the assessment of the glacier volumes will be more in the focus instead of the evaluation of pros and cons of the different approaches.

Detailed comments

P.4815: Also Clarke et al 2012, using inverse modeling.

Included.

P.4817: Please include the version of RGI (e.g. 2.0 or 3.0). The idea behind RGI is that it will be constantly updated as new and updated data become available. RGI 3.0 already differs from RGI 2.0. Also note that there is a paper by Pfeffer et al 'The Randolph Glacier Inventory: a globally complete inventory of glaciers' submitted to J Glaciol and close to be in press.

The (updated) RGI version will be included. (For the current version the statement is still valid…). Pfeffer et al. (in press) will be included.

P.4824: How do you make sure that the sensitivity tests among different methods are comparable?

This is exactly our point: Due to the different nature of the different approaches, it is not possible to set up a sensitivity test that can be applied to all approaches. It is therefore hardly possible to directly and quantitatively compare the results of the different sensitivity analyses. See also point (2) of the General Comments.

P.4824: for each individual glacier? [modification of glacier area by ±5%]

Yes (and hence also to the total glacier area). Will be stated explicitly to avoid confusion.

P.4825: what dictated this choice? [modification of f by ±0.1 and tau by ± 0.3]

The parameter values used here represent the limits of realistic estimations based on the literature (e.g. Patterson, 1994 and Paul and Linsbauer, 2012 for *f*, and Patterson, 1994 and Huss and Farinotti, 2012 for tau; see also Fig. 9). More details and references will be given in the revised version.

P.4826: Is this because Arendt et al used parameters best suited for Alaskan glaciers? After all, c=0.28, which is much larger (by 40%) than c in the other two V-A scaling methods.

In the revised version of the manuscript, Arendt et al. (2006) will not be considered anymore. This parameter set was included to allow for a comparison to the results obtained by Cogley (2011) for the same region (see also our General Points in the beginning).

P.4826: Am not sure if the % is best way to show the differences here. % for a small region means much less than % for a large (large ice volume). If large region is chopped into several small regions your % would have completely different meaning. Can you report the errors in SLE as well?

Regarding the total ice volume this is true. However, since here we want to show the relative difference we think % is the best way to do this. Doing so allows directly comparing the difference of the entire region to the difference of one of the sub-regions. This would not be possible if absolute differences (these are differences, not errors) were given (3'500 km^3 for the HK region and 2'341 km^3 for the Karakoram). The same applies of course for SLE.

P.4828: Radic & Hock 2010, or better to say Radic et al 2013 used parameter c fro Chen and Ohmura 1990. Note that your estimate with these parameters are the closest to the estimates from other methods you used. Also SLE estimates are not using those studies (Raper, Radi 2010) any more since those studies suffered from incomplete glacier inventories. See IPCC AR5 for the current studies used for SLE. Please do not mix tools (V-A scaling) with assessments of SLE, since it is much more in the assessment (e.g. data) than just the tool that can cause the uncertainties.

We will replace the references given here with the ones used in the IPCC AR5, which are Grinsted (2013), Huss and Farinotti (2012), Marzeion et al. (2012), and Radić et al. (2013). (All these studies are used here as well in the discussion, see P.4831 L.8-18 and Table 5). Regarding the difference of tools (V-A scaling) data (glacier inventories), the entire section 5.3 'Model parameter' is dedicated to this issue.

P.4828: I do not understand why you use Arendt et al as a representative for V-A scaling large bias. This is your upper band estimate since c is 40% larger than c in other methods. Please mention this somewhere in the text because it seems to me that the authors do not point out the obvious.

As mentioned above and in the General Points, Arendt et al. (2006) was used to allow for a comparison with the results from Cogley (2011) who applied this parameter set to the same study region. However, this will be removed in the revised version.

P.4830: so basically this result was already found and there is no novelty in this study at this point. ["It is thus likely that the global total potential contribution of glaciers and ice caps to sea-level rise might be smaller than previously assumed, as it was also found by Huss and Farinotti (2012) at the global scale."]

Well, finding that some existing assessments of the global total sea-level equivalent of glaciers and ice caps might be overestimated is definitely of interest. Our finding is based on the application of additional models and approaches than the one used by Huss and Farinotti (2012). Thus, the finding made by Huss and Farinotti (2012) is confirmed here and definitely worth mentioning.

P.4829: But isn't the same data used for each method in this study? Why don't you discuss in detail the results from your study rather than generality stating what we know from before (i.e. that the input data differs).

Yes, in this study the same input data is used for all approaches. But here we discuss the differences to previous estimates. We address the difference between tools and data as requested in this review, see three points above!

P.4829: This section is irrelevant for this study since the whole point in the study is to use the same data (inventory) and apply different tools.

Yes, we used the same inventory for the approaches presented in the methods and results section. But in our view it is an essential component of a scientific work to make links and comparisons to previous studies and discuss new results in the context of the existing literature.

P.4829: except for evaluation of statistical methods which proved to be useful. ["… extrapolations of glacier size classes from one region to another are not required anymore"]

Maybe we did not correctly understand this comment. How can it be more useful to use size class extrapolations from another region instead of a glacier inventory of the region? We do not see how to include this comment in our revised version.

P.4830: Why is only conceptual aspect of V-A scaling discussed here? All other methods are also based on concepts.

Agreed, this section will be removed, see also General Points.

P.4830: Isn't the volume-area relation what you are explaining here, not ice thickness and area relation?

See reply to Review#1

P.4830: This argument is flawed (see e.g Lüthi et al 2008). Also, rather than trying to reinvent the wheel here (i.e. finding theoretical basis for V-A scaling in order to identify misconceptions) why not properly read Bahr et al (1998) who correctly points out the pros and cons of the scaling in the conceptual light (e.g. each closure in the scaling analysis is discussed).

We discussed this issue in the response to David Bahr's review. We recognize the ongoing debate. For instance, in contrast to the references given in this comment, Cogley (2012) says "*volume-area scaling is glaciological jargon for the observed tendency of glacier volume to be proportional to glacier area. It is a misnomer, because the independent quantity that is found to be proportional to area is mean thickness […]. Objections to the statistical propriety of correlating area with volume, because volume is the product of mean thickness and area, are therefore without merit*".

As stated above and in the General Points, this section will be removed and due to the points provided above, we have no intention to enter this debate.

P.4820: Scaling parameters are also determined theoretically (using scaling analysis of mass continuity equation and ice rheology). Please read publications of Bahr from late 1990ies.

According to the theory, yes. Nevertheless there exists a large variety of scaling parameters, both for the scaling factor and the exponent

P.4820: Wrong for gamma. Gamma is well constrained parameter (only differs between glacier and ice cap). Please read Bahr papers.

Agreed, but this applies only to glaciers (and ice caps) in steady-state conditions, (see e.g. Adhikari and Marshall, 2012; Farinotti and Huss, 2013), which is rarely the case. Furthermore, Grinsted (2013), showed that by fitting scaling relationships to measured data values for gamma well differing from the theoretical numbers derived by Bahr et al. (1997) can be found. However, it is definitely beyond the scope of this paper to investigate the exponent of V-A-scaling.

P.4820: It would make sense that the authors performed this sensitivity test (e.g. delineating the glacier complexes and not delineating, and delineating in different ways) to quantify the uncertainty of this factor. Especially since the effect can be large. This sensitivity test would make one valuable contribution to this study that so far has not much new to offer.

We assume the reviewer confuses 'delineating glacier complexes' with 'separating glacier complexes into individual glaciers', i.e. digitizing drainage divides.

As mentioned in the reply to Review#2, it is hardly possible to estimate the accuracy of a drainage divide algorithm due to the lack of a reference (cf. Kienholz et al., 2013), which would be required for assessing the sensitivity of this aspect. Furthermore the review is inconsistent in this point: Before it says several times that certain cited facts are not novel or do not add any novelty, but here it suggests repeating an analysis already done and shown in another study. Furthermore, the impact of glacier delineations only affects the results obtained by V-A scaling, but has only a minor influence on the results of the ice thickness distribution models.

P.4832: It would be worth providing the full range of estimates from V-A scaling (as you start doing it here), instead on focusing rigidly on the selected three methods one of which is an outlier (Arendt et a) for this region. There is no reason to stick to the outlier just because initially you thought it would be good to use it. But not only do you stick with the outlier (which I can understand) but you use the outlier to bring your main conclusions about the V-A scaling overestimated results. So the study is biased and lacks scientific objectivity.

Yes, therefore Arendt et al. (2006) will not be considered anymore in the revised version.

P.4832: Do these scaling approaches, in addition to different data sets and glacier delineation method, differ only in the parameters c and gamma? If yes, it would be useful to specify the scaling parameters for each estimate (add in Table 2). It seems that all what you are restating here is that the results will depend on the choice of c and gamma. Again, this is not new. It would be much better to provide systematic sensitivity tests for each method you use and provide a full range of results (and present them in Figure 5).

Agreed, the scaling parameters of these three additional studies will be added specified the revised version. In this new version, we will come up with uncertainty estimates for each of the three approaches. However, due to the fundamentally different nature of the methods, it will not be possible to set up sensitivity tests that allow direct quantitative comparisons. For example the issue of glacier separations has a large impact on V-A scaling, but does not influence results from GlabTop2 and the HF model. Nevertheless, these estimates of uncertainty will be included in Fig. 5 as suggested.

P.4832: Which should have been quantified. Again provide the uncertainty range for each method.

Will be done, see above.

P.4833: This point is not logical: You can not do 'by average' where 1 out of the sample of 3 is an outlier. The conclusion displays bias. Also, considering the emphasis on the importance of better estimates of SLE one would expect more effort from the authors in performing the uncertainty analysis, displaying its results and discussing their results in this light. It became obvious that the authors had some other agenda (e.g. blacking the V-A scaling by using flawed arguments) rather than submitting results of a scientific study.

As already mentioned, Arendt et al. (2006) is not considered anymore in the revised version, and numbers of the differences will be revised accordingly. Also an uncertainty estimate for each of the approaches will be included. We stress once again that our main objective is to come up with an estimate of the volume of HK glaciers using a variety of approaches, rather than repeating uncertainty analyses that have been done already in other studies. And we regret that the impression came up that we had a hidden agenda of blacking V-A scaling, which was not the case.

P.4833: The authors did not show any effort to propagate the errors from these individual assessment into regional estimates. This should have been done to arrive at a bulk uncertainty in these methods, in addition to sensitivity tests (whose results are also ignored in the conclusion). Again, this shows a bias in the presentation of the results.

We do not exactly understand what is meant with "[propagating] the errors from these individual assessment into regional estimates". However, in the revised version uncertainty estimates for all approaches will be included.

P.4833: How is this 100% deviation assessed?

This will be explained more clearly in the revised version. The number comes from the comparison of the lowest volume estimation compared to the highest volume estimation for both the entire HK region and some of the sub-regions, see P.4826, L.21-22. As mention in the reply to Review #2, this formulation is reworded to "results deviate by a factor of two".

P.4833: It would be nice that this paper reveals some insight from an error analysis of the discussed methods, and propose a way for future progress(e.g. what kind of measurements are needed and on what kind of glaciers, on how many glaciers etc) and what kind of measurements would help improve the parameterizations in the 'advanced' methods. We all know that more volume measurements are needed so this is not news. So, not only that the study displays bias (lacks scientific objectivity), under-performs in terms of uncertainty analysis (having all the data it needs!) but also lacks novelty.

The points mentioned in the first part of the comment will be considered in the revised version. Also we will include uncertainty estimates for each of the used methods, which, however, will probably not allow for a direct quantitative comparison. We are convinced that we will get the best possible estimate of ice volumes of HK glaciers based on the available data and that our finding add significant new knowledge to this field and region.

P.4842, Table 2: c is not unitless. Please include units.

Done.

P.4847, Fig. 2: What does different color shading refer to?

As stated in the figure caption, the colors represent the different size classes.

P.4850, Fig. 5: Would be good to add the uncertainty range for each bar, and to assess statistical significance of the differences in volume estimates among the methods.

Yes, uncertainty ranges for each bar will be added in the revised version.

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