The Cryosphere Discuss., 8, C483–C488, 2014
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The Cryosphere Discussions
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Interactive comment on "Elevation and elevation change of Greenland and Antarctica derived from CryoSat-2" *by* V. Helm et al.

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Received and published: 22 April 2014

General comments
The paper presents results from Cryosat-2 radar altimetry. Two Digital Elevation Models (DEM) were derived for the Antarctic and Greenland ice sheets respectively. In addition, two maps of elevation changes of these ice sheets for the period 2011-2012 are presented. With so much content the paper is dense. In a context of "race towards the number of papers", I appreciate the effort to combine several results in a paper rather than producing 4-5 shorter papers. However, this forces the authors to go rapidly through many points that would deserve further developments.
The authors report that the volume loss increased by a factor 7 in West Antarctica between the ICESat period (2003-2008) and 2011-2012; this, for instance would need

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to be further investigated. However, 2 years is an extremely short period of time for the interpretation of elevation change; it should be stressed more clearly.

Specific comments

Introduction
Introduction gives a good overview. It seems to be a choice not to have any reference in the introduction. The reference come later but I would prefer to have them from the beginning.
p.1674 l.16: I understand the error is given with an uncertainty as a convenient way to present bias and standard deviation but this form might not be very clear for all readers.
l.23: I prefer the formulation to write “around the mid-19th century” than “150 years ago”
p.1675 It would be good to cite Wingham et al. 2006a on this page.
p.1675 l.27 you could add that the across track footprint is not reduced
p.1675 l.28 – p.1676 l.1: In the first half of the sentence, you use a passive form, and in the second half, an active one is used.
p.1676 l. 5-9 New retracker: this could be the subject of a paper in itself. p.1676 l.24 cite Bamber et al., 2009 ?

2 New digital elevation models of Greenland and Antarctica
p.1677 l.7 grid posting vs resolution: is 1 km the actual resolution of the dataset or is it the grid pixel size? It seems to be the pixel size only, and some filling is needed towards the lower latitudes (across track spacing of 2.5 km at 70°).
p.1677 l.18 You already explained what SIRAL stands for in the introduction.
p.1678 l.4-9 How is the “ridge sampling” taken into account? Could you propose a method to resolve the valleys (e.g. to give more weight to the few measurements from

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the bottom of a valley)? A zoom of figure 12 on the Antarctic Peninsula, for instance, could give the reader a better idea of how valleys are affected.
p.1678 l.18
-I would suggest “map” instead of “distribution”.
-You can add a reference to figure 3 here.
- How is the slope map derived? Is it simply the gradient of the DEM?
p.1679-1680
- Is the dependency of Cryosat data on slope a residual effect of the slope error ? With a 1- I would have expected some residual elevation difference between ICESat and Cryosat due to microwave penetration through the snowpack. It seems the proposed retracker bypasses this problem. It would be interesting to have a more thorough discussion about this subject. Penetration depth and its variation with time are crucial for the understanding and interpretation of elevation changes in particular.
- Another point is worth discussing: I understand that a gridded DEM cannot reproduce sub-pixel topographic features and I would expect that the standard deviation is larger after gridding (i.e. the black in figure 6b is above the blue one). But it does not seem as clear why the median error is also larger for the gridded DEM. Is it a bias introduced by the “ridge sampling”, with more points at higher elevation producing a higher surface?
- The lapse of time between the acquisition of Cryosat data and ICESat data used for the comparison is not discussed. Over the 6 year-period, places such as Pine Island Glacier terminus lost several tens of metres. Is that not a problem (although a much localized one)?
- In the comparison with the GIMP DEM (p.1680 l.25), it can be expected that the GIMP DEM is comparing more precisely to ICESat data as ICESat measurements are at the core of this DEM. This paragraph does not tell us much.
p.1681: The comparison with airborne data is more interesting. The patterns described (raster, star-like) in table 1 need more explanation. It is a bit difficult to link the text to the table but it clearly shows that the GIMP DEM is not “outperforming” much the Cryosat DEM presented here, except in terms of resultion. It is not clear from table 1,

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is the airborne data compared to the DEM (as the caption suggests) or to raw Cryosat measurements?

3 Ice surface elevation change

p. 1682 l.15: Why use only two years of data? Is there not more available? The long-term significance of the derived elevation trend is directly linked to the length of the observation period. If possible, a longer span would be a plus for the revised paper. Also the time span is never clearly stated, only given by a vague "2011-2012". I guess all areas are not sampled at the same time and the period covered might not be exactly the same for all areas. Is the smoothing applied to the map enough to solve this problem?
p.1682 l.29: This is the only time you talk about mass change in this section.
p.1683 l.5: You do not define what you call significant. Is an elevation change significant when it is larger than a given fraction of the annual accumulation? When it is above the uncertainty given in figure 12 and 13? On figures 7 and 8, you could hatch the areas corresponding to "non-significant changes".
p.1683 l.11: "... values of 0.2-0.3 m yr^{-1} have been reported", could you add a reference?

3.2 areas of large elevation change
p.1684 l.13: The ICESat tracks are not visible on figure 9.
p.1684 l.14 and table 2: In table 2, all uncertainty estimates from "this study" are null. Is that a typo? If not, you might need to reconsider the way you compute these errors.
p.1684 l.21: Are the values in table 2 interpolated at the location of each GPS site? If you do not take the nearest neighbour and the real dh/dt field is smooth enough, the distance to the closest data point should not be too much of a problem.
With a 50-km smoothing, local minima in the centre of PIG are very likely smoothed

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out. p.1685 l.8: Do your point dh/dt estimates (before gridding) give larger elevation loss? You could give the extreme local value you obtain on the terminus, or closest to where Hurkmans et al. (2012b) gave their estimate.

4 Conclusions

p.1685 l.21: The use of "this" at the beginning of the line is ambiguous. Is "this" the Greenland contribution to sea level rise?

p.1685 l.27: You report an increase by a factor 7 of volume loss in West Antarctica, although the loss is known to be accelerating and the time period considered is short, this would require more than a line in the conclusion.

Appendix A1

p.1686 l.23-24: A full cycle starting in January 2012 finishes in January 2013, is that right? Why not use all of the data for dh/dt estimates?

Appendix A3

p.1689 l.2:

- You wrote "variance (SD)". Variance is not the same thing as standard deviation.
- Roughness does not have a common definition and you never give yours. On a horizontal scale, a sastrugi field is metre-scale rough, mega-dunes create roughness at footprint scale etc. Then, is roughness the standard deviation of actual elevation within a given area? This requires more precision.

p.1689 l.7-8: So the range for slope is 2°, for SD 12m, for roughness 5m and for number of points 1000? Is each parameter range divided in 100 bins and the parameters fitted separately?

p.1690 l.1: This description of the error estimation is difficult to follow. Could you give the formula for W_i as well?

From what I understand, these weights mean that the better the fit of a polynomial to

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the error distribution based on one parameter, the better this parameter represents the error?

Appendix A5
p. 1690 l.14: Could you give more precision on how you relocate the LRM data? A 1-km precision does not seem sufficient.
p.1691 l.3: dh/dt unit should be in m yr^{-1}
p.1691 l.4-5: "the resulting data are averaged": does that mean "all dh/dt for a given year 1 point are averaged"?
p.1691 l.6-7: Does it still make sense to produce a grid with a 1-km posting when you are using a 50-km search radius for the block median?
p.1691 l.16 and l.21: both variables should not have the same name.
p.1692 eq.A5 replace DEM_i with DEM_{ref} for the last index.

Figures
Figure 6: there should be a label on the x-axis.
Figure 7 and 8: superimpose some hatching on areas where non-significant elevation changes occur?

Interactive comment on The Cryosphere Discuss., 8, 1673, 2014.