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

This manuscript has been reorganized and many points have been clarified to address the concerns of the initial reviews. In my opinion this represents a large improvement from the initial submission, and the majority of my initial comments have been adequately addressed.

I believe the authors misunderstood one of my comments about the heavy reliance on a single metric, the RMS velocity misfit, as the sole parameter for judging the quality of the inversions. The use of this metric to quantify some inversions as "better" than others, and for comparing the results to previous studies, has now been eliminated. I think this adequately addresses my most serious concern about using a single parameter to judge the results of an inversion. My other concern, however, was about this latter point: using only a single parameter to judge the results of an inversion. I had suggested that some sort of spatial analysis of the misfit might be worth considering, but I did not mean that a higher-order model was necessary. I simply suggested that some type of spatial analysis of the 2D SSA misfit (or other type of physical consideration for selecting the geometry initialization) might also be worth discussing. For example, in Figure 3, where are the largest differences in the misfit between the different approaches? That would be more interesting to me than looking at a table to see which RMS misfit is lower.

This point is my only remaining concern about the manuscript. Since the velocity misfit is used in the identification of the uncharted pinning points, how sensitive (or robust) is this identification with respect to the different geometry initialization procedures outlined in the manuscript? I think that a sensitivity analysis, showing the velocity misfit for one of the newly identified pinning points under the different geometry initializations, could be insightful (probably more so than the sensitivity analysis shown for Larsen C in Figure 3, since no new pinning points were identified here). Other studies in the future might select different geometry initializations for different reasons (like lowering the surface elevation according to an assumed firn air content). In this case, would they be able to locate the same pinning points with the same type of velocity misfits as you have? Or would the misfit be sufficiently different that these pinning points would be missed? One would hope that these pinning points could be robustly identified regardless of the geometry initialization. Can the authors show this or otherwise address this?

Minor comments

In the absence of line numbers, I simply refer to Sections in the following:

- In the abstract, you say that you "present" an approach for assimilating the basal friction coefficient and the viscosity parameter. This is a bit misleading, as it makes it sound like I could read the paper to learn about the details of the inverse method. In fact, you are *applying* an approach that is (soon to be) published elsewhere. A simple phrasing change would reflect this subtle but important distinction.
- 2.2 Mesh: you now report a minimum of 1.2 km for the mesh resolution. Did this change from the 1.4 km in the initial submission?
- 2.4.1 Ice sheet geometry: in the Khazendar references, in fact the ice *surface* (not the thickness) is reduced using a modeled estimate of firn air content. This has much bigger implications for inferring ice thickness by assuming hydrostatic equilibrium!
- 3.1.1 Regularisation: you now report $\lambda_{\beta^2} = 10^3$, whereas it was initially reported as 10^5. Your comments about the robustness of the regularization in the author response letter would suggest that your initial regularization scheme still holds. Is this a typo, or did you change the regularization?