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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.

**J. Nilsson et al.**

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We would first like to thank Geir Moholdt for his insightful comments and suggested improvements of this manuscript. The response to the discussion part of the review is sorted by paragraph:

2. The main purpose of this paper is to determine the impact of different inter/extrapolating schemes of elevation changes to see how they affect the final mass balance result. The choice of a most suitable method (currently referred to as optimum in the manuscript) is only secondary and will vary as other methods are included. As the use of the word "optimum" might confuse the reader we will change this to "pre-

ferred" instead to soften its implications.

3. The main issue with using several different methods is how to judge their accuracy or quality in Arctic areas where validation data is often sparse or unavailable. We choose the original histogram as a relative proxy of accuracy, because the minimum requirement of the inter/extrapolation methods must be to at least reproduce the central tendency of the data set. The use of the word 'optimum' is based on the relative statistical measure comparing the original data with the inter/extrapolated estimates. Using only the raw mean would not capture the local variability in the data. On the other hand it would help us to identify methods that are less or more suitable than others of capturing different regions spatial elevation change patterns. The possible improvement of the mass balance lies in the possibility that one might find a method that better captures the spatial variability of the data. So by using several methods a more detailed understanding of the spread might be gained and hence the robustness of the mass balance estimate in the area. This does require that the ICESat sampling is random and that the elevation-area distribution have been adequately sampled.

4. Sub-sampling or cross-validation are indeed methods that can be used instead of the histogram approach. Another approach would be to compare the RMSE of the inter/extrapolated values at the locations of the ICESat elevation change data. The histogram approach was used in this analysis because of its simplicity. Further we agree with the reviewer that a more in depth discussion about the spatial sampling and how they relate to the different methods is warranted.

5. Unfortunately when the paper was typed for discussion there seems to have been some problems with labeling and other layout issues that we missed in the proofreading. These issues will be corrected in a revised manuscript.

6. The size of the figures containing the continuous fields will be increased and the layout will be improved. This to hopefully make it easier for the reader. We will also include more details on how realistic the M1-M4 methods are compared to point-data



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(as discussed above).

7. In a revised paper we will include the information about the ICESat sampling versus the DEMs elevation-area distribution. Furthermore, we will also include a 900 kg/m3 density scheme to make the results more comparable to other studies and the inter-comparison of the different methods. Though the approach that we have taken (500 kg/m3 (+) and 900 kg/m3 (-) density scheme) for conversion to mass change shows good agreement with many areas, such as RUS, CAN and CAS.

Abstract and introduction: The reviewer raises some good points and the manuscript will be changed accordingly.

P 5891, L7: will be changed accordingly

P 5891, L 10: will be changed accordingly

P 5891, L21: Though the magnitudes of the elevation changes are large, the spatial sampling is usually less dense. This will, in the authors eyes put constraints on the information that can be extracted due to less robust statistics, speaking in general terms.

P 5892, L9-12: Though there has been other studies done using these types of extrapolation scheme there is still many different interpolation/extrapolation schemes available that have not been discussed or researched. Thus the main idea of this study is to determine how sensitive different regions are to the application of different inter/extrapolation methods. The range of values will then indicate the robustness of the estimated mass balance.

P 5893: The reasoning here will be changed accordingly

P 5893, L12: will be changed accordingly

P 5893, L16: The DEMs are made from photogrammetry, and the hypsometry shows good agreement with ICESat over the different regions. We will in a revised manuscript



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include some analysis that compare the DEM we are using with for example ASTER.

P 5894, L3: A list of quality flags will be included in the revised document or suppl. material

P5894, L9: Tracks are visually inspected and removed if they displace the binned ground track position.

P 5894, L10: will be changed accordingly

P 5894, L13: This is always a trade off between noise reduction and keeping dynamics signals (not referring to ice dynamics). In this study we are mostly interested in the overall regional elevation trend so we have chosen to remove as much high frequency content as possible without destroying the overall signal.

P 5894, L17: The 'local window' refers to the subset of points used for the smoothing procedure around every along-track point. The text will be changed to make it more clear for the reader.

P 5895 L3-7: The text will be changed, as the 6 years referred to in the manuscript should actually be 6 months.

P 5895, L9: This would correspond to roughly 1 km resolution depending on latitude.

P 5895, L13-23: In a revised paper we will include also the 900 kg/m3 estimates for easier comparison with other studies that have used the constant density assumption. On the other hand the application of our density scheme shows only small difference against the pure 900 kg/m3 for many regions except Svalbard.

P 5896, L25-26: An appropriate reference will added to describe the least squares collocation procedure. LSC for stationary signals can be considered to be similar (but not equivalent) to "Simple Kriging".

P 5896, L10: It does not improve the cross-track sampling at all. The main goal is to fill out larger areas affected by the editing procedure (quality flags) and post-processing



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(outlier removal).

P 5896, EQ 2: This is done on a regional basis and the model order used for the different regions, except Svalbard (N=3), was N=1.

P 5897, L5: will be changed accordingly

P 5898, L26: The outlier rejection procedure does unfortunately not remove all outliers. There is the problem of what should be considered as an outlier. In this case we have chosen the median as we believe that this value better represents the central tendency of the data, compared to the mean, in the presence of outliers, dynamics and other factors.

P 5898, L3-14: This paragraph will be moved up

P 5899, L2-4: The main point here is that when the tracks converge in the north more data will be available over the glaciated regions than in the lower latitudes, thus higher data density per area unit. This will be rewritten to make it more clear for the reader.

P 5899, L12: This will be either be divided with the number of years or used as m/yr for an even more conservative estimate.

P 5899, L13: This is true! Unfortunately, these were not available (to the authors knowledge) at the time of publication.

P 5900, L10: The size of the rectangles is 10 kmˆ2

P 5901 L1: This is applied to the entire glaciated area. A short comment will be added that states that this doesn't include densification.

P 5902, L1: I would agree with this and the manuscript will be modified accordingly

P 5902, L5: Change to "less negative".

P 5902, L7: will be changed accordingly

P 5902, L10: A more thorough discussion will be applied here, including examples.



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P 5902, L12: This is referring to the magnitude of elevation change and not the actual volume change and will be corrected to make it more clear, as dV/A equals elevation change.

P 5902, L13-14: See previous comment

P 5902, L18: will be changed accordingly

P 5902, L21: Yes this is true.

P 5902, L22: will be changed accordingly

P 5902, L23: This will be provided and plotted against the ICESat elevation changes.

P 5902, L25: See above

P 5903, L2: will be changed accordingly

P 5904, L6: Its important to know when we talk about dynamics we do not referee to ice dynamics. We use the word to describe high variability and signal content (multiple signals). This can be for example ablation rates, ice dynamics, temperature and other unknown ones. In hindsight, due to the target audience, we should maybe have used a different wording. On the other hand studying CAN one can detect clustering of highly variable elevation changes in areas with drainage systems.

P 5904, L7-9: Examples will be given instead, to reduce repetitiveness.

P 5904, L10-14: See P 5904, L6 and for the reader this will be made more clear in the next version of the manuscript. The main argument would be that the interpolation captures the local variability better than the polynomial. This due to regional fitting procedure of the polynomial.

P 5905, L24: Better in the sense that it shows larger agreement with the original histogram. Probably due to that it can handle local or spatial variations better.

P 5905, L25: This will be included in the paper.



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P 5906, L15: A fixed density of 900 kg/m3 will be provided in the next iteration.

P 5906, L16: will be changed accordingly

P 5906, L23-24: See 5906, L15.

P 5907, L23: True that the latitude dependence is a factor but when one looks at CAN and CAS these two areas both show convergence of all four methods. This even though they are both spread over a large range in latitude (both low and high). The effect of latitude should also be reduced by the hopefully random sampling of the ICESat data on a regional level.

P 5908, L8: See 5907, L23.

Table 1: More clarification will be included or moved to the text. The M2 methods does include the e_fit error and is a part of it.

Table 2: Good point will be included

Table 3: OK

Table 4: I don't think the density scheme is the culprit here. I think its the difference in method used to derive the elevation changes.

Fig. 1: This was done but seemed to disappear in the typesetting of the manuscript

Fig. 2: Will be included in the next iteration of the manuscript

Fig. 3: Yes we do agree! This is something we missed when checking the typesetting of the manuscript. Also glacier area per bin will be included in the next version.

Fig. 4: Again typesetting problem that we missed and some of the figures might also be removed, as they might be redundant.

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