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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.

Anonymous Referee #1

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GENERAL COMMENTS This paper describes a substantial international effort representing a huge amount of work by many groups. The aim appears to have been to map the grounding line, as determined from visible imagery, and the hydrostatic point, as determined by ICESat and then converting this to an ice thickness. Although the data set contains a wealth of valuable information about the nature of the grounding zone in Antarctica I found it difficult to determine what the purpose of the data set and associated paper is and the presentation seemed confused and confusing. For example, presumably the hydrostatic (H) line was converted to ice thickness so that it can be

used for mass budget calculations, as described in section 2, but this is where things become confused.

1) Mass budget calculations do not use the H point for a number of good reasons. As identified in the paper, it can be several kms from point G in Fig 2. Basal melting is a maximum close to G and can be > 50 m/yr but typically unknown. It is clear from Fig 2 that the surface has an elevation the same as point H but at some distance significantly closer to G than H, at a point that Rignot, for example, calls the hinge line. This is just one of many issues with the use of this data set for mass budget estimates.

2) Another issue is the fact that most (say 80-90%?) of the ice discharging from Antarctica does so through ice streams and outlet glaciers where determining the surface expression of the grounding line is particularly problematic, as stated in the paper (p196, l4-5). It is these regions, however, that are the most important to get right for discharge estimates. In addition, a significant number of these glaciers have measured thickness that can be used directly and should have been used for validation purposes here (I will come back to this point later).

3) H is not the point of HE because the ice shelf does not deform elastically (locally over a few kms this may be a reasonable approximation). It is not, as stated by Fricker and Padman 2006, the seaward limit of fringes either. As stated in the manuscript, the grounding zone is a complex region, which has been subject to ambiguous, contradictory and inconsistent definitions in the literature. This is not the place to clarify or consider these issues but it is also not the place to add further ambiguity.

4) the errors in thickness, elevation etc. are qualitative rather than quantitative. This is OK depending on what the intended use for the data is. If it is for discharge estimation, it is not OK as it results in an unknown error in the discharge, which renders the estimate useless. If the authors really want this data set to be used for discharge estimation then they need to undertake a thorough validation using the various measured estimates of thickness across the grounding zone that are available and so on.

For these, and other reasons, I believe the presentation of this data set in the paper is misleading.

5) There appears to be a general lack of care and attention to detail throughout the paper, which gives the impression of a rushed and not well constructed manuscript. Here are a few examples, which are not exhaustive. I will start with the figures.

Fig 1: this is directly from a review paper in Science. It is used to show that "less than 50% of the expected total discharge" was accounted for by R&T 2002. But this figure is ten years out of date. Rignot has published two further estimates since then: Rignot et al 2008 and 2011. I believe Rignot 2008 covers $\sim$ 80% of the ice sheet and Rignot 2011??. Why are the authors using an out of date result to make an out of date point. I find this extremely misleading for anyone who is not very familiar with the relevant literature. The authors then go on to say that these assessments do not include the slow flowing regions. That is true, but they do not state what proportion of the total discharge is accounted for by these slow flow regions? It is therefore impossible to assess how important this is and whether it is in the noise compared to the other uncertainties in mass budget calculations.

Fig 3. These should be labelled a) and b) and include a scale bar and an indication of where it is from. It needs all of these. Fig 4. Ditto. Fig 5. Looks like a screen capture to me. Could be much clearer, especially the legend. Fig 6. Well this is a screen capture. Even at 200% I can't make out the detail in the lower graph. Fig 7. At 200% and with a magnifying glass on the screen I could just make out the very blurred legend on these two figures. They also need an a) and b). Fig 8. Ditto. Fig 9. Can't make out the scale bar on fig "a" at all. The x axis needs re-labelling on "b" Fig 10. I struggled to see what was what in this figure. The legend, if it's needed, is almost legible at 300%. Again, no scale and no location information, either lat, long, x,y or anything. Fig 11. I presume that the x axis is some proxy of distance but it's unclear. This all seems a bit hurried.

Further points regarding lack of attention to detail or discussed under specific com-

ments.

Summary. The analysis and data set in this paper is of value, but the presentation is problematic and substandard. In my view, the authors have two options. They can either put a lot more work and rigor into the validation, error assessment, analysis and corrections applied or they can reframe the presentation of the data set. The former will require considerably more work to be undertaken. The latter will require a reworking of some sections. I recommend the latter and will justify this with some specific comments below. By doing this, the lack of rigor in parts of the analysis becomes less critical. I recommend they remove fig. 1 and section 7 on elastic beam comparison and that they focus the paper on the grounding line deduced by their methods. Their derivation of a hydrostatic line and thickness from this raises more questions than it answers as presented.

SPECIFIC COMMENTS P188, l5. Rignot & Thomas is $\sim$10 years out of date. Cite Rignot 2008 and/or Rignot 2011 and forget R&T 2002.

P195, l16. This is only going to be the case for slow flowing ice. Fast ice will not have such a clear break in slope.

P198 "Between these points, the hydrostatic line was drawn to reflect the general shape of the grounding line". Doesn't sound very reproducible.

P198 "increasing our confidence that a reasonably accurate mapping of this feature". What does "reasonably accurate" mean? It could mean anything, $\pm$10 m, $\pm$10 km. . .?

P200, l24, "where a strong data set had gaps". What is a strong data set? This is unscientific language that appears frequently in the text.

P202 "Errors are likely larger for this class and we assign a standard error of 25–50 m." This statement doesn't fill me with confidence that the errors really are 25-50 m, which in itself is a variation of 100%. I understand the desire to assign errors to classes of data and this is worthwhile but only if the authors can demonstrate that these error estimates are justified.

L22. “This is probably because the smoothing artifact of altimetric data that biases the elevations high at the grounding line where the slope change is most rapid, is less compromising farther out on the ice shelf.” What is the “smoothing artifact”? Sounds like mumbo jumbo. This statement needs a reference and explanation.

p203 “Surface elevations were sought primarily for the purpose of converting surface elevation to equivalent ice thicknesses; this requires the ice be in hydrostatic equilibrium, thus, this conversion is valid along the hydrostatic line (and seaward)” That statement is incorrect. See their own Fig 2 why and see Rignot 1996. There are however other problems with this section that are more serious.

L15. “referenced to the WGS-84 geoid”. This is really worrying. First, there is no such thing as the WGS-84 geoid. There is the WGS-84 ellipsoid which is a reference surface. There is then a geoid chosen that may “typically” be used. This is could be EGM-96, which is itself years out of date and does not include data from GRACE and so on. I have no idea what geoid was used but it is probably EGM-96. The conversion from ellipsoid height to geoid height is extremely important and it is worrying that the authors appear unclear about what they have done here. Additionally, there are many other corrections that they do not discuss at all. These include, ocean tides, inverse barometer correction, mean dynamic topography, all the corrections for ICESat, variations in ocean density and firn/ice density in areas of convergent flow etc. etc. All these issues have been considered by others in the literature working on similar problems (e.g. Padman, King, Fricker. . .).

P205. “It is worth repeating here that our grounding line was also checked against the independently identified collection of grounding line points using repeat GLAS profiles and agreed . . ..” Why is it worth repeating this here? This is neither a quantitative, comprehensive nor representative comparison of InSAR vs. ASAID grounding line locations. This could have been undertaken and would have been interesting but it

was not and I find this statement as misleading as Fig 1. It would be good to have seen a more extensive comparison between InSAR derived GL and the ones shown here. This would have been a useful exercise and, from the comment posted by Rignot, it sounds like this would be possible to do. The authors should be clear, however, that different groups derive InSAR GL in different ways and they do not produce the same result. Rignot, for example, generally uses double differences where he has them. Joughin, on the other hand, uses decorrelation. It is important to understand this and discuss these differences if the authors wish to compare approaches. They did not do this at all and this is another example of either lack of understanding and/or care.

Section 6. This whole section is bewildering. It starts by stating that “Various ASAID participants collected or provided either new or existing data to validate the ASAID products” and then goes on to use one of these from BAS. Why? There are numerous validation data sets that they could have used such as the data recently collected over Pine Island and Thwaites, the CASERTZ data over the Siple Coast, AWI data over Ekstrom, etc. etc. plus those that were acquired for this project. Then the authors compare thickness vs H point. Presumably they are attempting to show that the H point they identify does not have a simple relationship to thickness but why? I am not aware of anyone that uses the H point as the gate for mass budget calculations, which is where I think they are trying to lead the reader here. It was not at all clear.

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