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Interactive comment on "Geodetic mass balance record with rigorous uncertainty estimates deduced from aerial photographs and LiDAR data – case study from Drangajökull ice cap, NW-Iceland" *by* E. Magnússon et al.

Anonymous Referee #1

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The manuscript by Magnússon et al presents a time series of DEMs covering the period 1940s - 2011 for the Drangajökull ice cap in NW Iceland generated from historical aerial photographs and controlled with a contemporary lidar DEM. While the approach is not new, the data are new for the region and the authors' undertake a novel and robust error analysis and simulation that are of considerable interest and value. This is an important study and I find the quality of the manuscript generally good; it is well written and nicely presented. However, there are a few issues that need to be addressed before it is of publishable quality for The Cryosphere. Below I provide general comments on the

manuscript followed by more specific editorial suggestions.

General comments:

I'm uncomfortable with the way the authors brush over the use of lidar as control for a photogrammetric DEM and I think they could provide better acknowledgement to papers that have used this approach (which I point out in the specific comments below). First, as the underlying lidar DEM is of paramount importance to the quality of the DEMs, it is surprising that very little is said about the lidar data set itself. This is addressed in another publication and the authors provide a blanket accuracy statistic of 0.5 m. However, these sorts of figures are typically associated with actual lidar hits (not interpolated) over a reflective surface (which wet glaciers and dark mountain rocks typically are not). The DEM used by the authors is interpolated to a 2 x 2 m grid (no info on the original resolution) which will degrade quality further and contribute error to the resultant ground control. There is also significant uncertainty associated with identifying homologous points between an optical image and a shaded-relief topographic model. I think more discussion about the lidar DEM and the resulting errors are required. At the very least acknowledgement that a significant component of the errors they report could be as a result of poor quality GCP. Second, typical photogrammetric adjustment algorithms expect ground control to be an order of magnitude higher accuracy than the resulting DEM elevations and the sigma values that control the degree to which these parameters can be adjusted in the block adjustment are set accordingly. In order to use atypical quality ground control, especially when providing a time series of surfaces which are to be compared, it is important to address how the system deals with the higher error in the GCPs. Changing the sigma values for the adjustment can have a significant impact on the RMS errors of the GCP which the authors have provided in Table 2 but without appropriate values for sigma, the RMS errors are not good indicators of data quality. I appreciate that all this is detail that may not be of great interest in an already long manuscript but there needs to be an appreciation that it is not as straightforward as the authors imply, especially when they suggest in the conclusions

that satellite imagery can be used in the same way.

The methodology could be written in a way that's easier to follow. Several methodological issues are raised in a number of locations in Sec 2 which would be easier for the reader if they were addressed together. Specifically, the GCPs and placement strategies, the manual editing and delineating glacier margins. It's also unclear to me how the conversion of glacier volume changes to w.e. is addressed or if in fact it has. The correction for date offsets in DEMs has been addressed (or purposely ignored) in a number of recent publications but the paragraph that discusses this issue is lacking literature support.

The conclusions could be strengthened. The first paragraph is awkward and isn't describing anything new; several previous papers have generated DEMs at various epochs without support from field data. The second paragraph describes the contribution from this manuscript but does not make sufficiently clear its significance. The revelation of the importance of seasonal corrections is also not that surprising especially for glaciers with high annual turnover. This case could be made stronger with more reference to previous literature that deals with this earlier in the manuscript. Finally, the resultant estimates for B ÌĞ would be more meaningful with context.

Specific Comments

Page 4734

Line 2 – is 'constraints' the right word here? Maybe, ". . . can be used to control glacier surface DEMs. . ." or simply ". . .can be used to extract glacier surface DEMs. . ."

P4735

L14 – the sea-level

P4738 L8-15 – While the lidar data collection of Drangajökull is covered elsewhere, it would be useful to the reader to include some basic information here since the lidar underpins the rest of the data in this manuscript. Who undertook the survey and in-

strument used and basic flight parameters I would have found useful. When reading, I assumed this was a data set with which I was familiar but realised after consulting Johannesson et al. (2011) that is was not.

L19 – manual editing not edition

L26-27 – Was a similar set of ground control points used in each model? I think the authors explain this on P4739 L3-5 where Kraus (2007) is cited but the language isn't clear here. The meaning of 'regular scheme of distribution' isn't clear. Maybe state this more explicitly. Later in the manuscript you suggest that they were positioned to be spread around the glacier and throughout the vertical extent of the terrain. I think this is important and should be stated clearly. The whole methodology described here was first presented in James et al. (2006) (and again in James et al., 2012) and are a more appropriate and accessible reference than Kraus (2007).

P4739

L1-3 - This one sentence implies that extracting GCP from lidar is trivial but I do not think this is true and certainly becomes less trivial with satellite imagery as raised in the conclusions.

L1-5 – Again, I think an acknowledgement to the work in James et al (2006) is appropriate, predates Kraus (2007) and is more accessible to readers.

L19-21 – I think I know what you mean here but the sentence is awkward and I don't think this will be clear to many readers. Can you be clearer about what you mean by matching in lower resolution? I presume you mean using a reduced resolution image to undertake the matching which typically improves matching success in low contrast areas but I don't think most readers will know this. Also, the effect of a larger window size and lower correlation coefficient will be useful for readers not familiar with stereo-matching.

L25 – why the different grid resolutions? This does not coincide with GSD from Table

1.

P4740

L6 – again why the two different grid resolutions here?

L7-9 – Where/why is their only monoscopic imagery? Your text and Figure 2 suggests that all your imagery is stereoscopic. Also, for the older epochs, the elevation of the lidar will be quite different at the contemporary ice margin and will result in fairly significant errors in the orthoimages especially if off-nadir parts of the imagery are used in orthorectification. For this reason I think you can't use the blanket term "accurately" on L8. For delineating the margins maybe this error isn't that important but you can't say that it's accurate. Maybe specify estimate worst case scenario errors and state this is sufficient for delineating ice margins.

L19-21 – I think this is a very important point as is true for comparisons between the other DEMs as well.

L23 – deduced is not the right word. Perhaps derived?

Fig S1 – Can you specify in this figure that the white areas are those that required interpolation?

P4741

L2-3 – this is a very complicated way to say that only bare surfaces were used in the quality assessment. It would be more straightforward to say this explicitly and then add the 40 horizontal distance condition.

L4 – semiautomatic classification is vague. Supervised classification?

L24-27 – this sentence is awkward

P4742

L25 – of the errors

P4743

L1-4 – this really should be raised earlier when GCPs are first raised.

L6 – 'allow' rather than 'secure'

P4744

L1-2 – another bit that should be realized earlier since this is not part of the bias correction discussed here.

P4746

L1-2 – do you mean they are less dense inherently as part of the DEM generation process or by your design because I find the opposite to be true. The snow free ice surface has quite a lot of texture.

L8-10 – this should reference Cox and March (2004) which is the first instance I know of that points out the variability of elevation changes will be lower than absolute elevation.

L16 – photogrammetric not photographic

P4749

Sec 2.6 – lacking literature support here for a very common problem.

P4757

Sec 3.3 – how were the w.e. units derived? It's typial to address the snow/firn/ice density issue here.

P4759

L24-2 – The interpolation of surface change rather than surface elevation due to its lower variability is not new. For example, Cox et al. (2004) uses this approach in their comparison of geodetic and glaciological mass balance techniques. This should be acknowleged here and where this was raised in the methodology.

P4760

L3-13 – As I have said in my general comments above, there's an important element to using relatively low-quality ground control to control a photogrammetric model that has been omitted from this manuscript.

P4764

L25-26 - the first sentence here is awkward. Consider revising.

P4765

L4 – DEMs

Figures – I find the figures to be too small in print format. The annotation text is almost invisible.

Figure 1 could be improves by making part c the main figure with two insets. The location of Vatnajokull and Langjokull are not relevant here.

Figure 2, each subfigure could be a full figure with the relevant DEM as the background image maybe to appear later in the paper. This would be more useful to readers.

Figure 3 is completely illegible in this size. Should 'vicinity of the glacier' be 'off ice'?

References

Cox, L.H. and March, R.S., 2004. Comparison of geodetic and glaciological mass-balance techniques, Gulkana Glacier, Alaska, U.S.A., Journal of Glaciology, 50: 363-370.

James, T. Murray, T. Barrand, N. & Barr, S. (2006). Extracting photogrammetric ground control from lidar DEMs for change detection. The Photogrammetric Record 21(116), 312-328.

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