The Cryosphere Discuss., 7, C3411–C3422, 2014
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The Cryosphere Discussions

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Interactive comment on "Mass change of Arctic ice caps and glaciers: implications of regionalizing elevation changes" *by* J. Nilsson et al.

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Received and published: 12 March 2014

Mountain glaciers and ice caps have recently contributed as much to sea level rise as the ice sheets [Gardner et al., 2013]. Most of this glacier mass loss has occurred in the Arctic, and this paper presents a consistent analysis of elevation changes (dh/dt) from ICESat laser altimetry over all major glacier regions in the Arctic except Greenland. The aim of the paper is to determine the optimum technique for interpolating/extrapolating altimetry-derived elevation changes to unmeasured glacier areas outside the tracks. This is likely the largest uncertainty of altimetry-based estimates of regional glacier mass budget, and the study therefore makes a welcome contribution to the field.

Gridding of elevation-change measurements from altimetry is something that has been frequently done over the ice sheets [Hurkmans et al., 2012], but not typically over mountain glaciers and ice caps due to limited track coverage and complex glacier morphology. The gridding can potentially serve two purposes: (1) improved visualization of spatial patterns of elevation change, and (2) improved estimation of regional volume change and mass budget. This paper tries to achieve both for entire glacier regions, whereas earlier studies have only focused on the latter using zonal averaging (by altitude and/or sub-regions) rather than gridding. Unfortunately, the paper falls between the two chairs and in the end I am not sure what message to take home.

The paper tests four different methods (M1-M4) of spatial interpolation/extrapolation and evaluates them regionally with respect to the mean of the original elevation-change distribution. Hence, the ideal method in the authors' eyes is one that reproduces the histogram of the original along-track elevation changes. In that case, I wonder what the purpose of the regionalization is in the first place - why not just use the raw mean dh/dt? In my eyes, spatial inter-/extrapolation techniques are ways of compensating for non-random spatial sampling due to effects such as few satellite tracks (e.g. Iceland, Alaska, Southern Canada), sub-regional or altitudinal variations in climate and glacier morphology (e.g. Svalbard, Alaska and Russia), and also the general track convergence towards northern latitudes (for large regions). Unfortunately, all techniques with such capabilities would get a poorer score with respect to the optimum case of an unchanged dh/dt histogram. In current form, the criterion is more a way of minimizing the risk of gross errors due to inter-/extrapolation than actually improving on the regionalization itself. I urge the authors to comment on these issues, make a clearer justification for their optimization strategy (Section 4.2) and potentially revise the criterion.

I do not have a straightforward answer for an improved optimum criterion, but one idea could be to sub-sample some of the well-covered regions and compare the results with the full distribution of dh/dt measurements. This is analogous to what has already been done to assess the applicability of spatially-limited airborne altimetry for determining

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regional volume changes in Alaska [Arendt et al., 2006; Berthier et al., 2010; Johnson et al., 2013] and Canada [Gardner et al., 2012]. In these cases, it is obvious that a simple mean dh/dt is not an optimum criterion because the airborne profiles are often selected to follow the centerlines of the largest glaciers. I think the paper would benefit from a more general discussion about these aspects of spatial sampling and how they relate to your methods (M1-M4) and the optimum criterion.

The most interesting part of the paper is the application of two spatial interpolation techniques (M1-M2) for generating continuous fields of elevation change from very limited samples of satellite ground tracks. It is however difficult to judge how well these methods perform because the regional figures are very small (Fig. 4) and show the result of one method only, the so-called optimum one which varies from case to case. Maybe one figure with all four methods for a few regions would illustrate this better. There is also very little discussion of how realistic the gridded fields actually are, for example over a glaciologically complex region like Alaska. Do they serve any purpose at all? This could be tested for a few regions using external validation data or internal sub-setting to see if there is an acceptable correlation.

There is a tendency in Table 2 and 3 that all regionalization methods (M1-M4), especially the hypsometric ones (M3-M4), produce more negative area-averaged elevation changes than the observational mean dh/dt ("the optimum"). This is the background for the main claim of the paper - that current regional mass losses from ICESat "might be overestimated by as much as 4-19%". But how do know that those are "overestimates" and not actually your best estimates of the real changes? For example, if the rapid thinning at low elevations (Fig. 3) was undersampled, then the observational mean dh/dt would be positively biased. To keep their conclusion, the authors need to show that there are no significant spatial sampling biases (e.g. by plotting the ICESat sampling against glacier area as a function of lat/lon/h) and that the 1km resolution DEMs are of sufficient quality to give realistic hypsometries. The latter can be checked against actual ICESat elevations and high-resolution DEMs like ASTER GDEM. I would also strongly encourage to do all methodological comparisons before applying the density scheme because it has a variable impact on the different methodologies.

Besides my main concerns above, I have some smaller comments and suggestions that I have put in chronological order below:

Abstract: The abstract counts about 400 words which is at least twice as long as the editorial recommendations. Especially the beginning contains a lot of redundant material.

Introduction: Could also be shorter and more focused.

P5891, L7: There is now a newer IPCC report! Normal citations should be in parenthesis.

P5891, L10. Measurements are not derived from records...

P5891, L21: Considering that elevation changes are typically much larger over glaciers than ice sheets, I do not see that glacier applications are "inherently more difficult".

P5892, L9-12: I disagree. Several of these studies have used multiple regionalization approaches, including mean, median, polynomial fits, hypsometry from two DEMs and different sub-regional summations. Arendt et al. [2006] and Nuth et al. [2010] are also two good references where regionalization methods are carefully discussed. The novelty of this study is mainly the use of surface fitting and spatial interpolation (M1-M2) which has not been previously explored in these regions, partly justified by limited data coverage in combination with glacier complexity.

P5893: It is okay to exclude the Greenland periphery, but not on the reasoning that it has "been studied in more detail in other studies" as it is probably the least studied glacier region that only recently got a complete glacier inventory.

P5893, L12: The Randolph inventory has soon a new reference [Pfeffer et al., 2014].

P5893, L16: What are the data sources for these DEMs and how well do they compare with ICESat? Do these 1 km DEMs produce consistent hypsometries with higher resolution DEMs like ASTER GDEM? See e.g. Gardner et al. [2011] and Nuth et al. [2013] for relevant usage of the latter global DEM.

P5894, L3: Which ”quality flags and rejection parameters”?

P5894, L9: What is “large separation”? 100 m or 1 km?

P5894, L10: The description of the elevation change calculation needs to come here, not in Section 4.1 which is about regionalization and volume/mass change estimation.

P5894, L13: Could this potentially also remove large elevation changes that are real? For example, it seems like some of the rapid changes on the Academy of Sciences Ice Cap [Moholdt et al., 2012] and Bering Glacier [Arendt et al., 2008] have been filtered out.

P5894, L17: What is the “local window“?

P5895, L3-7: Move to Section 3. The requirement of a 6-year data record frequently fails along repeat-tracks in cloudy regions like Svalbard, Iceland and Alaska. Could a lower limit (e.g. 4 years) help to improve the data coverage and hence also the regionalization? In that case, there would maybe not be sufficient data to estimate a seasonal signal, which is a drawback of including so many parameters in the regression.

P5895, L9: What is the corresponding planimetric resolution? Sufficient to represent small glaciers accurately?

P5895, L13-23: It is very difficult for a glacier to grow if positive elevation changes are given a density of 500 kg/m3 and negative changes a density of 900 kg/m3. This implies that random noise in the measurements transfers into a negative mass budget even for glaciers in equilibrium. At least, elevation changes must be averaged over large areas before such a density scheme becomes realistic. Given that large-scale thickening has only occurred on a few ice caps/fields, I think it would be easier/better to stick with a constant density everywhere.

P5896, L25-26: Is this really all the description for method 1? More details are needed about the characteristics of the surface and how it is fitted.

P5896, L10: How does this denser 100 m resampling improve the interpolation (not considering the smoothing effect of Eq. 2)? The cross-track spacing is still several km.

P5896, Eq. 2: Is this done on a track-by-track basis or for larger areas across the tracks? What model order (N) was typically used?

P5897, L5: is -> are

P5896, L26: Outliers are already removed, so why use the mean as you do elsewhere? Figure 2 shows that the dh/dt histograms are negatively skewed which means that a median estimator will be more positive than the true area-averaged dh/dt. See supplementary table 5 of Gardner et al. [2011] for an example of this effect.

P5898, L3-14: This concerns method 3, so it fits better one paragraph higher.

P5899, L2-4: The track convergence makes a denser sampling towards the north, hence an uneven spatial sampling which is opposite of the argument here.

P5899, L12: Assuming an error of 1 m is okay, but I do not understand if/how this quantity enters the RSS summation mentioned in Table 1. The other quantities in the table are in unit m/y and hence cannot be directly combined with an absolute error in meters.

P5899, L13: Has now been quantified for R33 in at least two paper, but the values are not coherent, so any corrections are debatable. Also note the Gaussian-Centroid offsets [Borsa et al., 2013] which were an implicit part of the inter-campaign biases and can now be corrected directly. These are systematic errors that come in addition to the random ones considered in section 5.

P5900, L10: what is the size of these sub-rectangles?

P4900, L24: were -> where

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P5901, L1: Is the density error applied to the entire glacier area or only thickening areas? Should maybe also mention that this error does not account for densification changes.

P5902, L1: Do you mean all glacier cover or just the pure ice caps here? Widespread thickening only occurs in NE Svalbard and on a few isolated ice caps, so maybe “near balance” or “small changes” are more appropriate words for interior areas in general.

P5902, L5: Thinning becomes positive?

P5902, L7: Are these the most appropriate references? It is unclear if you talk about glaciers or ice sheets here. A simpler way would be to just say: “…observed in both studies of ice sheets (e.g. Pritchard et al., 2009) and glaciers (e.g. Gardner et al., 2013). More specific references fits better when each region is discussed.

P5902, L10: Can you be a bit more specific here? Or maybe rather discuss it with each region. Dynamics causes variability, but is not a big player in the total mass budget.

P4902, L12: low -> relatively low (the Canadian thinning is not low in an absolute sense)

P5902, L13-14: What is “area-averaged volume change”? Elevation change?

P5902, L18: Fig. 2?

P5902, L21: Interesting here is the negative skewness of all regions. Worth to mention.

P5902, L22: Fig. 3 and 4? Please check the numbering elsewhere too.

P5902, L23: Is the data density really skewed to mid elevations or is it just a function of glacier hypsometry? I guess the latter, and it can be easily shown by plotting glacier hypsometries with the data sampling in Fig. 3.

P5902, L25: Again, need to account for hypsometry to say if sampling is “poor”.

P5903, L2: Table 3?

P5904, L6: This is not necessarily true. Steeper slopes and larger climatological variations and sensitivities might be as important explanations. For example, Gardner et al. [2012] showed that low elevations have a higher mass-change sensitivity to temperature increase than what the higher elevations have.

L5904, L7-9: Repeats P5902, L8-10. Rather give some examples.

P5904, L10-14: I do not see any reason that dynamics can explain a bias between the interpolation (M1-M2) and extrapolation methods (M3-M4). All methods are based on the same data with the same sampling issues. Please remove or explain better.

P5905, L24: Why are interpolation methods better than hypsometric ones when you say there is a "clear linear relation in elevation"? Counter-intuitive.

L5905, L25: There is a more recent paper with both ICESat and GRACE estimates for the same time period [Arendt et al., 2013].

P5906, L15: ELA-like relationship? No, you assume a thinning/thickening relationship which can be very different and tend to produce more negative mass budgets. I rather suggest a ELA/firn division or constant conversion factor, as in most existing studies.

P5906, L16: Nine years of observations? The ICESat mission lasted 6 years.

P5906, L23-24: Yes, and for that reason it would be better for the paper to stick with volume changes or a constant volume-to-mass conversion factor such that the interpolation/extrapolation procedures become directly comparable without having been shifted by the density scheme which has a different impact in each case.

P5907, L23: The uncertainty range is even more "driven by" by latitude as the High Arctic has much denser sampling than the lower latitudes like Alaska, Baffin and Iceland.

P5908, L8: Again, the density of tracks is the most important factor.

Dsicussion in general: I miss a quantitative comparison with other recent studies, es-

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pecially since you claim that they overestimate the mass losses. All regions except Iceland have previous ICESat estimates, and GRACE provides an independent comparison for the same period. See Gardner et al. [2013] for an overview.

Table 1: This table is good for understanding which of the error components that apply to which method, but I think it would also be clarifying to mention that before each formula in the text using the abbreviations M1-M4. I thought e_fit was for M3 not M2, and if so, it is already a combination of e_dhdt and e_ext, so it is already accounted for. Caption edit: "…combined into an area-averaged elevation change error"

Table 2: Two additional columns with optimum method (M1-M4) and sampling density (N/A) would be useful. Caption edit: "…mean observational elevation change… the area-averaged elevation change" (not thinning rate!).

Table 3: Maybe emphasize the optimum methods in italics or bold.

Table 4: I am struck by how different these values are from what I obtain by multiplying the mean elevation changes in Table 2 with the glacier areas and a density of 900 kg/m3. Is this marked negative shift due to your density scheme? If so, I am very skeptical to such a volume-to-mass conversion, especially when no other scenarios are considered.

Fig. 1: For clarity, maybe each region can be marked with the same abbreviations as in the tables. Suggestion for caption: "Selected glacier regions in the Arctic".

Fig. 2: Should mention what the relevant altitude bin size is. Also, it would be nice if the regions in the figures follow the same ordering as in the tables.

Fig. 3: This figure must be improved. Numbers and labels should be readable and the panels marked with (a)-(f) and corresponding regional names and/or abbreviations. Also, this does not show the "density of ICESat's elevation sampling" since glacier area is not accounted for. Either you need to plot ICESat observations per area in each bin (which should ideally be a straight line) or add another near-similar curve that shows

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the glacier area per bin (hypsometry). See examples of the latter in e.g. Arendt et al. [2013].

Fig. 4: This is a lot of panels in one figure, but given the scope of the paper, I still think they are needed in one way or the other. To improve the readability, I think they should be expanded to the full paper width. If this requires too much space, it could be an alternative to put full size figures in a supplement and then only show some highlights here for the most interesting cases that are (or will be) discussed in the text. In any case, each panel must be marked with its corresponding letter from the caption. Also, it should be mentioned that the color bars and scales are different for each region such that they are not directly comparable (which is unfortunate but maybe necessary to enhance spatial patterns).

References

Arendt, A. A., K. Echelmeyer, W. Harrison, C. Lingle, S. Zirnheld, V. Valentine, B. Ritchie, and M. Druckenmiller (2006), Updated estimates of glacier volume changes in the western Chugach Mountains, Alaska, and a comparison of regional extrapolation methods, J. Geophys. Res.-Earth Surf., 111, doi:F03019, doi:10.1029/2005JF000436.

Arendt, A. A., S. B. Luthcke, C. F. Larsen, W. Abdalati, W. Krabill, and M. J. Beedle (2008), Validation of high-resolution GRACE mascon estimates of glacier mass changes in the St Elias Mountains, Alaska, USA, using aircraft laser altimetry, J. Glaciol., 54, 778-787.

Arendt, A. A., S. Luthcke, A. Gardner, S. O'Neel, D. Hill, G. Moholdt, and W. Abdalati (2013), Analysis of a GRACE global mascon solution for Gulf of Alaska glaciers, J. Glaciol., 59(217), 913-924, doi:10.3189/2013JoG12J197.

Berthier, E., E. Schiefer, G. K. C. Clarke, B. Menounos, and F. Remy (2010), Contribution of Alaskan glaciers to sea-level rise derived from satellite imagery, Nat. Geosci., 3, 92-95, doi:10.1038/ngeo737.

Borsa, A. A., G. Moholdt, H. A. Fricker, and K. M. Brunt (2013), A range correction for ICESat and its potential impact on ice-sheet mass balance studies, Cryosphere, 8, doi:10.5194/tc-8-1-2014.

Gardner, A. S., G. Moholdt, B. Wouters, G. J. Wolken, D. O. Burgess, M. J. Sharp, J. G. Cogley, C. Braun, and C. Labine (2011), Sharply increased mass loss from glaciers and ice caps in the Canadian Arctic Archipelago, Nature, 473(7347), 357-360, doi:10.1038/nature10089.

Gardner, A. S., G. Moholdt, A. Arendt, and B. Wouters (2012), Accelerated contributions of Canada's Baffin and Bylot Island glaciers to sea level rise over the past half century, Cryosphere, 6(5), 1103-1125, doi:10.5194/tc-6-1103-2012.

Gardner, A. S., et al. (2013), A Reconciled Estimate of Glacier Contributions to Sea Level Rise: 2003 to 2009, Science, 340(6134), 852-857, doi:10.1126/science.1234532.

Hurkmans, R., J. L. Bamber, L. S. Sorensen, I. R. Joughin, C. H. Davis, and W. B. Krabill (2012), Spatiotemporal interpolation of elevation changes derived from satellite altimetry for Jakobshavn Isbrae, Greenland, J. Geophys. Res.-Earth Surf., 117, doi:10.1029/2011jf002072.

Johnson, A. J., C. F. Larsen, N. Murphy, A. A. Arendt, and S. L. Zirnheld (2013), Mass balance in the Glacier Bay area of Alaska, USA, and British Columbia, Canada, 1995-2011, using airborne laser altimetry, J. Glaciol., 59, 632-648, doi:10.3189/2013JoG12J101.

Moholdt, G., T. Heid, T. Benham, and J. A. Dowdeswell (2012), Dynamic instability of marine glacier basins of Academy of Sciences Ice Cap, Russian High Arctic, Ann. Glaciol., 53, 193-201, doi:10.3189/2012AoG60A117.

Nuth, C., G. Moholdt, J. Kohler, J. O. Hagen, and A. Kääb (2010), Svalbard glacier elevation changes and contribution to sea level rise, J. Geophys. Res., 115,

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doi:10.1029/2008JF001223.

Nuth, C., J. Kohler, M. Konig, A. von Deschwanden, J. O. Hagen, A. Kaab, G. Moholdt, and R. Pettersson (2013), Decadal changes from a multi-temporal glacier inventory of Svalbard, Cryosphere, 7, 1603-1621, doi:10.5194/tc-7-1603-2013.

Pfeffer, W. T., et al. (2014), The Randolph Glacier Inventory: a globally complete inventory of glaciers, J. Glaciol., In press.

Interactive comment on The Cryosphere Discuss., 7, 5889, 2013.