Review of “Assimilation of Antarctic velocity observations provides evidence for uncharted pinning points” by Fürst et al.

This manuscript details a continent-wide analysis of Antarctic velocity and ice domain geometry observations, assimilated in an ice sheet model, to infer the locations and influence of pinning points that offer some measure of ice shelf buttressing. An inverse control method is applied that simultaneously calculates the spatial distribution of basal friction and ice hardness (“viscosity parameter”) everywhere. The Shallow Shelf Approximation (SSA) to the stress balance equations is applied for both grounded and floating ice. The analysis focuses on the spatial pattern of the viscosity parameter for select ice shelves as well as the corresponding mismatch between the velocity observations and the model velocity field resulting from the assimilation. The viscosity field and the velocity mismatch field are both used to determine the possible locations of important pinning points in various ice shelves, the location and geometry of which are not present in the Bedmap2 data set (*Fretwell et al.*, 2013) used to constrain the ice sheet/shelf geometry, nor in the locations of grounded ice determined from published differential InSAR analysis (*Rignot et al.*, 2011). The results of this analysis will be beneficial to future ice sheet modeling investigations that rely on accurate representations of ice shelves and their buttressing influence on the ice sheet, as well as to planning flight lines for airborne campaigns (e.g. Operation IceBridge), since these tiny but apparently important pinning points have been overlooked by such campaigns.

Overall I am enthusiastic about this work. It covers some important topics related to ice shelves, model initialization, and the importance of even very small pinning points around the continent. There is a great deal of detail in the manuscript, which is commendable although it was difficult to follow in many places. A lot of this detail is related to topics other than the identification of uncharted pinning points, the subject of which only seems to sneak in toward the end of a long manuscript. I think this is primarily a matter of manuscript organization and presentation. I think that many of my concerns can be addressed with a careful re-write of the manuscript.

General comments

1. Detailed attention is paid to the initialization of the geometry of the ice shelves (Table 1 and Section 2.4.1) and a careful sensitivity analysis is carried out using a range of possible assumptions. This is a welcome contribution, as this topic has not seen much (as far as I can tell) open discussion in the literature on modeling ice shelves. Although it is convenient to work with an “equivalent solid ice thickness” for modeling an ice shelf, the actual geometry of ice shelves determined from observations (e.g. *Fretwell et al.*, 2013) indicates, as the authors point out, that the bulk density of the ice is spatially variable and lower than typically assumed when modeling an ice shelf as a “solid ice” body. Although the model results appear somewhat insensitive to the geometric setup of the ice shelf (according to a single metric of velocity mismatch (RMS), which is somewhat reassuring), in my opinion even more attention could be paid to the spatial details of the inferred viscosity that results from different model setups. There are other implications that result from making different assumptions about how to initialize the density, thickness, and lower and upper boundaries of an ice shelf, especially the vertical stress distribution in the ice column. This is important for thinking about (for example) crevasse formation and propagation, which admittedly is beyond the scope of the present analysis but it strikes me that this careful sensitivity analysis could be more broadly beneficial to future ice shelf modeling studies if the results were presented in a bit more general detail.

2. It would be helpful to show the equations you used to calculate the viscosity parameter for different initializations. You describe the hydrostatically balanced (HB) and damage-corrected (DC), but it’s not clear exactly what you mean. An equation for each would leave no room for misunderstanding.

3. On a related note, are you starting from a full 3-D temperature field for the ice sheet? Or are you specifying B from a depth-integrated temperature? It would be helpful to have a bit more information

about this procedure, as a 3-D B field which is then depth-integrated would be different than a B field specified from a temperature field which is first depth-integrated. How do your starting temperatures compare to the temperature values/fields that other ice shelf studies have used?

4. I don't understand the description of how you use the velocity observations. You say that "velocity components are not interpolated on the model grid but directly used...." This would make sense to me if you were using a regular model grid with vertices that correspond with the velocity observation grid. What does this mean for your irregular mesh though? I'm missing something here.

5. Maybe I don't understand your description, but if you select the T-geometry (using Bedmap2 thickness and enforcing flotation to specify the lower and upper surfaces), are you then using the calculated average ice density from the Bedmap2 geometry, or enforcing a solid ice density everywhere? (e.g. in p. 1473, line 22, what is the "model ice density"?)

6. Other ice shelf modeling studies adjust the ice shelf geometry by lowering the ice surface according to the (estimated or modeled) firn air content before adjusting for hydrostatic equilibrium and using this equivalent ice thickness in the model (e.g. *Khazendar et al.*, 2009, 2011; *Borstad et al.*, 2012). How would this technique fit into your sensitivity analysis for the geometry initialization? It might be worth at least mentioning this technique for completeness.

7. You use the RMS velocity misfit as your metric for selecting which geometry initialization is "best" in your sensitivity analysis. I would be more convinced by some kind of physical rationale for which approach to take. My comments above about the resulting vertical stress distribution in the ice is just one potential consideration, though there may be others. More importantly perhaps is the fact that the RMS will depend on the *choice* of regularization, a choice that is not "objective." The differences in the RMS misfit aren't that large anyway, so I'm not convinced that this is the best way to choose a "best" approach.

8. There seems to be a lot of discussion of results in Sections 3.1 and 3.2 before the Figures get discussed in much detail in Section 3.3. Some of this figure/discussion presentation seems a bit out of order.

9. Section 3.5 needs a better explanation, as I'm kind of confused about what you've done here. I think you mean that you have supplemented the Bedmap2 mask of grounded/floating ice using the dInSAR grounding line data of *Rignot et al.* (2011), which do show some isolated grounded locations in various ice shelves. Is this the case? If so, you can describe it in just one sentence. The four paragraphs of this section really confused me. I don't understand what PIN1, PIN5 and PIN10 mean from the description. Perhaps a schematic diagram could clarify this?

10. Is your L-curve analysis for the entire model domain, grounded plus floating ice? Since you're interested primarily in the ice shelves, I would guess that you would find a different "optimal" value of the regularization term for the viscosity parameter if you conducted an L-curve analysis for just the ice shelves (or just an individual ice shelf). This would then give you different RMS misfit values, which would call into question the heavy reliance on this metric for judging how "good" your results are. I caution against relying too heavily on the velocity misfit for judging results; some physical analysis and intuition should also come into play.

11. How important is it that you infer both the basal friction and the viscosity parameter simultaneously? Since the basal friction doesn't come into play on the ice shelves, how different would your ice shelf results be if you inverted for just basal friction on the grounded ice sheet? Since you are relying on a method (Arthern et al., 2015, which is in preparation) that is not published, you should probably describe this dual-inversion technique in a bit more detail either way.

12. The manuscript and figures go to great length to discuss the inversion results for the viscosity parameter and velocity misfit for numerous ice shelves (Larsen C, Filchner-Ronne, Brunt/Stancomb-Wills) and compare them to the results from previous studies. As outlined above, much of the discussion hinges

around arguing that you produced "better" results by using the RMS velocity misfit metric, which I find questionable. I think that much of this material could be shortened, as it is not really relevant to the later work that is reflected in the title: finding uncharted pinning points. The sensitivity analysis is nice to see in Figure 3 for Larsen C, but otherwise the results for these ice shelves could be shown with a simple and short claim for each that the results are comparable to those from previous studies. Much of the detailed discussion of these results is not so relevant, and shortening it might facilitate readability of the manuscript.

13. I was confused for a while about the way you discussed "additional pinning points" which sounded kind of vague to me, and I got confused with the "uncharted pinning points" that came later (e.g. Section 3.5 is "Introduction of missing pinning points" versus Section 3.6 "Identification of uncharted pinning points"). I think I now understand that the "missing" or "additional" pinning points are areas of grounded ice, not a part of Bedmap2, that are indicated in the dInSAR grounding line data of *Rignot et al.* (2011). I think it would be more descriptive (and appropriate) to label these "pinning points indicated by *Rignot et al.* (2011)" or something similar.

Line-by-line comments and technical corrections

- p 1462, line 8: "reduced to" implies a reduction from some baseline. So the RMS is reduced from what?
- p 1463, line 5: the vast majority *are* confined and *exert* control
- p 1464, line 7: attention has to be *paid*
- p 1464, line 9: *for* granted
- p 1464, line 21: not clear what you mean by a "nonlocal mismatch" here
- p 1466, line 1: unclear what you mean by "alignment of the local flow into the dynamic state of the surrounding ice"
- p 1466, line 6: the constitutive equation links deviatoric stress to strain-rate, whereas deformation is most commonly associated with strain.
- p 1466, line 25: why the lower-bound target of 1.4 km? This seems rather arbitrary. Is there some specific reasoning behind this number?
- p 1467, line 13: why 750 iterations? is this based on experience? do you have any other metrics to confirm convergence?
- p 1469, line 7: unclear what you mean by "changes are most expressed"
- p 1472, line 13: not clear what you mean by "revert this initial bias." Revert to what? What is the initial bias?
- p 1473, line 25: I believe this should be the "Gipps" Ice Rise (also in Figure 3).
- p 1474, line 6: volume may be preserved, but not mass if you are using the wrong ice density. Shouldn't mass be the more appropriate metric to be conserved here?
- p 1474, lines 7-8: I agree that advecting a variable density field presents a numerical challenge, but shouldn't the modeling community address (or at least discuss) this challenge if products like Bedmap2 indicate that the actual density of ice shelves is quite variable spatially?

- p 1475, line 11: the mismatch of 50 $\mathrm{m\,a^{-1}}$ in *Larour et al.* (2005) was not reported as an RMS misfit, rather an "average misfit," but given the fact that the misfit values in this paper were reported with one digit of precision and with plus/minus values, this may have actually been a qualitative assessment of misfit from the figure. For this reason you cannot really use your RMS alone to say that your results are "better" than the previous study. *Larour et al.* (2005) used the original Bedmap data, and this alone might explain the difference.

- p 1476, line 11: a bit of a picky point here, but something that seems to be often overlooked. You compare your "average" viscosity for Brunt/Stancomb-Wills to a previously-reported value, but the average is only meaningful as a measure of central tendency if the distribution (e.g. histogram) of your viscosity values is symmetric. There is no physical reason (that I can see) to believe that the spatial viscosity of an ice shelf should necessarily be symmetrically distributed, in which case the median viscosity might be a better measure of central tendency to compare against other studies.

- p 1476, lines 20-21: the "shelf-wide velocity mismatch of 50 $\mathrm{m\,a^{-1}}$" that you reference here from *Larour et al.* (2014) is a qualitative assessment of the misfit from the figure, not an RMS that you can compare against to claim that your results are better.

- p 1478, line 28: should this be "Shackleton" ice shelf?

- p 1483, line 13: you have not made any statistical comparison of your results against other studies, so you cannot claim that your RMS is "significantly" lower. This is especially the case if other studies did not report the exact same metric for comparison, i.e. the RMS misfit (similar comments above).

- p 1483, line 23: can you quantify how "much softer" the shelf is is when using different ice shelf geometry assumptions? This might be a useful contribution, related to comments above.

- p 1484, line 28: airborne radar could also indicate the geometry of these pinning points, not only in-situ measurements.

- p 1486, line 9: I don't recall seeing the acronym ASS defined, did I miss it?

- Table 1, caption: I don't understand the statement "Avoiding redundancy, complementary information on pinning points is excluded on the basis of how far they are away from grounded ice in BEDMAP2"

- Table 1: the sensitivity of the model results to different geometry initializations is an interesting and valuable contribution here. However, I wonder how these results would compare to the sensitivity for different initial (assumed) ice temperature distributions. In other words, take your temperature guess according to case "TB" and vary it, then see how different your results are. The results of *Borstad et al.* (2012) and *Borstad et al.* (2013) seem to indicate that uncertainty in temperature might lead to just as much variation in inversion results (though admittedly in these studies the temperature was varied ultimately to determine damage, though the inversion results were sensitive to the different initializations). If you're using a temperature field from a model, how well constrained is this field? It might be worth at least commenting on this.

- For all figures that show the velocity mismatch, is this $V_{model} - V_{observation}$ or vice-versa? This should be specified.

- Figure 1: too small make out the pink squares at the print size of the figure. Is it necessary to show the whole continent, since you're only really interested in ice shelves?

- The panels in Figure 3 are small, and it is difficult to make out detail (dashed black and white lines are indistinguishable, I do not see any pink squares as described in the caption, nor a black dashed line for the 100 $\mathrm{m\,a^{-1}}$ isoline).

- Figures 6 and 7: the ice shelves themselves are too small to really resolve in these figures. I do not think it is necessary to show so much grounded ice and open ocean by drawing a giant rectangle that includes the whole region. Can you not zoom in and just show each individual ice shelf? This is where the interesting results are, yet I cannot see them.

References

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