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The Cryosphere Discussions

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

D. Farinotti (Referee)

farinotti@vaw.baug.ethz.ch

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— SUMMARY ————-

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 easy to

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

— GENERAL COMMENTS ————-

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!

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".

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...

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 ex-

periments 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.

5) Additional discussion that would be valuable The following points would benefit of a more in depth discussion:

(a) 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. . .

(b) 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.

(c) 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?

— SPECIFIC COMMENTS ————-

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.

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

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.

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

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

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. . .

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.

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.

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.

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

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

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?

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

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

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.

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?

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

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?

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”??

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

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

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?

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.

P 887 L 21: What’s your cut-off for “unrealistic”?

P 888 L 10: Point at Figure 10.

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

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

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

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

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

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

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

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

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

P 982 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.

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.

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

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

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

P 893 L 8-9: But this statement is based on one perturbation experiment only! That’s a bit weak for concluding what you state!

P 983 L 11: Well, not only “time-dependent” but also “correct”!

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.

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! ;-)

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

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

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.

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

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

— STYLISTIC COMMENTS ————-

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

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.

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”.

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”

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

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

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”.

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

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 982 L 22-24: Please reformulate this sentence. It is not clear what you mean.

P 982 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 19-20: “bump dimensions” sounds rather sloppy. Consider “bedrock roughness” or similar.

P 897 L 21: Remove “after many iterations”.

P 898 L 9: Remove “,” between “approach” and “related”.

— COMMENTS TO FIGURES ————-

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”).

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

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)...

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.

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

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.

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.

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

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