

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

Anonymous Referee #2

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Mass change of Arctic Ice Caps and glaciers. . . . By J Nilsson et al

1. The most fundamental problem with this manuscript is that it is very badly written. This makes it quite difficult to follow and assess the logic of the arguments presented.
2. Another problem is that the description of the analysis undertaken can only be described as sloppy. This leads to a concern that this might also be the case for the analysis itself, so the authors need to make a convincing argument that this is not in fact the case. This will demand substantial rewriting and additional explanation.

Other more general comments and some suggestions for improving the manuscript are summarized at the end of the comment.

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Detailed comments, keyed by Page and Line Number

1.14: criterion (singular, not plural) 1.15: How can you determine what is the optimum method for extrapolating/interpolating elevation changes if you have no independent knowledge of the correct answer? And on what basis do you justify the claim that you can avoid the need for external validation? 1.25: that both previous estimates, and these current estimates based on ICESat observations might underestimate the mass balance by. . . (NB if you are talking about mass balance, a more negative value is an underestimate, but if you are talking about mass loss rates, then a more negative value is an overestimate – you need to be clear and accurate about what you actually mean). 2.3: What criteria are used to determine “the most suitable method” – again this statement seems to imply independent knowledge of the correct answer. 2.5: The last sentence in this paragraph is, to say the least, glib. The whole issue of what assumptions are appropriately made about the density of near surface materials for the purposes of converting measurements of elevation change to estimates of mass change is completely glossed over. The really tricky issue – that the assumptions that have to be made almost certainly change quite rapidly in a period of climate warming (or cooling) – is completely avoided, so the discussion that is presented is barely credible. 2.7: Intergovernmental Panel !!!... (IPCC)!!! And why reference AR4 when we have new information in AR5 that actually relates to the time period covered by this study? We also have SWIPA, which focuses specifically on glaciers and ice caps in the Arctic, which postdates AR4. Whatever, the final reference needs to be bracketed. Ditto the next paragraph. 2.11: from series of.. 2.12: Isn't it obvious that all measurements are, by definition, biased to the areas in which they are made, and against areas where they are not made? 2.18: determine the geodetic. . . 2.25 to large differences in the. . . 3.4: “based on knowledge of the ice-covered area of each region and densities of surface materials” . You need to cite sources for this – and I would really like to know what this “knowledge” of densities (of what?) actually amounts to. 3.6-3.10: Be specific about how many, and which, methods were actually used in these studies. 3.10-3.12: But little analysis has been performed to. . .regionalization schemes. . . .estimated regional

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mass balance. 3.15: estimates, by analyzing. . . 3.17: over the period 2003-2009. Four methods were used (BUT explain what they were and why they were judged to be appropriate) 3.20 “optimum method” How is optimum defined and/or determined in this case, given the lack of objective knowledge of the true values of the quantities being estimated? 3.23: to derive robust. . . BUT how do you actually know that this approach will reduce the need for external validation data? 3.24: They will also (THEY refers to results (plural) in the previous sentence). Again, given the lack of independent knowledge of the quantities being estimated by these different approaches, it is not at all clear to me how you will determine which methods are best applied in which situations. If you are going to critique methods used by others, you need to properly explain what those methods were and why they are deemed to be problematic – and you need to demonstrate that your approach does not suffer from similar deficiencies. Simply asserting that that is the case is not at all persuasive. 4.1: mass balance, 4.2: excluded because . . . (BUT the argument is not convincing because the same is true for Arctic Canada, Svalbard and Arctic Russia) 4.5: BOLCH 4.7: I cannot see any conceivable logic for lumping glaciers in the Gulf of Alaska with glaciers in eastern Canada – so I presume you actually mean western Canada? But the paper is ostensibly about the Arctic, so you need to explain exactly what you do and do not include as there are virtually no glaciers in western Canada that actually lie within the Arctic – and that is also true for the Gulf of Alaska 4.8-4.10: This sentence is very poorly written and the place names of all three Russian Arctic regions are spelled wrongly. 4.11-4.12: There is now a paper in J Glaciology on the RGI (Pfeffer et al., 2014), so that should be referenced – as should the technical document that describes the inventory. 4.11: were extracted. . . 4.13: What do you mean by “regional elevations for each region”? Since you are using different DEMs for different regions you really need to discuss the accuracy of the DEMs from each region, the difference in date between the collection of data used to construct each DEM and the data on which the altimetry data used in your analysis were collected, and the possible significance of those differences for the outcomes of your analysis (including the uncertainty associated with your conclusions). In particu-

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lar, I presume that large areas of these DEMs consist of regions that are permanently snow-covered. If this is the case then elevation errors may be very large, depending upon how the DEMs were actually constructed. This issue simply has to be addressed. 4.18: ICESat carried (past tense as the satellite is no longer functional). . . The system operated. . .and had 4.22: footprint approximately. . . 4.25: delete “to date”; . . .period 2003-2009. General point about section 2. The problem to be addressed in the paper is very poorly defined and the motivation for tackling it is not stated convincingly. 5.2: Data were. . . (data = plural of datum) 5.6: ground tracks do not. . . there can be large (up to one degree) offsets. . . . 5.10: using the method.. 5.12: samples of what? 5.15: Why do you consider that outliers are errors rather than regions of unusually slow or rapid change? 5.16: define what you mean by “ low number of data points” 5.20: unweighted 5.21: smoothing was undertaken to remove noise from the — BUT does smoothing actually remove noise or incorporate it into the estimates of the moving average? 6.3: tracks were. . . 6.4: change was estimated.. 6.5: data are available 6.8: What do you mean by “the regional elevation change”? and by “regionalized” 6.11: I think you mean “Data for glaciated regions were then extracted. . .” 6.14: varies with elevation 6.15: this is not straightforward. 6.16: How do you rule out the possibility that positive elevation change values are due to flux convergence? As the density of firn. . .we assigned it an average density. . . BUT – given that there is plenty of evidence for secular changes in firn density as a result of the climate warming that has occurred over at least some of your study regions during the ICESat era, I have to say that I think this assumption is almost certainly wrong, and likely to a degree that has significant bearing on the reliability of your results. At the very least, this issue has to be discussed thoroughly in a revision of the manuscript. If you just brush off all the inconvenient realities to keep the analysis simple, it’s unlikely that the results of the analysis will be of much value. The last sentence in the section is absolutely correct, but it raises the question that, if you don’t even attempt to deal with such a major issue , how can you legitimately claim to be able to identify other sources of uncertainty in your results? 7.3: . . .the individual interpolated grid values are multiplied by the grid cell (pixel) area

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and summed as follows: 7.9: the procedure described to me just seems to be interpolation, so I don't actually see how it can be claimed to increase resolution? 7.15: as ARE the model coefficients 7.19: How do you know it would be less robust in such areas? 7.29: "sufficiently smooth error surface" – what exactly does this mean, and how is it defined? 8.4: parameterizing 8.5: glaciated area, 8.8: What exactly do you mean by: "multiplied with the glacier area-elevation distribution"? 8.11: value assigned to the elevation bin z 8.12:-8.15: sentence needs rewriting so it actually makes sense 8.16: referred to as M3. ...parameterize. ...changes involves fitting a ...elevation (as in.(2010a)) 8.23: referred to as M4. ... parameterize. involves binning the. ... 8.27: Values for bins that are empty (due to lack of data for the specific elevation band) are estimated by. ... 9.5: can be incorporated. ... 9.6: statistic 9.8: which ratio? 9.9: parameterize 9.10: fits the distribution best (as measured by r², as used by. ...) 9.12: An elevation bin range of 50m was chosen for all regions, consistent with Gardner et al. (2011). 9.23: By measuring the shift. ... 9.24: smallest mean shift relative to the ...of values, ...BUT this paragraph needs further explanation (point 2 in particular). In this section, I think you are trying to say that the hypsometries of the ICESat ground track and essentially the DEM are the same –so why not just say that clearly? 10.1-10.4: need supporting evidence for this statement 10.3: assumption because the high latitude. ... 10.6: central to error analysis 10.8: follows the approaches of Nuth et al., (2010) and Moholdt et al. (2010a). 10.10: errors over ice sheets 10.11: approach and assumed an error of ... BUT, what is the evidence that this approach is valid 10.12: There is also an inter-campaign bias. ... (Siegfried et al., 2011) 10.13-10.15: Odd sentence – either the bias is included in the ICESat error or it isn't. 10.17: least squares solution to what? 10.19: brackets around the reference 10.21 either (i) divided. ... uncorrelated, or (ii) divided 10.25: we combined the two approaches 10.25: "divided the tracks into elevation bin segments" – what does this mean exactly? 11.4: How do you know the segments are uncorrelated? 11.8-11.10: What does this sentence mean? 11.11: How do you justify this assumption? 11.15" "collocation prediction of non-zero data" – what does this mean? 11.16 parameterization.. 11.18: original el-

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evation change estimates. ... 11.23: In this study we adopted the. ... (2010), where the 12.5: referred to. ...so as not to. ... 12.15: but you cannot reasonably assume that the density of firn is a single constant number 12.15: are the densities of ice and firn respectively 12.16: estimated as the RSS 12.18: error by the regional area. 13.2: large peripheral thinning is not obvious from Figure 3 13.4: How can thinning become more positive? You mean the thinning rate is less 13.5-13.7: I think this statement is misleading because much of the low elevation thinning in Greenland and Antarctica is linked to changes in ice dynamics, rather than surface mass balance. 13.8: large variability in what? What are clustered around the coastal regions? 13.9: Are you sure that large variability in thinning rates is always associated with fast flowing outlet glaciers? You should really demonstrate this statistically. 13.11: exhibit. ... 13.12: volume change, while regions such as ICEL. ... 13.13: change, while. ...BUT the Iceland and Gulf of Alaska regions are much warmer/wetter than the other study areas – so this needs discussion 13.16: volume changes, we.. 13.20: Arctic regions show a lower. ... 13.22-23: such a distribution is exactly what you would expect for ice caps with a convex upwards surface profile (steep slopes at the margins and decreasing are with successively higher elevations in the relatively flat interior regions 13.23: For Figure 3 to be meaningful you also need to show the actual distribution of area with respect to elevation 14.2: Table 3, I think 14.16: tend in general.. 14.24: facts that the most positive elevation change estimates. ...elevations and these higher elevations account for only a small ...The only case where this was not true was Svalbard 15.1: How do you define "optimum" here? ...were determined. 15.5: low elevation variability: the actual rates plotted don't look anything special to me, so I'm not persuaded that you are catching dynamically-driven variability here, as opposed to variability in surface ablation rates. What is the evidence that the ACTUAK areas in question are active and undergoing rapid changes 15.17: "average a lower estimate" – confusing – do you mean more or less negative? The last sentence in the paragraph is not clear. 15.20: only need to say "we find that.." once 15.21: What is the optimum approach? "produces" should read "produce" 15.22-15.24: To make a statement like this and be

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convincing you need to undertake a proper analysis of thinning rates over fast and slow flowing marginal regions, and you also need to evaluate the sampling bias associated with the location of ICESat tracks relative to surface velocity patterns 15.26: “usually” is redundant 15.27: First 8 words of sentence don’t make sense 16.1: Why are highest elevation areas under-sampled by ICESat? 16.1: their small size. . . 16.3: What assumption? 16.4: that sample the ice cap geometry only sparsely 16.6: Figure 2a shows large. . . elevation change rates at both lower. . . and higher . . . elevations 16.8: number. . . . variability in the data cause.. BUT- I don’t understand lines 16.6-16.9 16.11: does “overestimate” mean too positive or too negative in this case? 16.13: You really need to properly explore how bad is the sampling bias against outlet glaciers 16.15: symmetry to the . . . relation with elevation) 16.18: Given that none of the in situ data are presented in this paper, and you don’t present any quantitative evidence either, this statement is unconvincing and potentially misleading. 16.19: region’s 16.24: inside previous estimates published by. . . BUT what is the basis for these estimates> 16.25: Another study (Berthier et al., 2010), however indicated. . . overestimated by. . . 16.29: the degree of inter-annual. . . (due to ice dynamics. . .) in the elevation changes observed, 17.3: Arctic have a continental. . . 17.5: Not convinced that Figure 4 actually shows a “spatially uniform pattern of elevation change RATES” 17.10: first indicator of what? 17.11: changes is larger. . . 17.11: Why should one consider using several independent methods in this case? 17.17-17.19: Why would the accumulation zone thicken if the ice mass is in overall equilibrium, or in a state of overall negative mass balance. This makes no sense to me. 17.20: has less impact – BUT less than what? 17.22-17.24: What rules should be followed in making the decision about which density conversion scheme to use? 17.27: where in Figure 3 can this be seen? It is not proven that these signals have a dynamic origin, so why interpret them this way? How can you rule out ablation rate variability as a cause? This could be linked to albedo variability, especially in a place like Iceland where the patchy distribution of volcanic ash in glacier ablation areas is likely a large source of variability in surface albedo and the protective effects of debris cover that could result in large variability in ablation rates. 17.28: Pa-

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parameterizing (should do a global correction on this word as I think it is spelled wrong every time you use it) 17.29: delete “the polynomial” 18.1: The low elevation.. BUT the rest of the sentence starting “usually” doesn’t make any sense. Nor does the following sentence. 18.3: due to their ability.. 18.4-18.8: I don’t think this makes sense either. 18.9: balance because different methods tend.. 18.10: We have shown that. . . . regions exceeds the error estimates 18.12-18.13: One should exercise caution. . . 18.19: impact of different regionalization schemes on mass change estimates. 18.22: different estimates of mass change rates . . . BUT strongly correlated with what? How do you know it is driven by inter-annual rather than spatial variability? I don’t actually think you have evaluated this question quantitatively. 19.1: produce 19.2: estimates than the interpolation methods 19.2-19.4: This is not a sentence! 19.4-19.6: You need to explain the basis for this statement 19.7-19.13: I don’t think anything in this paragraph has been proven at all. 19.19: Isn’t it “Top Level Research Initiative?”

A couple of more general points:

1. I think you need to conduct a more rigorous evaluation of other possible causes of variability in thickness change rates – such as surface albedo and snow cover variability.
2. You need to investigate the role of dynamic effects in observed thickness change patterns much more thoroughly. At present, the case made is essentially based on assertion rather than evidence.
3. Why not evaluate patterns in the rate of thickness change in individual years to clearly differentiate spatial and temporal variability from each other. If you don’t do this, the analysis will come across as essentially speculation.
4. Caption to Figure 2 – here you say the Russian Arctic is treated as a single region – but in the text it is presented as three separate regions.
5. Figure 3: axis labels are unreadable. Not clear which region is represented by each panel. What time period does the analysis refer to?
6. Figure 4 – same comments as for Figure 3.

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

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