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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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Summary:

RC: This paper presents a method for recreating the bedrock topography of a glacier given that (a) the surface topography is known for a specific point in time, (b) the mass balance history can be estimated for a sufficiently long time frame, and (c) a model for the ice dynamics is available. The method consists in running the ice-dynamics model forward in time, and iteratively adapting an initial guess of the bedrock in order to minimize the mismatch between modeled and known surface geometry. The merit of the paper doesn't lie much in the application in three dimensions of a methodology which is already found in the literature (see Heining, Phys. Of Fluids, 2011, or Michel et al, Inv Proc., 2013), but in the clarity of the presentation, making the method very

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easy to understand. If this is sufficient for making the paper worth publishing, shall be decided by the editor.

AC: We are grateful to referee Daniel Farinotti for giving useful and thorough comments about the manuscript. As a first response we believe it is appropriate to first emphasize the most important new elements of this study. First of all, we do not (and do not want to) claim the method to iteratively correct the bed has not been used before, since there have been several studies within the field of glaciology using such an approach to reconstruct basal topography along a flow-line transect. This includes studies by Oerlemans (2001), Leclercq et al. (2012) and Michel et al. (2013). These are referred to in both the Introduction and the Model Method section (section 2.2). The novelty of the work therefore does not lie in introducing the correction method, but rather in the application of the correction method in three dimensions, testing the functionality in idealized steady-state experiments and showing a step-by-step application of the approach to a real glacier geometry given noisy data from a DEM and an actual time-dependent climate forcing. The synthetic experiments illustrate successful application of the approach in 3D, which indicates that accounting for the cross-flow response of surface heights to bed perturbations in addition to the along-flow component, is not a limitation for convergence. We illustrate in section 3.1 that, despite the non-locality of the surface height response to bed adjustments, recovered bumps gradually attain the right size and move into the right position. This is explained by the interaction of the surface misfit and the adjusted bed in consecutive iterations. In a flow-line context Michel et al. (2013) argued that the non-locality of the surface response could be a limiting factor for convergence. The synthetic experiments give useful new insights in the sensitivity of the method to the chosen relaxation factor, initial topography, mass balance forcing and bump dimensions. We believe it is very relevant to study and present these idealized results prior to applying the method to a more complex case study with actual data. The application to Nordenskiöldbreen is novel in the sense that it is the first time such a correction method is successfully applied to distributed DEM, while accounting for time- and height-dependence of the surface climate forcing (this includes accounting for height feedbacks of the mass balance and surface temperature). Given uncertainty in the DEM, a reasonable way of stopping the inverse procedure is desired before the reconstructed bed is contaminated severely by unrealistic adjustments of the bed. The application of an L-curve stopping criterion uses the principle that beyond a certain amount of iterations bed adjustments no longer lead to a significant improvement of the surface height and provides a new way to deal with data noise. We believe the stopping criterion provides a useful alternative to methods like the "discrepancy principle", the "recent-improvement principle" and smoothing of the solution. The application to Nordenskiöldbreen may serve as a manual for future applications and importantly shows how the approach can be used as an initialization procedure for forecasting experiments. It is further illustrated that given scarce bed data, e.g. from GPR, the approach can indirectly help to constrain basal parameters, like the mean material till strength. Sensitivity experiments explore the robustness of the approach and help to quantify uncertainties related to the chosen initial bed profile, material till strength and the climate forcing. Validation against GPR data indicates that much of the bed variability can be resolved and that the approach performs better than the perfect-plasticity assumption. Finally, we discuss sources of uncertainty and discuss possible uses of the method, e.g. to estimate ice volume contained in ice masses. Altogether, we believe the paper contains numerous new elements and could be of substantial use for future studies requiring distributed basal topography/ice thickness and/or a method of initialization. Next, we will address all the general, specific and technical comments raised by the referee.

General comments:

RC: 1) Very clear presentation... The presentation of the paper is very clear, and makes the presented concepts very easy to understand. This is – I believe it's worth to say – well done!

AC: Thanks!

RC: 2) ... of a method that is not new. Unfortunately, the method is not new in the literature, which relativizes the importance of the contribution. At page 879 line 11-13 the authors correctly acknowledge that “A similar correction method has previously been applied in a flowline context in Oerlemans (2001), Leclercq et al. (2012) and Michel et al. (2013) [...]” but actually, looking at Equation (30) in Michel et al. (2013) for instance, one can easily appreciate that the correction is not “similar” but rather “identical”. I think that for correctness, this should be emphasized in the paper, avoiding the impression that something pre-existing is sold as “new”.

AC: In our first comment we outline that the novelty of the work does not lie in introducing the correction method. Since we want to avoid any confusion that we might be the first ones to use such a correction method, we added the following to the introduction: "This study builds on previous applications of such a correction method in a flow-line context (Oerlemans, 2001; Leclercq et al., 2012; Michel et al., 2013) and extends the analysis to three dimensions. The real glacier application to Nordenskiöldbreen, with added complexities related to the presence of data errors and climate variability/feedbacks, serves as step-by-step manual for future applications recovering distributed basal topography and/or initializing ice flow models." We further replaced the word "similar" by "the same" and added a reference to Heining (2012) in section 2.2: "In the field of fluid-dynamics Heining (2012) studied gravity-driven film flow over an undulating bed and used the correction method to successfully recover basal topography in idealized flow-line experiments." Finally, throughout the paper sentences are reformulated to more explicitly refer to the 3D character of the experiments.

RC: 3) Why such a complex case study? The real-case application to Nordenskiöldbreen is intriguing, and it is certainly necessary to present a case study that breaks out from the synthetic experiments. But why such a tricky case? Nordenskiöldbreen has a polythermal structure and even a calving front, and one is likely to run against all sorts of complications that can be avoided by considering a purely temperate, non-calving glacier. In particular, this would better allow to distinguish which mismatch between reconstructed and actual bedrock geometry is due to deficiencies of the inverse procedure, and which to deficiencies in the selected ice-dynamics model. At the moment, all discrepancies of the final bedrock could easily be attributed to the ice-dynamics model...

AC: One reason to investigate a relatively complex case is to explore the limits of possible application of the inverse approach. The case of Nordenskiöldbreen is interesting, because it is polythermal and hence contains both cold-based and temperate-based ice. This implies that different stress equations, with different bed to surface transfer properties, apply in different parts of the glacier (dominant SIA in cold parts, SSA in sliding parts). These results, as well as the synthetic experiments in section 3, demonstrate convergence of the method for a polythermal ice mass. Furthermore, previous surface modeling work on Nordenskiöldbreen (Van Pelt et al., 2012) in combination with longterm records of precipitation and air temperature (Divine et al., 2010) allow us to prescribe a surface climate forcing, which varies in time and depends dynamically on the surface height, thereby accounting for height-feedbacks of the mass balance and surface temperature. The availability of GPR data is useful for validation of reconstructed beds. As we discuss in the final section, indeed much of the uncertainty in the final bed comes from errors in modeling the ice dynamics, and in particular the conditions near the base. A second source of error comes from the inverse approach itself and results mainly from the early termination of the inverse method. The latter is affected by the degree of error in the surface height dataset and is therefore independent of the quality of the ice-flow model. For now we are mainly able to quantify the total uncertainty by validating reconstructed beds against GPR data. However, sensitivity experiments with varying till strength, initial beds and climate forcing do give an idea of the uncertainty related to these individual components.

RC: 4) The concept of “validation” In the Nordenskiöldbreen case-study, “validation” of the method is performed by considering the difference between reconstructed bedrock and GPR measurements. However, at page 887 lines 11-13 it is clearly stated that “All experiments start the first iteration with an initial bed generated through interpolation between known bed heights from the GPR data [...]". How can the results be validated against something which is already contained in the initialization? Probably I understood something wrong, but at this stage, the "validation" seems not admissible. A far more realistic initial guess would be given by an uniform ice thickness, derived from volume-area scaling for instance. This is certainly the most realistic scenario in the case that the methodology is applied to "a set of glaciers", as the authors suggest to do already in the abstract.

AC: First of all, we would like to stress that only the initial bed at the start of the first iteration in the iterative procedure is constructed using the GPR data along a track and interpolation (resulting in a smooth initial bed lacking much detail). Nevertheless, as also discussed in our response to the second referee, we would still like to quantify a possible bias in the reconstructed bed introduced by the use of GPR data in the initial bed in the very first iteration. 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). This initial topography (Fig. 14c) 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 (Fig. 11, 4th column) 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 of the reconstructed bed w.r.t. the $\phi = 13$ experiment reduces, which indicates 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 (Table 1). Hence, it can be concluded the 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. More discussion on this has been included in Section 4.4.1.

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RC: 5a) What is the influence of the imposed climate? In the Nordenskiöldbreen case, for example, the past climate history can be reconstructed only approximately. Who tells that this reconstruction is correct? What would happen to the reconstructed bedrock if this climate would be altered systematically, or with some random noise? At the moment, the sensitivity experiment presented at page 893 only considers "constant climate" as an alternative, and concludes that "The relatively large and systematic bed misfit found when ignoring the time-dependence of the surface forcing indicates the relevance of accounting for temporal variability when recovering basal topography". It would be interesting having some quantitative results showing the effect of a climate history reconstructed with 1K-bias in temperature, or 20% in precipitation, or similar...

AC: Indeed there is uncertainty involved in the climate reconstruction and the sensitivity of the reconstructed bed to these uncertainties is to some extent tested by repeating the $\phi = 13$ experiment with a constant climate forcing since 1300 AD. We agree that it could also be useful to apply a temporally consistent perturbation and therefore we performed an additional sensitivity experiment with temperature perturbed by -1 K since 1300 AD. Associated results are discussed in Section 4.4.1 and shown in a new Figure 12, which now collectively shows the results of all climate sensitivity experiments. Furthermore, also in response to the second reviewer, we performed an additional sensitivity experiment with perturbed temperature between 500 AD and 1300 AD, to illustrate the relative insensitivity of the reconstructed bed to the climate forcing over this period (Fig. 12, 2nd column). Table 1 includes correlation and misfit values of the climate perturbation experiments.

RC: 5b) What is the effect of an altered initial surface and bedrock geometry? Lines 19-20 at page 892 conclude that "regardless of the initial bed, after many iterations the reconstructed bed always seems to converge to a similar bed profile" but this seems hard to believe: Probably there is a point starting from which the solution does not converges anymore (what's for example if the initial estimate is "uniform 0 ice thickness"?). And the same is very likely true for the initially prescribed surface. Moreover, similarly as for climate, it would be interesting to know about the effect of non-uniform perturbations in the initial estimate, e.g. a severely overestimated ice thickness in the flat parts, and a underestimated one in the steep ones. Or even, one may use the result from the perfect-plasticity assumption as an initial guess, etc.

AC: With "initial bed" here we mean the bed at the start of the first iteration of the inverse procedure rather than the bed at the start of every model iteration (with every iteration running from 500 AD to 2007 AD). After many iterations the influence of the chosen bed in the first iteration decreases and reconstructed beds (starting from different initial beds) become more and more similar. Due to preliminary stopping of the inverse procedure (after ~23 iterations) there may still be some influence of initial bed on the reconstructed bed as discussed in section 4.4.1 and in the response to comment 4). This influence will likely be somewhat larger if one would start the inverse procedure with a very strongly perturbed initial bed. We however believe that there will also be convergence when starting from extreme beds; starting from a very odd basal topography leads to odd surface heights and strong bed adjustments in the first few iterations. These adjustments will however quickly move the bed towards more reasonable values after which the surface height also attains a more reasonable shape. An example of this has been shown in the synthetic experiments (starting with a bed equivalent to the known surface height). In line with the reviewer's suggestion and as mentioned in our reply to comment 4) we did perform an additional sensitivity experiment where we start the inverse procedure from a bed derived using the perfect-plasticity assumption (Sect. 4.4.1).

The initial surface at the start of every iteration in 500 AD is set to present-day surface heights (in 2007) from the DEM. Starting every iteration with a very odd initial surface would have some influence on the final surface in every iteration and in the end on the reconstructed bed. This effect is indirectly studied with the additional perturbation experiment in which the temperature between 500-1300 AD is altered leading to a perturbed surface in 1300 AD.

RC: 5c) In the presentation “PISM” is used as an ice dynamics model. How important is the level of sophistication of the model used. In the discussion section the author show how using the perfect plasticity assumption instead of PISM deteriorates the ability of recovering the bedrock. But in between there is a whole range of other possible models. The author stress the problem of “over-fitting” (i.e. the introduction of unrealistic noise in the reconstructed bedrock if iteration is continued for too long) several times in the text, but what’s about “over-modeling”? I.e. what artifacts are introduced by the fact that a given model tries to describe phenomena which are hardly understood? In the presented case one could easily question the accuracy with which the geothermal heat flux, the englacial temperature distribution, or the local water production at the glacier bed can be reproduced by the model. How likely is that these processes introduce additional noise in the recovered bedrock?

AC: Since much of the uncertainty in the reconstructed bed can be ascribed to errors in modeling ice flow, one can expect that the level of sophistication of the ice flow model plays a substantial role in the accuracy of the bed reconstruction. The level of sophistication of the model used should depend on the application. For the Nordenskiöldbreen case it is desirable to use a model that at least accounts (in a general sense) for spatial variability in basal shear stress. This indeed depends on both the englacial temperature distribution as well as local water production, geothermal heating and the material till strength. The current basal model is already relatively simple, which reduces the amount of poorly-constrained parameters, but also implies a lack of detail in the computed basal shear stress. However, this is inevitable given the lack of measurements of subglacial processes. On the one hand, further reducing the complexity of the basal model likely implies less realistic spatial variability in the yield stress and in the end less accurate reconstructed basal topography. On the other hand, further extension of the basal model, for example by implementation of a more sophisticated water transport scheme, only leads to more accurate bed estimates if associated model parameters can reasonably be constrained by observations. Finally, since modeling uncertainties seem to be mainly contained in simulating basal processes, we do not expect application of a higher-order or full-stokes model would lead to significant improvements of the bed accuracy in this work.

Specific comments:

RC: P 874, L 18-21. These sentences should be moved to the “discussion and conclusions” section. This is an outlook, and not a result of the paper, and as such it shouldn’t be in the abstract.

AC: Agreed. We removed these sentences and extended the discussion about this topic somewhat.

RC: P 875, L 22. Consider adding the reference to Huss and Farinotti, JGR, 2012, that used a very similar approach.

AC: A reference to this paper is now included in the Introduction

RC: P 876 L 4: “given a set of surface height data”. At this stage it is unclear if “set” refers to two points in time. One may think that you need one surface DEM for initializing the model, and one for assessing the difference between modeled and observed surface. Please clarify.

AC: Changed to "given a map of surface height data".

RC: P 877 L 19: “fixed geothermal heat flux”. What’s about this flux in the real application? It is not mentioned anymore.

AC: A constant geothermal heat flux of 0.042 W m^{-2} is prescribed at the lower boundary of the bedrock model in all experiments. The value is now mentioned in the text.

RC: P 879 L 9-11. Can you give a hint at this stage on how the magnitude of the relaxation factor is determined?

AC: Yes, we changed the sentence to: "The magnitude of the bed correction in response to a surface discrepancy hence scales linearly with the relaxation factor K,

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which needs to be chosen small enough to avoid instabilities due to overcompensation of the bed, and large enough to speed-up convergence of the approach."

RC: P 879 L 26-27. What do you mean with “reference bedrock and surface height profiles”? Do you mean “initial” instead of “reference”? Or what is “reference” referring to? And why “profiles”. Weren’t you working on 3D? Then “geometries” would be more appropriate... RC: P 880 L 3-4: That sounds a bit awkward. Does it mean that you start from a flat bed and surface, and by applying some climate, you construct an artificial surface? What you are trying to do is constructing a surface and a bed that you want to re-construct with your method later, isn’t it? This is not clear at this stage. Clarify.

AC: We agree this needs some clarification. The related paragraph has been reformulated.

RC: P 882 L 1-4: Please sate a local mean ice thickness in order to put the values of 10 and 150 m into context.

AC: Done.

RC: P 882 L 14 ff: So what you do is starting with the surface you want to have at the end, right? Or what is the “reference surface”? Please clarify.

AC: We actually start every iteration without ice, which should have been stated more explicitly (it was only mentioned for the initial run). This is now mentioned more explicitly in the introduction of this section. We further reformulated the sentence in question to: "An initial model run produces reference bed and surface heights shown in Figure 3a and 3b. The inverse procedure, starting from a flat bed underneath the ice, uses the misfit between the modeled and reference surface heights to iteratively update the bed.".

RC: P 883 L 8-10: Are you saying that you start from “uniform zero ice thickness”? Please clarify.

AC: No, it implies that in this extreme case the bed at the start of the iterative procedure

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equals the reference surface. This investigates an extreme case where in the first few iterations the surface height will be strongly overestimated (as the bed is way too high). We reformulated some sentences to clarify this: "The sensitivity of the approach to a different bed profile at the start of the first iteration is investigated by performing an additional run in which the initial bed is set to the reference surface height. This will lead to a significant overestimation of the surface height in the first few iterations."

RC: P 883 L 11-12. Well, making this statement after two trials only doesn't sound very convincing...

AC: We believe the severity of the initial bed perturbation does make it quite convincing that also for other (less severe) perturbations of the initial bed the method will converge to a similar basal topography as well. Additional experiments with perturbed initial beds are performed in the real application.

RC: P 883 L 16-17.: Please discuss how it is possible that you recover the right bed with the wrong mass balance. Is this not in contradiction with what you say later?

AC: Before applying the new mass balance distribution in the inverse procedure, we constructed new "reference" surface and bed profiles consistent with the new surface forcing. Hence, the mass balance is not "wrong". To avoid confusion, we added a note on this in the text.

RC: P 884 L 26: Have you a reference for these GPR measurements?

AC: No reference is available, since this is the first time the data are used. Some additional information has been added to the text in section 3.1.

RC: P. 885 L 4-5: What is a ”principled stopping criterion”? Why “principled”?

AC: We decided to leave out the term "principled".

RC: P 885 L 7: At this stage the question is unavoidable: What data have you got since 1300AD? You explain it later, so I wouldn't mention the data here.

AC: We believe it is useful to mention the use of the time-dependent climate forcing at this stage. To avoid the question, we added a reference to section 3.3 where the climate forcing is discussed.

RC: P 885 L 24: What do you mean by “freely evolve”? There is Lomonosovfonna on the one side, the sea on the other. . . What are the boundary conditions there?

AC: This becomes clear in the next subsection, where the application of a "force-to-thickness" mechanism outside the Nordenskiöldbreen grid is discussed. To avoid confusion at this stage we left out this sentence here.

RC: P 886 L 6-7: Same question as before: Where are the climate data coming from?

AC: We agree it is a bit odd here to refer to the climate data before presenting them. Therefore we moved the "climate forcing" section to subsection 4.2, whereas the "setup" section is now subsection 4.3.

RC: P 886 L 1-11: The initialization is not completely clear to me: You have an initial surface (which one?) and an initial bed (which one?), assign it to 500AD and run the model with constant climate until 1300AD. Then you impose a climate for 1300-2007 (The question “where are the data coming from?” is still not answered) and run the model for that period. Why such a long period? Is it necessary? Would you get a different bed with a different climate? Who tells that the climate is right?

AC: As mentioned in an earlier comment, the initial surface in every iteration in 500 AD is set to surface heights from the DEM. The initial bed in 500 AD is iteratively adjusted at the end of every iteration (in 2007) and is therefore different at the start of a new iteration. We start already in 500 AD, since we aim to have an ice mass with properly initialized thermodynamics and steady-state geometry in 1300 AD. A period of 800 years is sufficient to achieve this (the response time is typically a few hundred years) After 1300 AD, we prescribe a varying climate and sensitivity experiments with perturbed climate indicate that the surface heights in 2007 do depend significantly on

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the prescribed climate. Hence, the reconstructed bed is significantly influenced by the chosen climate forcing. By performing a sensitivity run with a constant climate forcing, we illustrate that not accounting for the transient nature of the glacier (glaciers are never in steady-state), significant errors in the reconstructed bed can be expected.

RC: P 886 L 13-14: Not sure what you mean with “which involves mass balance adjustments”. Does your model impose a mass balance outside the domain shown in Figure 6? Please clarify. P 886 L 15: What are “severe time-stepping restrictions”??

AC: We reformulated the related sentences and hope the principle is more clear now: "A “force-to-thickness” mechanism is applied there, which involves prescription of an artificial mass balance required to force the surface height to approach present-day (DEM) values. The resulting slightly smoothed surface outside the grid resembles present-day surface heights and avoids severe time-stepping restrictions, i.e. very short model time-steps, related to steep surface gradients."

RC: P 886 L 24-25: Points “(1)” and “(2)”: “Interpolated” from what?

AC: It is now more clearly formulated that interpolation is done between iteratively updated bed heights on the grid and fixed bed heights outside the grid: "Interpolation involves application of a spring metaphor to estimate unknown bed heights by interpolating between surrounding bed heights from inverse modeling (on the grid) and/or fixed bed heights (outside the grid)."

RC: P 886 L 26-27: What are the “reference profiles”? You did not define them. Do you mean the initial geometry?

AC: With the reference profile, we mean the present-day surface heights from the DEM. Some interpolation of the bed is needed when the ice cover in the model is slightly smaller than in the reference surface (DEM). To clarify this, we reformulated the sentence to: "This includes: 1) interpolating the bed for grid points that are directly connected to ice-free grid cells, 2) interpolating the bed for modeled ice-free grid cells which are ice-covered in the reference surface from the DEM, 3) not adjusting the bed for grid cells which are ice-free in the reference surface and ice-covered at the end of a model iteration."

RC: P 887 L 1: Does it mean that you just "chop-off" ice at the current calving front? Starting from 1300AD? Isn't it inconsistent forcing a model with a correct climate but an unrealistic geometry? What is the effect on your estimated bedrock?

AC: Indeed the ice is not allowed to extent beyond the current calving front position during the runs, which leads to uncertainty in near-frontal velocities and geometry further back in time. Although we do not expect this to have a major impact on the reconstructed bed, this effect has not been quantified. Actually, the only way to reasonably quantify this uncertainty is to model in more detail the movement of the calving front, which is infeasible at the moment.

RC: P 887 L 11-15: As said: You can not re-use these data for validation if you use them in the initialization process! I would suggest starting from "uniform (not constant!) ice thickness" for the entire glacier.

AC: As mentioned in our response to general comments 4) and 5b) we performed an additional sensitivity experiment with a perturbed bed which is not affected by the GPR data, These results indicate that including GPR data in the initial profile at the start of the first iteration has only a minor influence on the reconstructed bed. The relevance of the choice of the initial bed is now more elaborately discussed in section 4.4.1.

RC: P 887 L 21: What's your cut-off for "unrealistic"?

AC: This sentence has been moved to the second paragraph of section 4.3 and now also mentions the slope threshold used to avoid unreasonable ice thicknesses.

RC: P 888 L 10: Point at Figure 10.

AC: We believe it is not appropriate to refer to Fig. 10 before actually discussing the results shown in this figure.

RC: P 888 L 11: NO! You used these data already. They are certainly NOT an “independent source for validation”!

AC: By ’independent’, we refer to the GPR data not being used in the stopping principle, rather than the use of GPR data in the initial topography. We mention this, since GPR data could be useful to stop the iterative procedure right at the moment where the misfit between modeled and observed bed heights is smallest. We however want to show a stopping principle that can be used regardless of the presence of (scarce) bed data. To avoid this confusion we reformulated the sentence to: "Note that an alternative stopping criterion could be to stop the inverse procedure when the misfit between modeled and observed bed heights minimizes. With the L-curve criterion we illustrate a stopping principle that can be applied in presence and absence of (scarce) bed data."

RC: P 888 L 23: What is “winter temperature”? Air temperature? Surface temperature? Englacial temperature? For a given point? Spatially distributed?

AC: Reformulated to: "Given a winter air temperature reconstruction at Svalbard Airport near Longyearbyen..."

RC: P 888 L 24: Where do you get annual accumulation starting from 1300AD from?

AC: This is explained two paragraphs later.

RC: P 889 L 8-9: Give a formula for what’s happening here.

AC: We reformulated the sentence to make the procedure somewhat more insightful: "We compute the mean ratio of trends in winter temperatures and annual mean temperatures over the instrumental period (since 1912) and subsequently use this ratio to convert the winter temperature record, presented by Divine et al. (2011), into annual temperature estimates back to 1300 AD."

RC: P 889 L 10: Say a word on how the time series was derived in Divine et al. (2011).

AC: Added some information on the use of a composite $\delta 18O$-record in Divine et al.

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(2011).

RC: P 889 L 25ff: Give a hint on how the mass balance is computed and what parameters are involved.

AC: We added a sentence on the coupled model: "A coupled distributed energy balance - snow model, presented in Van Pelt et al. (2012) solves the surface energy budget and simulates melt water percolation, refreezing and runoff in the firn pack."

RC: P 890 L 3-5: State this earlier! I.e. in the previous page!

AC: Done.

RC: P 890 L 10-14: Is this done for producing Fig 8 or from the data that are shown therein? Clarify.

AC: As discussed in the next paragraph, the diagrams were filled (by curve fitting and interpolation) to produce look-up tables for the mass balance and subsurface temperature for every altitude and climate perturbation. Fig. 8 depicts such an (interpolated) look-up table. We reformulated the sentences to avoid confusion.

RC: P 890 L 15-18: Also this needs to be stated before!

AC: Note that this is about the dynamical model and not the coupled surface mass balance model. We therefore believe this is the right place to discuss this. Nevertheless, we did reformulate the sentences somewhat to clarify the content.

RC: P 892 L 29: No idea if “on average 11 m” is “small”. No plot shows the ice thickness and 11 compared to 100 doesn’t looks too small. . .Moreover, please state RMS or the average absolute deviation.

AC: The 11-m difference is much smaller than the 100 m initial difference, which indicates that reconstructed beds seem to converge to similar profiles. As discussed in the text and earlier on in this Author Comment, due to early termination of the iterative procedure, the final bed is still (slightly) affected by the chosen initial bed at the start of the first iteration. It is now stated that the 11 m difference is an absolute deviation.

RC: P 893 L 1ff: As stated in the general comments: There will almost certainly be a point starting from which you are not able to reach convergence (“uniform zero ice thickness” for example). Moreover, some sensitivity experiment with non-uniform perturbations would be insightful.

AC: As indicated in the response to the earlier general comment, we did not encounter divergence in any case. As mentioned, we believe there may have been a misunderstanding here about what is meant by initial bed. In any case, in the synthetic experiments we did start a sensitivity experiment with a zero initial ice thickness at the start of the first iteration, which still let to converging results with very strong bed adjustments in the first few iterations. In the real application, sensitivity experiments with a perfectly-plastic initial bed and a shifted initial bed (-100 m) both indicate converging results.

RC: P 893 L 15: Why “sliding velocities”? Also creep deformation will increase.

AC: Agreed. We replaced ’sliding velocities’ by ’ice velocities’.

RC: P 893 L19-21: Any estimate of the uncertainty in the bedrock estimate due to the uncertain climate imposed?

AC: We performed an additional experiment in which the temperature between 1300-2007 is perturbed by 1K to quantify the sensitivity of the reconstructed bed to the imposed air temperature forcing. A brief discussion has been added to the text.

RC: P 893 L 22ff: Again, apparently the GPR measurements where already used...

AC: As discussed before, we performed an additional sensitivity experiment with an initial bed determined from perfect-plasticity to put the validation results into perspective.

RC: P 894 L 8-9: But this statement is based on one perturbation experiment only! That’s a bit weak for concluding what you state!

AC: Agreed. Now it includes two perturbation experiments in the real application... Further confirmation of this statement comes from the synthetic experiment starting with a rigorously perturbed bed (initial bed = reference surface).

RC: P 894 L 11: Well, not only “time-dependent” but also “correct”!

AC: Agreed. We added a note on this here in relation to the new sensitivity experiment with perturbed temperature since 1300 AD.

RC: P 896 L 1: So in your “constrained bed”-experiment all grid cells for which you don’t have a GPR measurement doesn’t knows at all about the measurements? What is the difference then between this experiment and replacing the relevant grid cells in your “unconstrained bed”-experiment? Would it be the same (if yes, it doesn’t seem very useful. . .)? As you mention yourself later, it would be suitable that cells in the neighborhood of available GPR-data would experience some influence of that data. Consistency with the “true” surface could be achieved by tuning a spatially distributed ϕ, in which a given value can be determined for the locations with GPR data and ϕ=13 is set for the “far field”. In between an inverse distance interpolation can fill the gaps.

AC: The grid cells for which we do not have GPR data are still certainly affected by the fact that bed heights are fixed to the GPR data at some distance from the grid cell. Fixing the bed locally implies that the surface height and hence the reconstructed bed will also respond non-locally. In fact, the reconstructed bed will shape itself around the fixed bed heights and certainly feels the influence of the fixed grid points. As suggested by the referee, one could attempt to locally calibrate the till strength for the grid cells with a known bed. This is however not straightforward. Due to the lack of spatial coverage of the bed data, ϕ is only adjusted for specific grid points. Due to the non-local response of the surface height to local adjustments of ϕ, there might be multiple solutions for ϕ along the GPR track that produce a similar surface topography. Note that this is not the case for the other experiments in which we adjust bed heights for the entire grid (rather than just a set of selected grid points).

RC: P 896 L 6-8: What do you mean? Why should this be expected? If your models, your method, your boundary conditions, and your measurements would be correct, you should get a perfect match everywhere! ;-)

AC: This is expected, since farther away from the fixed bed grid points there is not much influence anymore of the fixed bed on the final reconstructed bed. Hence, the difference in bed heights between the two experiments will decrease with distance from the GPR data grid points. You are right the discrepancy would go to zero in case of a perfect model ;-)

RC: P 897 L 6: Add “history of” before “surface climate forcing”. Your show how it is essential to have a correct climate evolution.

AC: Corrected.

RC: P 897 L 15: add “and a synthetic glacier geometry” after “surface forcing” – this is a rather important point!

AC: Corrected.

RC: P 898 L 13: Is “61 m” an average absolute deviation? Give a context for this number by stating the mean ice thickness for example.

AC: Done.

RC: P 898 L 26-28: Well, what’s the reason for not doing it?

AC: This would involve a lot of additional runs (depending on the desired accuracy of the recovered mean ϕ) and would not add much to the story. Additionally, one could debate whether it is best to calibrate ϕ such that the mean ice thickness matches the observations or when the mean RMSE/absolute difference reaches a minimum, which will not be the case for the same ϕ.

RC: P 899 L 18: Give a hint on how the approach by Polland and De Conto (2012) works.

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AC: Done.

Stylistic comments:

RC: P 875, L 7-9: ”at the start of a prognostic experiment” artificially involved. Can you simplify the sentence?

AC: We reformulated the sentence.

RC: P 875, L 12-14: Consider moving ”only partially” at the end of the sentence. P 877 L 24-25: Reformulate to “In the simplified water model, exchange of water between grid cells is not accounted for.” or something similar. The model is not “yours”, right? If yes, you need to describe it.

AC: Corrected.

RC: P 880 L 1-3: Why “In the initial run and the iterative model runs”? What other runs exists? Why not only “in the model runs”? Remove “-in-time”. Consider “applied” or “imposed” instead of “used”.

AC: Much of this paragraph has been reformulated to clarify the content.

RC: P 882 L 14: Insert “,” after “Fig. 3a” P 883 L 7: Well, Fig 4 doesn’t show the problem of “over-compensation”. So reformulate “Figure 4 clearly demonstrates [. . .]” P 833 L 8. Replace “profile” with “geometry”. P 883 L 26: Remove “with the number of iterations”

AC: Corrected.

RC: P 884 L 8-19: Consider moving this sentences to the next section.

AC: We believe it is appropriate to discuss this here, since it provides a transition from synthetic experiments to a real application.

RC: P 885 L 20-21: Remove “SPOT 5 stereoscopic survey of Polar Ice: Reference Images and Topographies”

AC: Note that this is an explanation of the abbreviation SPIRIT and probably should be included in the text. We rewrote the sentence for clarity.

RC: P 886 L 21-22: Well, this is corrected as well, just with another scheme, right? Reformulate. P 887 L 24: Consider “contaminated” instead of “polluted”. P 888 L 6: Reformulate to “The L2-model norm is used, which is equivalent to the square-root of summed squared deviations of the reconstructed bed height relative to the initial bed”. And by the way, why “is equivalent” and not “is”? P 890 L 22-25: Well, this applies only when following the approach you mention. The sentence is too general. P 890 L 28: “experiments (Experiment I)” sound strange (because of singular following plural). P 891 L 1: Consider the formulation “bed allowed to freely evolve everywhere”.

AC: All corrected.

RC: P 891 L 9-10: “bedrock heights” and “surface heights” can be replaced with “ice thickness”.

AC: We do not think that would be appropriate.

RC: P 891 L 11: Consider “lubricating” instead of “weakening”. P 891 L 15: Remove “in areas where basal sliding is significant”. P 891 L 21: In Eq. (1) the same parameter was defined as “material till strength”. Be consistent in the formulation. P 892 L 1: “log-log” (not “loglog”). P 892 L 14-15: Do you mean “sliding velocities”? P 892 L 19: Consider “After a dozen (or what you think is appropriate) iterations” instead of “Regardless of the initial bed, after many iterations”. P 892 L 22-24: Please reformulate this sentence. It is not clear what you mean. P 892 L 24: “stopping criterion” (not “stopping principle”). P 893 L 10-11: A verb is missing in this sentence. “Overestimating” perhaps? P 894 L 4: “stopping criterion” (not “stopping principle”). P 895 L 17: Here it sounds as you would trust more to point-measurements. Why you don’t do point comparisons if this is the case? But probably that’s not what you wanted to say.... P 897 L 1: “Discussion and conclusions” (and not viceversa). P 897 L 21: Remove “after many iterations”. P 898 L 9: Remove “,” between “approach” and “related”.

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AC: All corrected.

Comments to figures:

RC: Fig. 2.e: Show only lines for the iteration numbers displayed in panels “c” and “d”. The grayscale is not visible. Fig. 3.d: Show only lines for the iteration numbers displayed in panel “c”. The grayscale is not visible. Scale in panel “c” is not necessary (already given in “b”).

AC: We think showing just 4 lines in these figures is not an improvement and therefore did not adjust these plots.

RC: Fig. 5: Scale in panel “c” is not necessary (already given in “b”).

AC: We removed the color bar.

RC: Fig. 10: Name the two panel “a” and “b” (not “left” and “right”). You use the notation “Fig. 10a” in the text (P 891 L 28)...

AC: We added a) and b) to the figure.

RC: Fig. 11: Replace the wording “misfit” with “difference” (“misfit” suggests that the bed you get from the ϕ=13 experiment is the correct one. . .). Moreover, state the magnitude of the applied variations in the caption.

AC: Corrected.

RC: Fig. 12: Check the font of the legend inside the plots. Name of the variable is displayed as “!”...

AC: We do not encounter this problem when viewing the manuscript.

RC: Fig. 13: This plot is not adequate for showing what you want (differences in the panels “a” and “b” are hard to note, for example). Better show differences to what you think is your “best estimate”, similarly to what you show in Fig. 11.

AC: We increased the contrast of the figure somewhat. We think it is good though to

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show the absolute bed heights since from this the difference in spatial variability w.r.t. the modeled beds can more clearly be seen.

RC: Fig. 14: Make the color of the red dots the same as the scale on the left-hand side. Resize that scale to a size that is consistent with the scale in other figures. Make an inset with a scatterplot of “measured vs modeled” velocities. In the caption, φ should be Φ.

AC: We altered the color of the dots and resized them as well as the numbers. We do not think an inset scatter plot would be of much use here (only 6 points).

RC: Fig. 15: Use the same scale for panels “b” and “c”.

AC: Fixed.

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