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Interactive comment on "An iterative inverse method to estimate basal topography and initialize ice flow models" *by* W. J. J. van Pelt et al.

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General comments:

RC: This paper presents a simple inverse technique to deduce bedrock topography given an ice flow model, surface mass balance distributions in space and time, and modern surface elevation data. The approach is very simple and iterative, iteratively running the model forward to the present, and adjusting bed topography locally in proportion to the surface elevation difference from observed (ignoring the non-local nature of dynamics). The technique is shown to work well in synthetic tests, with interesting convergence properties. Then it is applied to the Nordenskiöldbreen glacier, Svalbard, where results are validated against limited radar tracks of bedrock topography. The procedure also produces a reasonable spun-up modern state of the ice model that

can be used to initialize future experiments. The paper is clearly written, and proceeds nicely from simple concepts and idealized tests to the more complex real glacier setting and experiments. The method, although simple, is new to my knowledge; its simplicity will make it very amenable to other groups modeling glaciers where bed topography is largely unknown. The paper shows clear improvement over the much earlier perfect-plasticity method (pg. 894-5, Fig. 13). Also, it shows how validation versus radar bed information can also constrain the best-fit till-strength value in the basal sliding parameterization (Fig. 12). Overall the paper is an interesting and valuable contribution, and in my opinion needs only minor revisions, assuming the answer to the last question in point 1 is positive.

AC: We are grateful to the referee for giving useful and constructive comments. We will address all the specific and technical points mentioned by the reviewer below. We will also indicate the changes made in a revised version of the manuscript.

Specific comments:

RC: 1) The paper addresses the possible influence of the initial bed construction on the reconstructed bed (pg. 892-893). This is tested to some extent by Fig. 11 column 3, where the initial bed is lowered by 100 m. But in all runs, even with "unconstrained beds" (Figs. 9 et seq.), the initial bed is still constructed using the GPR radar data (pg. 887, lines 11-13), as can be seen in Fig. 9 for n= 1, where the over-deepening mentioned on pg. 891 line 27 is already in the initial bed. It would be preferable to avoid any influence of the validation data in these runs at all, even in initialization. Could a more radical perturbation to the initial bed be tested, such as constructing it with the GPR data ignored? Would the inverse procedure still produce the over-deepening?

AC: We fully agree it is important to show how sensitive the reconstructed beds are to changes in the initial topography, also to quantify a possible bias introduced by using the radar data in the initial bedrock profile on the accuracy of the reconstructed final bed in comparison to the radar data. Therefore, we performed an additional sensitivity experiment where we start the iterative procedure with a bed derived using the perfect plasticity assumption with a high slope threshold (0.04). The resulting initial topography shows a rather different spatial pattern than in the $\phi = 13^{\circ}$ experiment and lacks prominent features like the overdeepening along the main flow line. We find that the spatial pattern of the final reconstructed bed agrees well with the reconstructed bed in the $\phi = 13^{\circ}$ experiment and clearly includes features like the overdeepening. With the amount of iterations the discrepancy between the reconstructed beds reduces, which is a sign of convergence. Validating the reconstructed ice thickness against the radar data shows only a minor influence of the perturbed initial topography on the mean difference, the RMSD and the correlation coefficient. Hence, it can be concluded a possible positive bias by using the radar data in the initial topography on the reconstructed bed is small and that spatial features like the overdeepening can also be recovered when they are non-existent in the initial topography. These results and the related discussion will be included in section 4.4.1, Figure 11 and Table 1.

RC: 2) This is mostly a comment. As discussed in the paper, surface elevations also depend strongly on uncertain bed sliding properties (here mostly encapsulated in ϕ, the material till strength). This under-determination is handled well, simply by trying different uniform values of ϕ (Figs. 11, 12). But even with uniform ϕ there are probably large spatial variations in sliding due to water pressure p_w in Eq. (1). p_w in turn depends on basal water amount W predicted by the model. Presumably there are regions under Nordenskioldbreen with essentially no sliding, where W is almost zero and/or the bed is frozen, and these regions have significant effects on ice thickness. (Perhaps that is why internal deformation is dominant in the interior, and sliding near the margins; pg. 881, line 7-9). The distribution of $W \sim 0$ or frozen-bed areas can be regarded as another model source of uncertainty in the results, as discussed in general in the paper. But it is an important one, with potentially large effects on ice thickness in the forward model, and thus on the deduced bed topography in the inverse procedure. Another paper (van Pelt and Oerlemans, 2012, referenced here) focuses on these aspects in a synthetic setting. Given that, the single sensitivity test over a range of ϕ (Figs. 11, 12)

seems sufficient, especially since Fig. 12 suggests the results worsen for ϕ outside the range. But it would be of interest to add a figure(s) of the distribution of W, p_w and/or τ_c, mainly to show the areas of frozen/dewatered bed with no sliding. Also, following Eq. (1), it would help to give a bit more information on the treatment of basal water in PISM. The formulae for p_w and W are given in van Pelt and Oerlemans (2012), but they could be repeated here, or at least described verbally.

AC: Agreed! The ice thickness depends on the basal resistance, which is not only a function of the material till strength but also the presence of water at the base. We agree that inaccurate modeling of subglacial water may lead to substantial uncertainty in the distribution of temperate and cold-based ice, affecting the ice thickness distribution. We added a subfigure to Fig. 15, showing the distribution of the basal temperature and discuss the pattern. We find a transition between temperate-based and cold-based ice at around 600 m a.s.l. along the main flow line. The jump in sign of the bed misfit around this altitude (Fig. 13) could very well be related to an inaccurate position of the temperate to cold ice transition. A discussion on this is now included in section 4.4.1. In section 2, the equation for the pseudo-plastic basal shear stress is now given and the dependence of the water pressure on water thickness is mentioned.

RC: 3) The discussion and implementation of the L-curve stopping criterion (e.g., Fig. 13) is valuable. But in the synthetic experiments, which are stopped after n=40 in Figs. 2-4, it would be interesting to know what happens if the iterations are continued much longer. Is there any further reduction of the error from the actual bedrock bump, shown in the last panels, or is there little change after n=40?

AC: Some reduction of the misfit can be expected beyond n=40, but likely the misfit will converge to a very small but nonzero value. We hypothesize there is a numerical limitation to the detail in the bed that can be recovered because beyond some point very small bed adjustments may no longer affect the surface height due to numerical rounding/diffusion. A sentence has been added on this: "Nevertheless, we hypothesize there is a numerical limitation to the detail in the bed that can be recovered because beyond some point very small bed adjustments may no longer affect surface heights due to numerical rounding/diffusion." In real applications, like in section 4, this is however irrelevant, since the iterative procedure is terminated by means of the stopping criterion way before a possible numerical limit is reached.

RC: 4) The importance of prescribing realistic time history of climate forcing is demonstrated (Fig. 11 column 4), from 1300 AD to modern (from 1598 AD for precipitation), as described in section 4.3. But the climate prescription for the earlier part of the runs (500 AD to 1300 AD) seems quite casual in comparison (pg. 886, line 6-7), set constant to the mean after 1300 AD. Presumably this is because there is no comparable data available before 1300 AD. But could a sensitivity experiment be done to show whether different but still reasonable choices of climate for 500-1300 AD significantly affect the results?

AC: We performed an additional sensitivity experiment with perturbed temperature for the period 500 AD to 1300 AD with air temperature lowered by 1 K. The resulting bed and its discrepancy relative to the $\phi = 13$ experiment is now shown in Fig. 12 (2nd column). These results illustrate the relative insensitivity of the reconstructed bed to the prescribed climate during the spin-up period. In Table 1 associated values of the misfit w.r.t. the radar data are now included. A brief discussion of these results is included in section 4.4.1. In response to the second referee, we also performed an additional climate perturbation experiment with temperature lowered by 1 K after 1300 AD. As expected the reconstructed bed is more sensitive to perturbations since 1300 AD than during spin-up.

RC: 5) pg. 899, line 10 (and abstract line 18-20): The discussion of applicability to “larger sets of glaciers and ice caps” could be amplified. As it is, the concept is not very clear. Does it mean that a large number of glaciers could each be treated separately by the procedure as in the paper, and the individual results summed? Or that the input properties (surface profiles, surface mass balance) of a large set of glaciers be averaged, and the procedure applied once to that, yielding just one regional result?

AC: Given a surface topography dataset, a climate forcing and an ice flow model, the approach can be applied to individual glaciers. By "larger sets of glaciers and ice caps" we mean the summed results of application of the approach to individual ice masses. This is now more explicitly discussed in section 5: "As this method can potentially be applied to estimate the thickness distribution of larger sets of glaciers and ice caps, e.g. to estimate ice volume, one could decide to use a computationally inexpensive ice flow model at the expense of detail in the reconstructed bed. Application to a set of ice masses could involve application of the inverse approach to individual glaciers, given a surface height data set, a climate forcing and an ice flow model, to estimate the summed volume contained in a set of individual ice masses."

RC: 6) The recent paper by De Rydt et al., The Cryo., 2013, could be mentioned, which supports the theoretical results in Gudmundsson (2003) and Raymond and Gudmundsson (2005) with field data analysis; the latter 2 papers are referred to several times here.

AC: Agreed. We now refer to the paper in section 2.2.

Technical points:

RC: pg. 880, line 18-19: Why are there multiple layers in the bedrock? Does the model simulate vertical heat transfer and temperature profiles in the bedrock?

AC: Yes, indeed. At the bottom of the bedrock model, a constant geothermal heat flux is applied and within the bedrock enthalpy evolves diffusively. Ignoring heat transport within the bedrock would have a very minor effect on the results.

RC: Vertical bed deformation due to ice loading is presumably neglected here, as appropriate for small glaciers. But it could be significant for larger ice masses and longer time scales. If the model includes a bed-deformation module, in principle it could be included and the inversion procedure would operate on the prescribed ice-free equilibrated bed.

AC: This is indeed neglected here. We agree this could be significant for application to larger ice masses on longer time-scales. We expect that including bed elasticity would not limit the functionality of the approach although this has not been tested.

RC: pgs. 882-883: Specify the bump size for the reference bed in Fig. 3, and the two sizes in Fig. 5 (like 150 m for Fig. 2, on pg. 882 line 1).

AC: Bump sizes are now mentioned in the captions.

RC: pg. 885, line 25, and/or Fig. 6 caption: Along with the names of the exposed-rock areas, say that is what they are (or in other words, such as mountains, nunataks?), for readers unfamiliar with “fjellet”.

AC: Corrected.

RC: pg. 886, lines 22-27: The descriptions of how the procedure handles discrepancies between model vs. observed ice perimeters (around enclosed exposed-rock areas), and also how the divide is handled, are a little unclear. In line 22, for points in the immediate vicinity of the divide, is “nearest-neighbor interpolation” from points inside the glacier domain, or outside (with the latter set as on pg. 887, line 16-18)? And does “immediate vicinity” (line 21) mean exactly on or touching the divide? For (1) and (2) in lines 24 and 25, “interpolating” between or from what, and is this still nearest-neighbor or other type of interpolation? Presumably these steps would also be taken for other glaciers with land-terminating margins (not the case here, where the calving front is held static through the runs as mentioned on pg. 887, line 1-2).

AC: The related sentences in section 4.3 are reformulated to clarify the content

RC: In later sections where sensitivity results are compared to the standard run with ϕ =13, the term “bed misfit” is confusing, e.g. Fig. 11 and 15 captions. It is a sensitivity to a changed model parameter, not a misfit from observations. (Except in Fig. 5(d) caption, where the term is appropriate - the misfit from the known synthetic bed).

AC: Agreed. In the later sections, "bed misfit" has been replaced by "bed discrepancy"

if appropriate.

RC: pg. 888, line 22: Probably “paragraph” should be “section”.

AC: Corrected.

Interactive comment on The Cryosphere Discuss., 7, 873, 2013.