

Interactive comment on “Application of a two-step approach for mapping ice thickness to various glacier types on Svalbard” by Johannes Jakob Fürst et al.

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Summary

In this study, Fürst and others present an updated method for solving the problem of inferring ice thickness from sparse observations coupled with surface data. They then apply this method to three glacial systems on the Svalbard Archipelago. The validity of the resulting estimations of ice thickness are established with a detailed error analysis.

The paper is clearly relevant and within the appropriate scope for the journal. Contemporary interest in methods for inferring ice thickness are of great interest to the

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community, as evinced by numerous recent publications on the subject, including a comprehensive intercomparison (Farinotti et al., 2016). This paper's contribution to the field stems primarily from its presentation of a way to circumvent some of the arduous data requirements required by the method on which it is based (Morlighem et al., 2010). I am concerned that this paper inherits some of the potential shortcomings from that work, namely an error analysis which is not, in my view, completely justified, as well as a misunderstanding of the definition of error for PDE-constrained optimization. Nonetheless, the manuscript does a commendable job with respect to discussing its own limitations and in firmly placing the issue in the context of error analysis. I am not sure that the paper will provoke a sea-change in thinking about Svalbard's glaciological processes, but I imagine that the results will be useful for modellers and others needing ice thickness estimates.

Stylistically, I think that the paper could benefit from significant distillation. The elimination of superfluous words, sentences, and perhaps even sections would help the reader to focus on essential points. As it stands, the manuscript feels like a methods paper mixed up with a case study. A stronger partitioning between these two parts would help. The paper also contains a fair amount of questionable English. I have tried to make corrections where I can, but a more detailed reading by the authors themselves is in order.

Ultimately, under the assumption that the authors can address the criticisms that I have presented below, I would encourage resubmission.

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1 Major Points

1.1 On the use of 'apparent flux divergence'

The phrase 'apparent mass balance' is well ensconced in the literature at this point, and the reasoning for its use is fairly clear: when $\partial_t h$ is used as an observation, it acts identically to \dot{b} , which is a source term. Combining them leads to a simplified equation involving the flux divergence and this unified source term. The term 'apparent flux divergence' makes no sense at all. In fact, a more correct statement would be to just call the apparent mass balance the flux divergence (rather than apparent flux divergence), because that is what the equal sign implies. However, this would be confusing and tautological to say that the flux divergence equals the flux divergence. It is equally confusing, but perhaps less correct to say that the apparent flux divergence equals the flux divergence. If it's not clear already, I suggest using the term 'apparent mass balance' instead.

1.2 General characteristics section, and the extensive use of proper nouns

This paper walks a difficult line between a methods paper and a case study. I don't have a problem with that, as it's generally useful to see methods applied to real cases to evaluate their worth. However, I think that the organization of this paper is such that it can be quite confusing. I would suggest reorganizing the paper such that the methods are completely stated (including the theory behind the error analysis; more on that in a minute), then switch gears and begin discussing the nuances of Svalbard's geometry and data availability. Thus I would not have to remember what and where Austre Torrellbreen is after reading many pages about hyperbolic PDEs.

Additionally, all figures need to be labelled with salient features discussed in the text. The reader should not have to cross-reference Figure 2, while at the same time reading

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the paper text and analyzing the figure content. Additionally, some basic annotations describing some particular key points referenced in the text and restated in the figure captions would be very helpful.

1.3 Estimates of input observation error

Input data error estimates need to be stated more clearly and with more complete justification. For example, the authors use a mean value of 5 m for estimated thickness error for all experiments, based on a (convincing) paper in which GPR was applied to a relatively thin glacial system, with lower wavelength antennae. It is not tenable to assume the same error estimate for airborne radar measurement from the 1980s. Also, no real effort is made to justify the 0.2 m yr^{-1} estimate in apparent surface mass balance.

For all of these error sources, the distinction between simple observational error and the error associated with the variables used in your equations (which are time-averaged) needs to be discussed. See the methods section of Brinkerhoff et al. (2016) for a discussion of what I mean by this; in short, Eq. 1 assumes that all of the inputs are temporally consistent, but in reality they are not, and this induces an additional source of uncertainty.

1.4 Flow directions

I strongly suggest reading Kamb and Echelmeyer (1986), when approaching flow direction computations. They provide a strong theoretical basis for how to approach ice routing using only surface elevation observations in order to approximately recover higher-order directions. With the availability of these theoretical results, which have been used successfully many times throughout the balance flux literature, I find the *ad hoc* method of smoothing by applying a contouring algorithm then using natural neigh-

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bors interpolation to degrade the surface elevation accuracy to be troubling. Barring the success of the smoothing proposed by Kamb and Echelmeyer (1986) at eliminating closed basins (which I understand to be the problem that elicited the *ad hoc* approach), there are plenty of basin filling algorithms that would also solve this problem in a somewhat more rigorous way.

This consideration should also be included when using the SIA to infer thickness fields from the balance flux. Contemporary surface DEMs often show topography at a much smaller scale than that which is relevant for determining driving stress in the Stokes' equations, and smoothing is necessary to avoid non-physical oscillations.

1.5 \dot{a} as a control variable

The \dot{a} resulting from the inversion procedure needs to be presented, because the derived thickness field only conserves mass with respect to this augmented field, and in my experience performing these types of inversions for \dot{a} without considerable explicit smoothing leads to an apparent mass balance that can look pretty weird. Indeed, it becomes the dumping ground for all manner of errors derived from other input fields and the model itself. One way of getting around this is to apply a regularization term to \dot{a} (with a regularization parameter associated with the length scale of feasible spatial variability in apparent mass balance), just as it is already applied to H in Eq. 4.

1.6 Boundary conditions

The PDE being solved here (Eq. 1) involves only first spatial derivatives and no time derivative. As such, the number of boundary conditions allowed is limited to one per characteristic. An ice divide implicitly acts as a zero-flux boundary, meaning that specifying the flux at the margin is not well-defined. Nonetheless, the magic of numerical solutions allows it anyways. However, one needs to be quite careful to understand that

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this introduces a fictitious source term (as it must for mass to be conserved) that needs to be accounted for in error estimates and interpretation of results. This artifact is evident in Fig. 4, along the eastern margin of VIC, where thickness is around 300m right up until it reaches a land terminating boundary. In a sense, this is the opposite problem of that which you're trying to solve with the first term in Eq. 4, but rather than the flux running out before reaching the margin, it doesn't go to zero fast enough. Perhaps consider introducing another term to Eq. 4 that adjust the surface mass balance so that flux goes to zero along land terminating boundaries?

1.7 Formal error estimate

This section is mostly based on Morlighem et al. (2010), with the key difference that flux at measurement locations is directly imposed via Dirichlet boundary conditions rather than a best match found through an inverse procedure. This is problematic, for the same reason discussed in the above point about boundary conditions: imposing the value of the flux at more than one location along a flightline must lead to a fictitious numerical source in order for the condition to be upheld, because the PDE is first-order and only admits a single boundary condition per characteristic. Thus there is a hidden source term that needs to be included in the estimate of \dot{a} that is likely to locally exceed the (already very optimistic) error estimate of 0.2m yr^{-1} .

On a related note, I think that the estimate of uncertainty in surface mass balance is probably incorrect following the inversion procedure. Consider the following line of reasoning: the PDE for error propagation is derived by stating that

$$\nabla \cdot (\mathbf{n} + \delta\mathbf{n})(F + \delta F) = \dot{a} + \delta\dot{a}, \quad (1)$$

which is separated into

$$(\nabla \cdot \mathbf{n}F - \dot{a}) + (\nabla \cdot [\mathbf{n}\delta F + \delta\mathbf{n}F] - \delta\dot{a}) = 0, \quad (2)$$

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where \mathbf{n} is the true (error free) flow direction, F the true flux, and \hat{a} the true apparent mass balance, and the δ -annotated quantities are the errors associated with each of these quantities. The first term is zero due to mass conservation, and the second is solved for δF to get the error in the flux. The problem arises in the definition of $\delta\hat{a}$. In the manuscript it is given a numerical value (0.2m yr^{-1}), which is supposed to represent the observational uncertainty. However, after the optimization procedure is complete, which makes significant modifications to \hat{a} (I assume, which is why it needs to be reported), this variable is no longer the value for which that particular error estimate holds. Instead, it is a new and potentially non-physical field into which has been placed error in surface elevation, smoothing lengths, numerical error, etc. The field resulting from the optimization is neither the true value \hat{a} , nor the original field for which the error estimate was made, and as such $\delta\hat{a}$ must pick up the remaining difference. I presume that it would thus be substantially larger than the initial 0.2m yr^{-1} estimate and would be neither independent nor identically distributed.

As a final objection, I do not understand this business of taking the minimum of two error propagation PDEs, one with a reversed velocity field. Why should a more favorable error estimate propagate back upstream from an observation, given that hyperbolic PDEs transport information in one direction only (with respect to a characteristic). It is interesting to note that neither publication that initially stated this method (Morlighem et al., 2010, 2014) gives a reference for it, and I have never been able to find one in my own literature searches. I invite the authors to take this opportunity to convince me of this idea's correctness.

Now, lest I appear too curmudgeonly on this issue: is all this a big problem? Probably not. My Bayesian treatment of the problem (Brinkerhoff et al., 2016) suggested that the error estimate suggested here isn't too bad in a practical sense, and the cross-validation presented in the latter parts of this manuscript suggest the same. Additionally, the additive model of errors used in the PDE error propagation equations would tend to overestimate uncertainty (for the same reason that adding two Gaussian ran-

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dom variables doesn't double their standard deviation). However, I would like to see a more robust defense of the theory behind the methods used, and a more transparent accounting of the simplifying assumptions.

1.8 Error estimate results

The formal error estimate should provide an upper bound on the actual mismatch, not a prediction of it. A good metric here would be to compute the frequency by which the actual mismatch falls below the predicted error. In the context of normal distributions, the actual misfit would be less than the predicted misfit 95% (or whatever your definition of error is) of the time. This is more or less the definition of credibility intervals in Bayesian statistics. If the mismatch falls outside the estimated error much more frequently than this, then one begins to question what use the estimated error is. While it's sort of interesting to consider the median values, this neglects the aspect of error prediction which is likely to be of most interest to people who would use this product: spatial skill.

1.9 Appendix A

If one cannot attribute the observed flux to deformation in a physically reasonable way (e.g. with a material parameter A that is physically justifiable), then doesn't it make sense to assume a certain amount of sliding? It seems to me that instead of allowing viscosity to be an order of magnitude less than temperate ice, one could adjust β^2 instead. This would make for a much more straightforward way of interpreting a figure like Fig. A1. I recognize that the authors may not wish to add the additional uncertainty of selecting a sliding law to the model, and I'm certainly fine with that. However, the notion that the viscosity parameter is aliasing unmodelled basal processes should at least be addressed.

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2 Minor Points and Technical Corrections

NOTE: This paper has a relatively high number of typos and grammatical errors. I will try to point them out where I see them, but the manuscript would benefit considerably from detailed copy editing.

P1L4 Please define what 'performs well' means.

P1L12 'Withholding parts': the paper shows this in a median sense, not a spatially explicit one, which is an important point.

P1L16 'are in fact' → 'is'.

P2L4 Delete 'large' (and non-specific adverbs throughout the manuscript at large).

P2L5 'thickness of the ice cover' → 'ice thickness'.

P2L11 'Antarctica Ice Sheet' → 'Antarctic Ice Sheet'.

P2L13 'thicknesses' → 'thickness'.

P2L35 Perhaps elaborate on what 'computationally less favorable' means.

P3L1 delete 'physical'.

P3L7 I don't understand the implication of this sentence.

P3L14 'allows to estimate' → 'allow estimation of'.

P3L15 Missing period.

P3L15 'Much development ...' needs citation.

P4L1 'For DEMs and elevation changes...' this sentence doesn't really say anything.

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P4L1–8 Citations are needed throughout.

Introduction at large I suggest including a paragraph on the general availability of thickness observations for consistency.

P4L23–24 This sentence needs a bit more specificity.

General characteristics at large This section should be compressed a bit to ensure that the information presented is relevant to the conclusions of the paper.

Glacier outlines why not use the modern high-res DEM everywhere?

P6L11 'For VIC, thickness measurements' → 'VIC thickness measurements'.

P6L14 Delete 'there only'.

P6L23 Do borehole depths agree with GPR?

Figure 2 Is $\partial_t h < \text{SMB}$ along ice divides? This was a problem I encountered in ITMIX.

P9L25 *Olex* needs a citation.

P10L8 I think you can delete everything up to 'incompressibility can be written as ...'.

All equations The divergence operator is traditionally written as $\nabla \cdot$, rather than just ∇ , which is typically thought to mean the gradient operator.

P10L28 'Inflow boundaries': I don't understand this sentence.

P11L18 I'm not sure I understand how using the spatial gradient in SMB solves the problem. Assuming that SMB is only a function of elevation, doesn't SMB also reach a maximum where slope goes to zero, i.e. SMB also has a very small slope where surface elevation does?

P11L24 Not sure what is meant by ‘ice-flux direction is positive’.

Eq. 4 I think that the integral should be

$$\int_F^{\infty} \delta(s) ds d\Omega. \quad (3)$$

This means that if $F < 0$, then the integral crosses the origin, and the δ function is activated. As it is, the function penalizes positive flux values. Writing that integral as a Heaviside function might be more clear.

P11L30 Maybe just state the parameter values and reasoning behind them, and forego subjective descriptions like ‘good performance’.

P12L11 In glaciology, aspect ratios under which the SIA applies are usually referred to as small, rather than large. This derives from the aspect ratio being the small parameter in the asymptotic analysis of the SIA.

P12L24 Again, read Kamb and Echelmeyer (1986) for some theoretic basis for how to smooth for SIA applications.

P12L29 A clearer sentence might read ‘We apply a correction to the computed flux in order to avoid negative thickness values’.

P13L2 Would it be possible to explain why these particular functions and parameters were chosen?

P13L4 ‘If no observations ...’ I don’t understand this sentence.

Formal error estimate There should be at least a mention of additional error induced by using the SIA. This should include error in the surface gradient norm, among other things.

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P14L27 ‘to’ → ‘too’.

P115L1 Maybe make a reference to L-curve analysis here.

P16L12 ‘geometry’ SIC.

P16L17 Maybe note that for a region bounded by two flowlines, flux is always at a maximum at the ELA.

P16L24–26 Is it that the old routing is still dominant on the surface topography, or that the velocity field and DEM aren’t contemporaneous?

P17L1 This line makes a strong case that $\delta\dot{a} = 0.2$ everywhere might not be right.

Ice thickness and bedrock elevation at large Is it possible to extend the results of your error analysis to integrated quantities like total sub-sea level area or total volume? It would make these numbers considerably more interesting if we could be sure that they represented a (statistically) significant departure from previous estimates.

P22L26 ‘Therefore ...’ I don’t understand this sentence.

Figure 8 A gradient, rather than random colorbar would be useful here. Also, linear axes would help to get a sense for the sizes of the error bars.

P24L4 ‘none’ → ‘any’.

P25L23 Delete ‘anyhow’.

Ice thickness at large Much of this section focuses on the differences between the first and second-step solution. Perhaps a figure showing the differences between the two predicted thickness fields would help the reader get a sense of how the differences are distributed?

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Figure 9 The transparency method for delineating which areas were subject to the second stage isn't very clear. Maybe switch colormaps or just draw a line around the areas that were updated.

Error estimates at large If error estimates go up when using the second stage, please convince me why the second stage is useful. Perhaps a similar plot to Fig. 8 is in order, which would hopefully show that including velocity observations reduces the actual mismatch for withheld measurements.

P27L11 There's no 'might' about it: ignoring sliding biases the result towards thicker ice.

P28L14 Perhaps consider reading Brinkerhoff (2016), which discusses this point in somewhat more detail.

P29L23 'mere' → 'a mere'.

P30L6 'tend to overestimate mismatch values' when taken in aggregate, but not necessarily individually.

P30L7 'Error estimates can here be considered upper and lower constraints of inferred thickness values' → is this not the operational definition of error? If we knew that error was the exact amount by which our estimates were off, then we could just subtract it and get perfect results.

P33L17 'For glaciers ...' this sentence is pretty awkward.

Figure A4 This might be better served by displaying a map of the difference in thickness between the two experiments.

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References

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