

Parameter and state estimation with a time-dependent adjoint marine ice sheet model - author response

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September 12, 2013

Response to M Morlighem:

The stress balance and mass transport equations are referred to as diagnostic and prognostic models in the abstract. I would suggest keeping stress balance and mass transport for clarity, as diagnostic and prognostic are not straightforward for readers not familiar with the jargon of Glaciology.

Agreed.

Throughout the text, slopes are expressed in tangent of degrees. I would simply use degrees for clarity (-tan(0.5) -> -0.5

It is done

p2851: It would be nice to show the basal friction equation (on β_{eff}) instead of referring to a paper

this has been included

p8252 line 20: Does single location mean cell (ie discretized domain), or an arbitrary x,y point of the model domain

Good point – I have attempted to eliminate this ambiguity in the new draft.

p2854 line 27: is well -> as well

ok

p2858 line 12: Gagliardini et al. 2011 present some interesting results about this topic. A reference to their paper would be relevant.

Yes, thank you, we had forgotten to mention this and the Walker et al study, and this has been fixed.

p2859: merge lines 11–12 and 22–23, which seem to repeat each other.

Done, thank you.

p2860 line 1: the figure the authors refer to is 2a, not 2b.

This must have occurred subsequent to my submission and typesetting by copernicus staff. I will be more careful when reviewing their typesetting.

p2862 line 19: missing period at the end of the sentence.

Thank you

p2865 eq. 14: remove km from the equation (eq 8 and 9 do not have units). Same thing for eq 18 and 19

Done

p2868 line 10: Seroussi et al. 2011 indeed show that ice thickness data is a major source of error in ice sheet model. A reference to their paper would be relevant.

Yes, thank you for pointing me to this. I have added to the citation list along with Durand et al as a work that finds strong dependencies on uncertainties in bed topography.

p2873 line 16: A matrix is never inverted (the invert of a sparse matrix is dense).

This is changed.

Figures: speeds are expressed in m/a, but the text uses m/yr (same for β^2)

Similar to above re: typesetting by copernicus staff. I always use m/a for meters per year.

Response to M Nodet:

General comment: we would like to respond to the general remarks of the referee. There seems to be a small amount of confusion regarding the contribution of this study, and our hope is that in attempting to settle this confusion this may obviate the need to address certain comments below, particularly those concerning experiment 3. The reviewer states that this is the first study to make use of the "complete adjoint" of an ice model for the purposes of inversion. We assume the reviewer is referring to stress balance models (see the abstract of the revised manuscript), because mention is also made of "approximate adjoints", where the strain rate dependence of viscosity is unaccounted for.

In any event this is not the case, nor do we claim it to be. See, for instance, Goldberg and Sergienko 2010 or Petra et al 2012. The contribution, which we are fairly sure is novel, is that the adjoint of the stress balance ("exact" or not, though ours is "exact") is only one component of the adjoint of the larger ice model, which includes time-dependent physics. At the moment the only

time-dependent physics are the continuity equation for thickness and ice shelf front advance, but hopefully we have argued that other physics can be added to the framework by very standard methods. In this way the adjoint sensitivities from a single stress balance solve can then be propagated backward through multiple time steps, as is done in experiment 3. Heimbach and Bugnion (2009) succeeded in applying AD methods to an ice model that DOES contain these "other" physics, such as temperature evolution and basal melting – but that model implemented a shallow ice stress balance. Our approach does the same for a higher-order stress balance, and this is the novel part. We are confident that those "other" physics, once added to the model, can be handled by the adjoint generation strategy. We feel this is adequately made clear in the revised abstract and the first few sections of the revised manuscript.

The study by Allemand et al is impressive, but we do not make comparison because we feel it is a fundamentally different inversion, although it is cited. That study uses a sequence of snapshot inversions - regardless of when the observations were made, it assumes that the velocities and surface elevations were exactly temporally coincident, and that the surface elevation is complete in spatial extent and exact. The type of inversion shown in expt 3 - using data taken at different times to infer parameters at yet another time (the initial condition) has never been done before, to our knowledge, with a higher-order model (though it has been done with shallow ice models in flowline settings, see Waddington et al 2007). And though, as you point out, our demands on surface elevation experimental error may be too high, we do at least allow for uncertainty in surface elevation observations, just as "snapshot" inversions allow for uncertainty in velocity observations (but not in surface elevation). To make comparison with oceanography, if one were working with ocean surface altimetry, a time series to constrain a time-dependent model would be more valuable than a single image; and that if one had other observations, e.g. currents or T/S at depth, and the observations were all taken at different times, one would not assume they were cotemporal.

Experiment 3 is meant as a first exploration of this type of inversion with a time-dependent higher-order model; and for this reason, it is done with idealized configuration and is slightly artificial with respect to input data (ie. complete coverage of highly-accurate surface obs, time-independent sliding parameters). it is not known at this point how performance will be with all of the modifications you suggest – and this is an active area of further experimentation (e.g. incomplete spatial coverage of observations). still, we feel the novelty of this experiment outweighs the relatively small subset of the experiment space we have explored. Furthermore, it seems an appropriate balance of what can be accomplished in one paper. We have added a short note at the end of 5.3 to this effect.

1. p2848 line 15 please also cite the first and grounding works of adjoint assimilation in oceanography, there are relevant papers in the 90s

The text in this introduction has been modified (5th paragraph of introduction in the revised manuscript) and earlier works are mentioned.

(* note - it is not useful for me to give page and line numbers, as the page numbers in my compiled draft will not match up with the online version)

2. p2851 I agree with M Morlighem: it would be nice to have the equation for β_{eff}

This has been done

3. p2862 l1 *There are plenty of past works on the subject of experiment 2, and a lot of them deal with real data, please mention this kind of works and refer to at least some of them.*

this is done - see 2nd paragraph of this section in the revised manuscript

4. p2863 (and fig3) l21-25 *Could you please show on your figure what initial guess you choose for beta. In particular, is it close to the background? Why couldn't you use something else, such as a linear beta, or a sine plus random noise, etc.*

Please see response to the next comment.

5. p2863-2864 *experiment 2: I find it difficult to assess the performance of the method based only on cost function graphs. You say the inverted beta is very close to the true one, but to get a clear idea it would be nice to see the corresponding sliding velocity, compared to the true one and to the one inverted by MacAyeal method.*

Hopefully your comments (4) and (5) will be addressed by the information and figures given here. The expression for the initial guess in the runs shown is

$$\beta_{guess} = \sqrt{1000 \sin\left(\frac{\pi x}{40}\right)} \quad (1)$$

We feel this is adequately described by the phrase "half-mode". We did not see the need to add results from a large number of experiments where different initial guesses were tried - as you point out, this is a well-worn type of inversion that has been tried time and again with real and synthetic observations. However, below we show the results from 2 other initial guesses for the high-slope version of the model (0.5 degree surface inclination). In one, the initial guess above is perturbed using the `randn()` matlab function with standard deviation 3, i.e. $\beta_2 = \beta_{guess} + 3 \times \text{randn}(40, 1)$. In another, a gaussian expression is used, i.e.

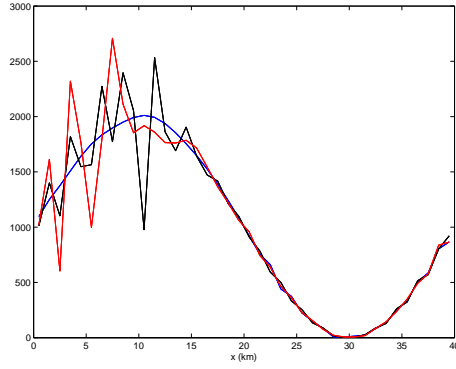
$$\beta_3 = \sqrt{1000 e^{-\left(\frac{x-20}{5}\right)^2}} \quad (2)$$

In both cases, the cost function is lowered dramatically, and the velocity misfit is small (order 1 m/a or less, compared with order 100 m/a or greater). You can see the inverted β^2 as well. For the initial guess used in the paper, the inverted field is very close to the true value, which is why we did not show it (it is well known that for such a simple synthetic inversion, this is easily achieved). With the other initial guesses, there is high-frequency variability inherent in the initial guesses and the velocity is insensitive to this variability due to the smoothing effect of membrane stresses; thus the noise is not dampened out in the inversion. A note is added to this effect in the new version of the manuscript, but we did not feel it was necessary to include any more figures.

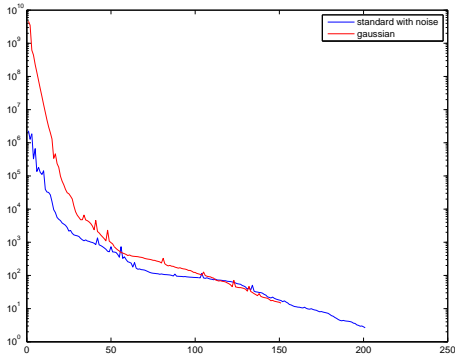
6. Questions about experiment 3:

(a) *the accuracy you require on S through s really is unrealistic at the moment, what are the results if you choose a realistic value (a few meters)?*

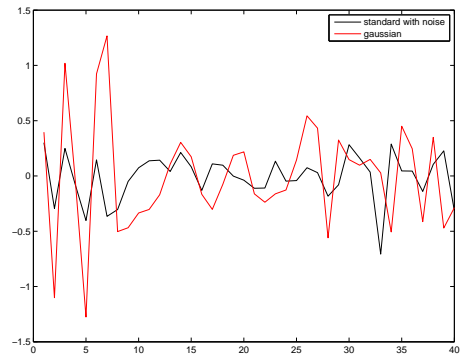
Given the relatively small change in surface elevation over the course of the experiment, meter-uncertainties in S would likely not recover the initial condition, as implied by fig 5(d). We assume



(a)



(b)



(c)

Figure 1: (a) inverted β^2 with high-slope (0.5 degrees) surface for different initial guesses - half-sine as in paper (blue), half-sine with noise (black), and gaussian (red). (b) cost function for alternative initial guesses (c) associated velocity misfit with "observations" for alternative initial guesses.

the meter-level uncertainties relates to satellite radar or lidar, which is of course the only way to easily sample such a large area. GPS measurements can give much higher accuracies, provided there is an ice-free promontory, or a position where ice elevation is non dynamic, somewhere in the vicinity. However we concede that generating such measurements once a year on a 40 by 40 1-km grid might not be realistic. Still, we feel that such artificialities are acceptable given the exploratory nature of this experiment (see discussion above).

(b) is it interesting to look for H ten years ago? wouldn't we want to provide forecasts for the glacier evolution in the future instead? in that case, the knowledge of the last 5 years would be enough. could you please provide references to papers where people ask this kind of questions?

As stated in the discussion, this type of inversion has not been carried out before. However, we point the reviewer to studies where historic accumulation rates have been inverted, such as Waddington et al 2007 (listed in the references). We do not consider accumulation; however, these studies require also to estimate historic ice thicknesses.

In fact, looking back to understand/constrain *past* thickness changes is not only interesting, but crucial to improve our understanding of the causes of past ice sheet evolution, and a prerequisite for understanding the forecasting problem. A good example is the decadal change of Pine Island Glacier and Ice Shelf, for which a transient inversion is of considerable scientific interest. The community tends to jump to the forecasting problem before having properly understood the hindcasting problem. Given intrinsic ice stream time scales, knowledge of the past 5 years is not enough.

(c) why not use also surface velocity information for the last 5 years? does it not make any difference?

as stated in the discussion, we are investigating the case where different types of observations are not cotemporal.

7. Also a lot of things are bothering me with this experiment:

(a) centimetric accuracy on S

see above (response to comment 6a)

(b) bed supposed perfectly known, but in practice it is also measured, so it would mean more errors, on ice thickness in particular

we accept this - however, we have addressed this issue through investigating bed uncertainties in expt 4

(c) looking at the thickness in fig 4c it seems that the experiment focus on a hill which is around 5 meters high. In that case is the hybrid model accurate the model such fine scale phenomenon?

we refer the reviewer to Goldberg 2010 and Sergienko 2012, both listed in the references, where

this question has been dealt with.

(d) the prescribed beta does not depend on time, but usually in such transient phenomenon we do have a time-dependent beta

this is true - this is an added complication to an already novel form of inversion, so we have not considered it yet. It is certainly worthy a study in its own (with added complexities in terms of the increasing numbers of degrees of freedom, physical motivation for time-dependence, etc.)

8. Thus I would have suggested a more realistic experiment instead of exp 3:

(a) similarly: known bedrock, observed surface velocities, thickness unknown

(b) S observed for 10 years, s = a few meters

(c) beta variable in time and unknown

see e.g. the framework of the surge of Variegated glacier by M. Jay-Allemand and others <http://www.the-cryosphere.net/5/659/2011/tc-5-659-2011.html>

We feel these suggestions would transform the experiment into a different type of experiment, which has essentially already been carried out by Jay-Allemand et al. As we explain at the beginning of this response, the present study is a novel transient inversion and our experiment is of an exploratory nature (such synthetic experiments to introduce novel concepts/approaches are well known in ocean data assimilation research).

9. p2870 l21 why does the model run for only a single timestep? what happens for longer time-windows?

This is a good question, but one not explored, because it would affect the ease with which these experiments can be compared to already published work. In this context we point out that studies of a similar nature (Morlighem 2010, and Perego et al, unpublished) similarly only consider a single time step (whether it is actually stated or not).

10. p2889 experiment 4 fig7: could you also please provide either a figure (or information reg. RMS errors) for the sliding velocity associated to the inverted versus the velocity without assimilation?

We intentionally do not provide RMS errors for the sliding factors - this is because, from a physical point of view, the sliding factor is not "real". It is an inexact, unknown function of many unresolved factors (bed geology, rheology of basal ice, presence of water, presence of asperities), but in the macro sense it is an artifact of the numerical model. For the same data, a Blatter/Pattyn model, a full-Stokes model, and even a model solving the same equations as ours but with a different scheme would give different answers. And so it can never be measured through independent means, as the bed elevation can (e.g. seismics, very accurate GPR).

11. did you implement a classical method (approximate adjoint) for experiment 4 as

well? it would be nice to see how it performs compared to the full adjoint method.

This is an excellent question – however, for the revision, we felt it was more important, given the timescale imposed by the journal, to address the issue you brought up below, which we did. The incomplete adjoint question is a subject of current investigation (answers will likely vary according to processes considered as part of the modeling, and observational coverage available).

12. p2871 eq (21): in reality s is not trully known but observed with measurement errors, so that the method your propose could not really be extended to real data. what happens if you do what you say line 21 and penalize the misfit of s in the cost function?

We have performed several more experiments, both of the nature you suggest, and in which error is inherent in the surface elevation but not penalized. Again, in the former we have assumed (possibly unreasonably) small errors in surface elevation – but still this experiment is largely exploratory, and one can infer that the results might be worse with larger uncertainties in surface elevation.

Please see section 5.4.2 in the revised manuscript, which is a new subsection. (a portion of section 5.4 of the original manuscript has been split off to form subsection 5.4.1)