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Interactive comment on "Density assumptions for converting geodetic glacier volume change to mass change" *by* M. Huss

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The review by G. Moholdt was very helpful to finalize the paper. It comments on several important points that needed clarification.
My responses to the review (in *italic*) are given below, including proposed changes to the text of the paper (in quotation marks).

... In most cases, however, it is slightly lower than the density of ice since parts of the gained/lost volume is in the form of low-density firn. To account for this in general terms, the author comes up with a somewhat magical recommendation of a conversion factor of 850±60 kg m-3. Like another interactive comment points out, this value needs to be

better justified, both in terms of how it is derived and when it can/cannot be applied.

Based on the findings in the paper and other relevant studies, I would restrict the usage to the following conditions:
1. A time span of minimum 5-10 years
2. A considerable firn area is still present
3. A stable mass balance gradient

Additional discussion is provided on how the value of 850±60 kg m^{-3} was obtained. The suggestion to recommend this mean value only for the three above cases is very good and is included in the revised manuscript.

"Although all experiments for the synthetic glaciers indicate a significant dependence of $f_{\triangle V}$ on the period considered (Fig. 4), a general statement about a representative value of the volume-mass conversion factor and the related uncertainties is possible with some restrictions. By averaging calculated $f_{\triangle V}$ for time intervals of 5-50 years (typical for geodetic mass balance determination) and the Experiments I, II and IV, a mean value of $f_{\triangle V} = 850\,kg\,m^{-3}$ is obtained. Based on a combined assessment of the effect of glacier size and differences in $f_{\triangle V}$ for short and multi-decadal periods, an uncertainty range of ±60 kg m^{-3} is assigned. In the case of (i) periods shorter than 5 years, (ii) significant changes in the mass balance gradients (e.g. Exp. III), (iii) small overall volume changes, or (iv) an insignificant firn area, this average conversion factor might however not be applicable and $f_{\triangle V}$ can be beyond the specified uncertainty ranges. If the above caveats are accounted for, $850 \pm 60\,kg\,m^{-3}$ is recommended for converting volume change to mass change for studies that do not perform an in depth analysis of changes in firn volume and density."

Restriction 2 will likely be more relevant in the future as the firn pack diminishes from some mountain ranges. Restriction 3 is less obvious, but is particularly important because the conversion factor becomes higher than the density of ice (Experiment

III). Enhanced melt in the ablation zone combined with increased precipitation in the accumulation zone is one of the expected footprints of climate change for glaciers that cover a large elevation range, e.g. Patagonia, New Zealand, Alaska and the Arctic. For example, the application of zonal densities in Moholdt et al. 2010 (used here in Fig. 5b) resulted in a theoretical volume-to-mass conversion factor of 1.0 for the Svalbard glacier region as a whole. Another study from the Canadian Arctic (Gardner et al., 2011) found that 85% of the volume change occurred below the firn line which implies that the conversion factor must be close to the density of ice unless there are unknown changes in glacier dynamics or firn densification that come into play. The latter is certainly a possibility given the increased refreezing and rapid firn warming that has been observed in the southern part of the region (Zdanowicz et al., 2012). Such climate-driven changes in the firn densification regime are however not included in this model either. It is only forced by changes in surface mass balance conditions despite the obvious relation with climate. This limitation of the model needs to be pointed out and discussed in the paper.

The firn densification model has not a sufficient physical basis to accurately simulate climate-driven impacts on firn densification. The model is simplified and illustrates the general effects of firn volume and density variations on the conversion factor between volume change and mass change. These limitations of the model are discussed in the revised manuscript.

"However, the simplified setting of the experiments cannot account for all influential processes present in nature. For example, densification due to refreezing is only modelled crudely although major changes in the firn density profile of polythermal glaciers due to this process have been reported related to ongoing climate change (Zdanowicz et al., 2012)."

The uncertainty of the recommended conversion factor is set to a fixed value of 60 kg m^{-3} although the experiments show that it is dependent on the applied mass balance forcing and particularly the length of the observation period. A bigger problem is that the implied mass balance uncertainty becomes unrealistically low when the measured volume change is small. This contradicts Eq. 4 which shows that a mass change may occur even if there is no change in volume. Is it possible based on your data or model to come up with a minimum area-averaged uncertainty for densification processes in a geodetically derived mass balance? For example, in a recent mass balance study of the Russian Arctic (Moholdt et al., 2012), the uncertainty of the volume-to mass conversion was set to the greatest of ±10% of the volume change and a constant of 0.5 Gt a^{-1}. These choices were rather arbitrary but underline the point that an additional conversion uncertainty needs to be included when the measured volume change is small. A data/model-based recommendation on this issue would be very helpful for the community.

The results of my study show that the conversion factor $f_{\Delta V}$ can be in the range $[-\infty, \infty]$ for volume changes that are close to zero; the relative mass change uncertainty can thus be very high. For larger volume changes, the relative uncertainty is much smaller, but the absolute uncertainty increases. The suggestion to specify a lower bound for the absolute uncertainty to address this issue is interesting but I am not sure that I can provide a well founded answer to this question based on my data/model: If the volume change is close to zero, the mass change will mainly depend on the accumulation / firn densification history of the last years: With no changes in the firn density profile, the uncertainty is small. With changes in the accumulation rate etc., significant mass changes can however occur despite a constant glacier volume. Specifying an absolute lower uncertainty bound would thus require knowledge about temporal firn evolution that varies from site to site.

Anyway, an attempt going into the direction of the reviewer's thought was made based on the data from the mass balance monitoring series of Gries- and Silvrettagletscher (Fig. 5). By calculating absolute uncertainties in mass change (due to a variable value of $f_{\triangle V}$), a first order estimate of an absolute lower bound uncertainty (similar as in Moholdt et al., 2012) can be provided. This is included in the paper.

”Evaluation of the complete series of annual conversion factors for both glaciers (n=92) shows that $f_{\triangle V}$ ranges between minimum/maximum values of -500 and 6500 kg m^{-3} for annual mass balances B_{a} of -0.2 to $+0.2$ m w.e. a^{-1} indicating a large relative uncertainty in the estimation of $f_{\triangle V}$ for small mass changes. The spread in $f_{\triangle V}$ rapidly reduces with increasing magnitude of mass change being 790 ± 75 kg m^{-3} for $\mid B_{\text{a}} \mid >1$ m w.e. a^{-1}. By multiplying the deviation of annually evaluated $f_{\triangle V}$ from a reference value of 850 kg m^{-3} with that year's mass balance, a maximum accuracy for the determination of mass balance can be estimated. It is found that for Gries- and Silvrettagletscher B_{a} is subject to an uncertainty greater than at least ± 0.05 m w.e. a^{-1} due to the variability in $f_{\triangle V}$.”

P220, L1-L5: What is ”the geodetic method”? Gravimetry is also a geodetic method, for example. Also, discontinuous elevation measurements from airborne or spaceborne lidar profiling have become popular for regional mass balance assessments (e.g. Alaska and the High Arctic) and have the same issue with volume-to-mass conversion. A phrase like ”...volume change derived from repeated elevation measurements...” would be more clear and general. This also applies to other parts of the text.

Reformulated throughout the paper.

P220, L5: It is not totally obvious that ”this conversion factor” refers to ”a density assumption”. Future studies might also use firn pack observations/modelling rather than simplistic conversion factors. I therefore suggest a rewrite to something like: ”... a density assumption or model. This study investigates the use of a constant density factor for the volume-to-mass conversion based on...”.

Reformulated.

P221, L26: mention that this number derives from the density of ice

”If Sorge's law holds, $\Delta\rho$ (Eq. 3) equals zero, and $f_{\triangle V}$ is about 900 kg m^{-3}. This number derives from the density of ice and has been adopted in many previous assessments...”

P222, L25: which kind of ”direct measurements” are you talking about here? Snow pit measurements, gravimetry, or..?

Sentence shortened and simplified.

P222, L29: The explanation of the direct glaciological method is unclear. A methodological reference would be good for the uninformed reader. This also relates to the previous comment.

Reformulated and reference added.

”Annual glacier mass change is normally estimated with the direct glaciological method (Kaser et al., 2003) by integrating surface-density corrected point measurements over the glacier area.”

P223, L5: Ground-based gravimetry has the potential to resolve small-scale glacier mass changes although not commonly used, e.g. Breili and Rolstad (2009)

Interesting reference – thanks. Added with a short description.

"Ground-based gravimetry has also been applied for detecting local mass changes (Breili and Rolstad, 2009). Application of this method for the determination of the overall glacier mass budget is however not feasible.."

P223, L10: This is not entirely true anymore, see Bolch et al. (2013) who used a firn densification model to account for density changes in the assessment of geodetic mass balance for peripheral glaciers around the Greenland Ice Sheet.

Bolch et al. (2013) is now cited in my paper. However, also with this new study, the issue of evaluating surface elevation changes in the ablation and the accumulation area separately and thus not considering the important influence of ice flow on dH/dt remains (as e.g. in Moholdt et al., 2010, Kääb et al., 2012).
The sentence is reformulated.

"To date, detailed studies for mountain glaciers that connect geodetically measured variations in overall ice volume to their mass balance by taking into account changes in firn volume and density at a spatially distributed scale are not available."

P224, L3: How exactly is the elevation range (size) varied?

Reformulated.
Individual model runs for synthetic glaciers with elevation ranges between 300 and 2000 m (in 100 m steps) are performed.

P224, L12-13: If systematic differences between the geodetic and glaciological mass balance were corrected, then the two data sets are dependent, and I don't understand how you can do the comparison in Fig. 5b? This needs more explanation somewhere.

Determining $f_{\Delta V}$ from a direct comparison of glaciological mass balance and volume change would be impossible due to the circularity of the problem and the measurement uncertainties.

As stated on P227, L17-24 the surface mass balance forcing is used to drive the firn compaction model. Observed volume changes of the glaciers do not enter the calculations. The correction of surface mass balance described on P224, L12-13 (accomplished in another study) warrants that the mass balance forcing is realistic which is important for a correct calculation of the firn density changes. The new manuscript now only states that the mass balance data set was homogenized and does not mention the calibration with volume changes in order not cause confusion.

"Eight (Gries) and six (Silvretta) DEMs are available over the last five decades, documenting changes in glacier area and volume (Bauder et al., 2007). Both mass balance series were homogenized by Huss et al. (2009)."

The procedure to calculate volume changes from observed mass balance forcing is reformulated and clarified in chapter 3.2.

"$f_{\Delta V}$ was also determined by driving the firn compaction model with observed mass balance for periods of between 4 and 14 yr corresponding to the dates of available DEMs. This allows validating assumptions on the volume-mass conversion factor made in previous evaluations of the time series (Huss et al., 2009), and illustrates characteristic values of $f_{\Delta V}$ in applied mass balance monitoring of mountain glaciers. "

P225, L9-10: What is the background for these density values - any references or measurements?

See also response to Reviewer #1.
The assumption of $\rho_{\text{ice}} = 900\,\text{kg}\,\text{m}^{-3}$ for mountain glaciers can be found in dozens of articles but it is difficult to detect a primary reference. The value of initial firn density is now backed up with measurements compiled from 19 firn cores in different mountain ranges.

P225, L19: Is this temperature profile representative for the selected set of firn density profiles? I assume several of them have temperature measurements as well. This could explain some of the mismatch between the observed and modelled density profiles. A good reference with multiple depth profiles of firn density and temperature is Zdanowich et al. (2012). Their data show that the firn density profile of the Penney Ice Cap on Baffin Island changed relatively little (0.9 m w.e.) over the last 15 years despite an impressive firn warming of about 10 deg C (Figs. 5 and 7). How do their findings relate to your model results? Nuth et al. (2010) also show examples of multi-temporal firn density profiles from Svalbard with relatively small changes over decadal time spans.

Both studies on changes in firn density are now referenced and are discussed. The firn density profiles shown in Nuth et al. (2010) and Zdanowicz et al. (2012) are included in the validation data set of firn cores (see additional table).

The schematic temperature profile described on P225, L19 cannot be directly compared to the temperature measurements (and the changes over time) in cold firn. As my model does not include an explicit temperature forcing (time series), it is not able to simulate the transient warming of the firn in response to climate change. The revised paper provides a better explanation of the assumed temperature profile (supported by references) used to estimate the amount of refreezing.

"In order to keep the model simple (not requiring climate data input) and location-independent, firn densification due to refreezing $R(t)$ (Eq. 5) is roughly approximated by assuming an end-of-winter firn temperature profile that linearly increases from $-5\,^{\circ}$C at the surface to 0 °C at a depth of 5 m. This profile corresponds to the typical penetration depth of winter air temperatures (e.g., Hooke et al., 1983; Greuell and Oerlemans, 1989) and defines a negative heat reservoir for refreezing melt water. For each firn layer, r is obtained by prescribing complete latent heat exchange. Total refreezing $R(t)$ after t yr is calculated as $R(t) = R(t-1) + r$."

P226, L8: What is the average annual accumulation for this reference firn profile? See also the other reviewer's comment about the climatic context of the firn profiles.

A table stating the characteristics of the firn density profiles has been added (see response to Reviewer #1).

P226, L20: At which elevation is the reference ELA set? At the elevation of 50% accumulation-area ratio? If so, is this realistic for the typical mountain glaciers of today *which are out of equilibrium with present climate? The experiment setup is fine, but this point is important for the discussion and implications.*

There is no need of specifying the elevation of the ELA or an accumulation area ratio. It is prescribed that the overall glacier mass balance is zero. With the definition of mass balance gradients in the ablation and the accumulation area, the ELA and AAR that yield a balanced mass budget can directly be calculated, corresponding to the glacier surface geometry. As smaller mass balance gradients in the accumulation area are assumed, the AAR is around 60%.
Clarified in the revised manuscript.

"Before applying a change in mass balance, the model is run for a 50-yr spin-up phase with the ELA that yields a balanced mass budget."

P227, L1: The description of Experiment III is somewhat unclear. Isn't it just a 50% increase/decrease of the mass balance gradients? Or is the ELA also shifted like the legend and caption of Fig. 4 suggest?

Clarified in the revised text.
Also the ELA is shifted slightly in Experiment III. It is intended to generate a small (positive/negative) change in mass balance (i.e. a volume change) after the spin-up phase. If only changing the mass balance gradients but not the ELA, mass changes would be minor and the calculated $f_{\Delta V}$ would go towards ∞, and thus be difficult to interpret/visualize.

"Experiment III: the step changes in mass balance are limited but the spatial distribution is changed.

Whereas the first scenario assumes a 50 % increase in mass balance gradients both in the ablation and the accumulation area, the second scenario is characterized by reduced gradients. Both scenarios also include a small positive/negative shift in ELA."

P227, L19: The volume change must include the ablation area as well, or? The current description of how volume change is obtained from the model is unclear.

Of course. This is implied by the subsequent sentence which is now formulated more clearly.

" For the ablation area, ice density is assumed to be constant at ρ_{ice}=900 kg m^{-3} for calculating volume changes."

P228, L13: Isn't this the case for all experiments except number III? Or are you just talking about experiment I in the rest of this paragraph? If so, make it clear.

Actually, the first general statements of this paragraph are valid for all experiments (also for Exp. III, $f_{\Delta V}$ converges to 900 kg m^{-3} after a sufficiently long time). The sentence is slightly reformulated.

"After a shift in ELA, $f_{\Delta V}$ converges towards 900 kg m^{-3} after some decades."

P228, L25-28: True in a sense, but in this case the volume change is extremely small, so the absolute error of the mass balance would be small for any conversion factor.

Agreed. This point is added.

> "This indicates that assuming validity of Sorge's law for estimating the factor to convert volume change to mass change is not even feasible for a small shift in mass balance forcing although the absolute error in calculated mass change would be limited for any conversion factor."

P229, L20: Shouldn't it be from minus infinity to infinity? That makes the next sentence excessive.

Yes. Done.

P230, L25-27: Are these numbers from Fig. 5? I don't see where they come from.

Thanks for this remark. See explanation on how it was addressed in the response to Reviewer #1.

P232-P323, the Discussion: The first and last paragraphs are good, but the rest is dominated by methodological descriptions of sensitivity tests that fit better earlier in the manuscript, possibly as a sub-section 2.4 Sensitivity Tests. The results and implications of the sensitivity tests can still be discussed here. Alternatively, the results and discussion can be merged with one section about sensitivity tests.

Although I basically agree with this comment, I would like to have the methodological description of the sensitivity tests together with the results. The set-up of the tests can be described very briefly; only 1-2 sentences are required. When splitting test

set-up and results, the sensitivity experiment would need to be re-introduced in the Results/Discussion section which is more difficult to understand for the reader and causes repetitions. Merging the Discussion with the Results does not seem to feasible as well to me. The Results have two very clear topics ($f_{\triangle V}$ from experiments, $f_{\triangle V}$ from real data) and adding a sensitivity discussion would rather dilute the statements. For these reasons, I would like to keep the structure of the Discussion section as it is now.

P233, L6-7: This concerns the different steepness of a glacier with constant width. In reality, glaciers are often more narrow towards the tongue (e.g. Fig. 1) which is also a form of area-elevation distribution. The conclusion can therefore be misleading unless you specify that or also test this type of different area-elevation distributions.

This point was also challenged by C. Nuth's Interactive Comment. The revised manuscript now includes an additional sensitivity test for which the shape of the glacier, i.e. its width and area-elevation distribution is varied, and effects on calculated $f_{\triangle V}$ are discussed. The impact on the results is relatively small.

> "In a first experiment, additional synthetic glaciers were defined for analyzing the dependence of $f_{\triangle V}$ on glacier geometry: (1) the glacier with a constant width has a slope of 8° below half of its elevation range, and is steep (35°) in its upper reaches; (2) it is inclined by 35° in the ablation area, and 8° in the accumulation area; and (3) the width of the accumulation area is increased by a factor of 5 relative to the ablation area (with a constant slope of 15°). The area-elevation distribution of the glacier has a small influence on $f_{\triangle V}$. Slightly higher average values were found for a glacier exhibiting a steep firn zone ($+1$ kg m^{-3}, excluding Experiment III), and lower $f_{\triangle V}$ are present for the glacier geometry with a steep ablation area (-18 kg m^{-3}). Increasing the width of the accumulation area yields a difference in average $f_{\triangle V}$ of -19 kg m^{-3}."

P235, L10: The meaning of "most cases" must be specified since some people might just take this value as a universal truth. See also the general comment about this.

Done.

"$f_{\Delta V} = 850 \pm 60$ kg m^{-3} is recommended in the case of periods longer than 5 yr, stable mass balance gradients, the presence of a firn area, and volume changes significantly different from zero."

Fig. 4: According to the color legend all experiments involve an increase or decrease of the ELA, but is that really the case for Experiment III? If so, the methodology is unclear.

See also my comment on this subject above. For clarification a colour legend for Exp. III is added in panel (c) of Fig. 4 specifically stating the changes in the mass balance gradients.

Fig. 5: Why are the mass balance units different in (a-b) and (c-d)? Also, the data sets and calculations behind this plot needs to be explained in more detail, preferably in the main text. See also a previous comment about this.

Figures 5a/b show absolute mass/volume changes, the unit being mass/volume. Scales are thus different for the two glaciers that do not have the same area. Figures c/d show mean specific mass balance (i.e. mass change divided by glacier area) which allows a direct comparison of the two glaciers. This is important as Gries showed

more negative mass balances than Silvretta ($f_{\Delta V}$ thus faster approaches ice density).
For clarification the axis title of c/d is changed.
The description of the calculations underlying this figure are described in more detail (see previous comments on this subject).

Stylistic comments are included in the manuscript as proposed by the reviewer.

Interactive comment on The Cryosphere Discuss., 7, 219, 2013.