The Cryosphere Discussions

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Comment on tc-2021-287

Anonymous Referee #2

Referee comment on "Inverting ice surface elevation and velocity for bed topography and slipperiness beneath Thwaites Glacier" by Helen Ockenden et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-287-RC2, 2022

This study demonstrates a method for inverting for bed topography and basal slipperiness under ice sheets using more readily observable surface conditions (elevation and velocity), based on earlier theoretical work (e.g. Gudmundsson, 2008). Given the wealth of high-quality satellite-derived data that have become available in recent years, developing (and updating) tools and techniques that can extract even more information out of them is a useful endeavour. This manuscript demonstrates one such example, which, with the supporting code, could be of interest to a range of scientists working on ice sheets, from modellers to those designing targeted field campaigns. However, as it stands the Thwaites case study presented here is not overly convincing and it would be useful to see if the accuracy of the results can be improved when more prior information is included. I outline my major comments below, followed by a few minor points.

The technique presented here seems likely to be most successful in regions where we expect mass conservation to also be the most successful (i.e. fast flowing ice streams) and so the statement in the abstract on line 14 ("… a complimentary technique in regions where those techniques fail") isn't that convincing to me. Similarly, in the discussion, you state that mass conservation is more reliant on good-quality radar measurements (L322) compared to this technique, which I agree with, but I would also perhaps argue that in the regions where MC and this inversion technique perform the best, those radar measurements do tend to exist already owing to their glaciological significance, and in those cases MC perhaps might be the better choice? Although I'm basing this assessment on the results in Figures 6-8 and it would be interesting to see what happens when the prior knowledge fed into the inversion improves.

Following on from the point above, what happens when the average thickness grid resolution is reduced? Various other sensitivity tests have been rigorously carried out as part of the study, and so not including a test for the grid resolution seems like an obvious omission. What is the justification for choosing 50 km x 50 km? How easy would it be to incorporate thickness measurements from radar data into the inversion, as suggested in line 253? Using radar data directly, rather than aggregating the BedMachine product, would help demonstrate the usefulness of this technique as an independent way of obtaining gridded ice thickness.

Do the results shown in Figure 1 suggest that in real world applications any unknown linear bedforms at an angle of less than 15 degrees to ice flow would not be resolved by this technique? This might be a very naïve interpretation, but is that why the Thwaites results are dominated by apparent bedforms that are approximately perpendicular to flow?

There is quite a lot of repetition between sections of the methods and the appendices, and equations in the main text and the appendix are referred to using the appendix reference (e.g. A7, A8 and A12 on line 87). I think the approach taken in Section 2.2 and Appendix C works the best – i.e. in the appendix starts with the relevant equations from the main text (using the same enumeration) and the further equations leading on from these (using the C1 etc numbering system). Could something similar be applied to appendix A and B?

The discussion about the steady-state assumption (L271-282) is weighted towards justifying the assumption at the bed, whereas I would argue that in regions such as Thwaites the assumption is more likely to break down at the surface due to flow acceleration and surface lowering. There are plenty of papers that could be cited here referring to observed changes in ice dynamics.

Related to the steady-state assumption, how important is it to have temporally consistent observations, e.g. surface velocity and elevations from the same year or period? In data assimilation techniques used to initialise ice sheet models, non-contemporaneous input data can result in spurious signals in forward simulations. Would you expect to see non-physical artifacts in the bed topography and slipperiness, if the surface data are not consistent with one another?

Minor comments:

L137-139 (Eqns 26-28) capitalise the subscripts in the last terms?

Figure 1: Consider including an arrow to demonstrate the flow direction (or description in the caption). This would help the reader quickly interpret the angle to flow.

Line 203: "Shallow-ice" --> "Shallow-ice-stream". Elsewhere, hyphenation in this term is inconsistent.

Figure 4: is this something that needs to be tested for each application? E.g. if the grid resolution changes?