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The Cryosphere
Discussions

Interactive comment on "Getting around Antarctica: new high-resolution mappings of the grounded and freely-floating boundaries of the Antarctic ice sheet created for the International Polar Year" *by* R. Bindschadler et al.

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On behalf of the author team, I thank this reviewer for the comments submitted and for their acknowledging the magnitude of this mapping effort. Most of the offered comments have led to improvements in the revised manuscript, some directly and others by illustrating confusion on the part of a reader that led to clarifications of the text. Below, detailed responses are given with the first few words taken from the associated paragraph of the review.

"1) Mass budget calculations. . ." The reviewer is questioning the value of the H line for the purposes of calculating mass balance. Because calculating mass balance is not the purpose of the paper, in the revised manuscript we defend the value of mapping this diagnostic property in its own right. As ice shelves thin and warm, the position of the H line will change. Thus, for the same reason that the grounding line position has been monitored as a change diagnostic, the H line is similarly of interest. Nor has this boundary ever been measured before. The advent of repeat precise laser altimetry provided a new capability to map these positions and we employ our technique to interpolate these measurements to a more complete data set. It has errors and it will be improved with time, but just as the initial mapping of the grounding line had errors and was improved, one has to start. These reasons are enough to justify mapping the H line, but I also dispute the reviewer's contention and defend the use of the H line as a useful "gate" for mass balance calculations. If the discharge at the grounding line were known, then measuring the discharge at the H line gives additional spatial resolution of where mass gain/loss is occurringâĂŤmultiple discharge gates is a very common goal of mass balance calculations and adds value to the interpretation of mass balance calculations.

"2) Another issue is the fact that most (say 80-90%?). . ." I don't dispute that most Antarctic ice is discharged through glaciers, but the 85% figure claimed by Rignot et al. (2008) is highly questionable. Rignot et al. state in the Methods section of their 2008 paper that they determine ice thickness by assuming hydrostatic equilibrium for altimetric elevations at their InSAR-determined grounding line. This grounding line is the most landward of any of the grounding lines mapped by any method so this reviewer should fully appreciate the errors associated with this method (as do we). We pursue this point in our revision and show that errors resulting from this assumption can be very large (or not). My concern is not assuaged by the statement that on Pine Island this method produced only a 4% thickness error. Pine Island is well known to have a lightly grounded ice plain, so the hydrostatic assumption may not be so bad in this particular case. The general case produces ice thickness errors biased higher, probably much higher, than those claimed by Rignot et al. (2008), thus discharge values also will be biased high, not random, as the authors claim. Yet even at 85% of the discharge, there is a considerable proportion of the discharge left unaccounted and, again, mass balance calculations are not the purpose of this paper.

"3) H is not the point of HE because the ice shelf does not deform elastically…" Vaughan (1995) and Rignot (1996) show that HE is an excellent approximation to ice shelf bending. Brunt et al.'s (2010) H points are the locations along repeat ICESat tracks where the amplitude of ice shelf vertical deflection equals the full tidal amplitude. At this point and seaward, the freeboard height is independent of the tide, so the relationship between freeboard and ice thickness is also independent of the tide. On the contrary, for any point landward of H the freeboard height does depend on the tidal magnitude and so the relationship between freeboard and ice thickness also depends on the tidal magnitude and is clearly not in HE. The value of our mapped parameter was defended above.

"4) the errors in thickness, elevation etc. are qualitative rather than quantitative…" This weakness is fully addressed in the revision by comparing ASAID selected elevations and calculated ice thicknesses with BEDMAP-compiled data sets.

"5) There appears to be a general lack of care and attention to detail throughout the paper…" Hopefully, the reviewer will notice the efforts made to improve the revised text. Fig. 1 has been removed and the discussion focused less on the ASAID project and more on the general value of these data sets. Other notes on figures are welcomed and higher-resolution, better explained figures are provided in the revision.

"SPECIFIC COMMENTS P188, l5. Rignot & Thomas is _10 years out of date…" Removed. The Introduction is recast to downplay the overall motivation of ASAID and justify the importance of these data sets independent of ASAID.

"P195, l16. This is only going to be the case for slow flowing ice…" The general statement is revised to acknowledge this point.

"P198 "Between these points, the hydrostatic line was drawn to reflect the general shape of the grounding line". Doesn't sound very reproducible." We acknowledge the limitations of this technique, but it is an initial estimate that will be refined with time and additional observations. This was motivation for the flexing beam analysis but the agreement is so weak that a reliable method did not emerge.

"P198 "increasing our confidence that a reasonably accurate mapping of this feature". What does "reasonably accurate" mean?..." We restate this to indicate the initial and approximate nature of the H-line interpolation and justify our positional accuracy.

"P200, l24, "where a strong data set had gaps"…" We acknowledge the poor word choice and revise this and similar instances.

"P202 "Errors are likely larger for this class…" Again, error quantification is massively improved in the revision by BEDMAP comparisons.

"L22. "This is probably because the smoothing artifact of altimetric data…" Not "mumbo jumbo" at all, rather the bias is due to smoothing of the elevation field which invariably "rounds off" regions of rapid slope change. Clarified in revision.

"p203 "Surface elevations were sought…" I'm not exactly sure what the objection is here. I acknowledge one error is that the elevation of the H-line varies with tidal phase while the ice thickness does not, so this point is included in our revision. Our Fig. 2 (Fig. 1 in revision) is notional, so it is of little use. Rignot (1996) discusses the grounding zone (of the highly confined Petermann Glacier) in more detail. It states that the region is well modeled by an elastic beam (contrary to the opinion stated by this reviewer) and states, based on the comparison of measured surface elevations and ice thicknesses in his Figure 8, that HE is achieved 2.7 km seaward of his hinge line, more than two kilometers upstream of the point that would have been mapped by Brunt et al. (2010) as the H-line. While the non-coincidence of the various data sets used by Rignot (1996) might alter these conclusions somewhat, it is entirely consistent with this paper to state that an ice shelf is in HE at the H-line.

“L15. “referenced to the WGS-84 geoid”. . .” The reference surface of the DEMs used the ICESat data (GLA06 product) and, thus the elevations included in our data sets are referenced to mean sea level as defined by the EGM96 geoid referenced to the WGS-84 ellipsoid. Some of the DEMs needed to be converted to this common reference, but we received guidance from geoid experts at NASA Goddard to ensure that this was done correctly for the DEMs and for the ICESat data.

“P205. “It is worth repeating here that our grounding line was also checked against the independently identified collection of grounding line points” The statement is relevant in the discussion, but its repetition is omitted in the revision. The more general comparison of grounding lines derived by various techniques was well treated by Fricker et al. (2009) and we agreed with these authors that we would not repeat this excellent and extensive comparative work. See also the comment to this paper by Scambos.

“Section 6. This whole section is bewildering. . .” Validation now appears as three greatly expanded sections on accuracy (of position, of elevation and of ice thickness). Again, this paper is not about mass balance calculations!

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Interactive comment on The Cryosphere Discuss., 5, 183, 2011.