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Discussions

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

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Repeated survey of glacier elevation is typically the easiest and most accurate way to determine glacier mass balance over decadal time spans. Most studies assume a constant density factor to convert the measured volume changes into mass changes, often without considering the uncertainty of this factor and the unknown contribution from firn densification. Given the popularity of this method, it is surprising that no one has gone into depth (literally!) about the issue of volume-to-mass conversion for mountain glaciers. The study of Matthias Huss is therefore a very welcome contribution to the field and will likely be a frequently cited paper in future mass balance assessments.

The paper has a good methodological setup and is well written. It uses the empirical firn densification model of Herron and Langway (1980) to investigate the volume-to-

mass relation for a range of simplistic mass balance scenarios applied to a set of idealized and real glacier geometries. The model is optimized to match with typical firn density profiles for a selection of glaciers around the world. The calibrated model might not capture the physical processes correctly (see the more detailed comments from the other reviewer), but the performed sensitivity tests indicate that the results are still relatively robust.

The four model experiments (Figs. 3-4) show some interesting results. The "volume-to-mass conversion factor" can be both higher and lower than the density of ice due to undetected changes in average glacier density (Eq. 4). 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 $\pm$ 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

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 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.

All in all I am pleased with the paper and hope to see it published in the Cryosphere after appropriate revisions. I also have some specific comments and suggestions that follow below according to the page and line numbers in the manuscript:

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.

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...".

P220, L18: most popular and accurate

P221, L5: and -> but

P221, L6: simpler and clearer: "...varies from case to case, and..."

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

P222, L9: Kääb et al.

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

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.

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]

P223, L10: This is not be 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.

P223, L22: "…time series of surface mass balance for…"

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

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.

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

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 10ËŽC (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.

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.

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.

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?

P227, L4: "a +100/-100 m shift in ELA (similar to…"

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.

P228, L10: dominate over

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.

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.

P229, L20: Shouldn't it be from minus infinity to infinity [$-\infty,\infty$]? That makes the next sentence excessive.

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

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.

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.

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.

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.

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.

References

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