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The Cryosphere
Discussions

Interactive comment on "New gravity-derived bathymetry for the Thwaites, Crosson and Dotson ice shelves revealing two ice shelf populations" *by* Tom A. Jordan

Anonymous Referee #2

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This paper presents new data and analysis and updates the sub-ice shelf bathymetry models of three major outlets of the West Antarctic Ice Sheet. As a technique paper this contribution is nice as it builds on previous work well and is convincing that the perhaps incremental improvements made here are worthwhile to do in bathymetry inversions. That being said, the discussion of error budget is lacking and while effort was made to compare the inversion to realistic observations to obtain a realistic error, this was only done in one relatively small area which I find inadequate for a general comment on uncertainty, especially considering the value is substantially lower than other similar work. The authors do a good job arguing that the basic bathymetry results should be an improvement over previous inversions of this area so it will be important for these

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maps to be available for ongoing numerical ocean modeling work. However, I believe the release of the bathymetry to the modeling community is the main contribution of this paper; in its current form the scientific discussion reads rather speculatively with a somewhat awkward discussion of grounding line retreat that I find largely unnecessary.

Specific comments: Lines 54 to 56: The sentence referencing the Parker-Oldenburg method is misleading as it suggests that the problems discovered by the Cochran and Bell 2012 analysis that led to large disagreements between actual and inverted seafloor depth discussed in Brisbourne et al, 2014 were due to the algorithm used. The Parker-Oldenburg algorithm was never said to be the problem as there are many other more likely factors that may have contributed to the disagreement including platform speed, line-spacing/data coverage and resulting grid resolution, and, most importantly, the lack of explicit constraints on the geological forward model. To avoid misleading the readers, remove this discussion or replace with a full discussion of contributing factors, room permitting.

Line 92: The comparison in wavelengths between OIB and ITGC suggests an instrumentation difference; to clear this up please explain the improvement in resolution between the two campaigns –flight speed, elevation, instrumentation, etc.

Line 95: Please explain what you mean by ""will have little impact". If you mean that not upward continuing to a common elevation could introduce errors when you invert a gravity gridded field that assumes a common elevation then please state this is what you did. Although it seems right that +- 200m will have little impact, please add an estimate of the error introduced. Please also include an estimate of the error introduced for the 5% of the lines flown higher than 450 m and lower than 1500 m and refer to a map in the Supplement illustrating that those lines (or line segments) are not in areas where those introduced errors will impact your interpretations/results.

Line 100: What is the stated resolution and uncertainty of the GOCO3 gravity model? Please explain why the 2 mGal difference you observe more likely to be due to drift in the marine system rather than a regional variation not captured in the GOCO3 model.

Line 181: Your error discussion currently highlights the 23 m contribution from crossover analysis and lack of geological knowledge. However, you have left out estimation of uncertainty due to platform speed, line spacing, and upward continuation. Either expand the discussion to including all sources of error in the budget or focus on the comparison with known bathymetry as you do later.

Line 200: Although I like your error estimation approach (comparing to known bathymetry), I don't think it is adequate to base your error for the whole survey region on only the multibeam area without at least showing that the errors are similar elsewhere; the multibeam area is less than 10% of your rather large survey area. This is additionally suspect as your 100 m error estimate is low compared to multiple other studies that quoted errors based on comparison with realistic bed data. This improvement in standard deviation is not expected considering that you are combining data from different platforms and instruments and your line spacing is coarse in many areas. Please present histograms for other areas to illustrate that both your mean and standard deviations are consistent where it matters –e.g. upstream of each grounding line. It may be helpful to compare your comparisons with known bathymetry to other studies that did something similar; the studies I'm aware of that also did this are: Brisbourne et al. 2014 (+-162 m), Greenbaum et al., 2015 (+-190 m), Hodgson et al. 2019 (+- 175 m).

Line 206: Typo: remove "there" after "where" Line 248: Please replace "typical shelf water" with something more descriptive.

Line 255: Please revise this sentence regarding MCDW supply. The supply of MCDW should be limited more by the depth of the shallowest bathymetry between source of the MCDW and the grounding line, not by the thickness of the water column near the grounding line. Profile C indicates a relatively shallow (500 m) sill which could reduce the supply of MCDW depending on the average thermocline depth which you refer to as 400-600 m. Unless you meant something else by “limit the supply of mCDW”. Later on line 319 you connect weak circulation with thin cavities, is that what you mean by limit the supply? If so, please connect this thought in both places.

Line 273-274: Your comment connecting the slight positive correlation to MCDW being forced onto shallow topography is very speculative and perhaps unnecessary; I recommend removing it otherwise please list other explanations for the correlation.

Line 320 to 330: It strikes me as an intuitive and even mundane result that more recently ungrounded ice shelf areas would have a tighter correlation with bathymetry than ice shelf areas that ungrounded previously. The discussion of this as it stands does not provide enough additional insight to convince me that the older shelf areas don’t simply lose the correlation because they’ve just had more time to spread under their own weight and melt. It is also expected that recently ungrounded areas are the most likely to re-ground under a new flow regime or ocean conditions. I concede that I may have missed a subtle (or not so subtle) nuance, if so, please revise this discussion in a concise manner in your response otherwise I recommend shortening this section and moving the correlation plots to the supplement.

Figure 6 seems unnecessary when you can refer to the literature for this information. I recommend either moving it to the supplement or at least stacking them vertically and placing them next to the thinning map in Figure 7 to save space.

References: Please add standard indentation to improve readability for the next revision.

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