The Cryosphere Discuss., 2, S267–S272, 2008
www.the-cryosphere-discuss.net/2/S267/2008/

The Cryosphere
Discussions

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Interactive
Comment

Interactive comment on "On the limit to resolution and information on basal properties obtainable from surface data on ice streams" *by* G. H. Gudmundsson and M. Raymond

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Received and published: 4 September 2008

This is a very interesting paper of high scientific quality and with important conclusions. It presents a novel method of retrieving (inverting) basal properties (basal topography and slipperiness) from surface observations on ice streams with a particular focus on its limitations and uncertainties involved with it. It gives quantitative estimates for these limitations and on how much information of the basal properties can actually be retrieved and how much they are affected by the errors in prior estimates and the errors in the measurements. It also shows that information for basal topography can be retrieved rather well where as the retrieval for basal slipperiness is rather limited. Although the presented paper only deals with linear siding and rheology, it also gives some clear indication on what one can expect for non-linear sliding and non-linear rheology.

This study is rather theoretical and mathematical and in some parts it may be hard to follow for non-experts in inverse modelling, however, the author explains and rephrases the important points and results on a level that is appropriate for a wider audience and illustrates and visualizes the sometimes rather complex results on simple examples and graphically.

The results of this paper have important implications future studies on retrieving information on basal properties (inverse modelling), but it also provides some guidance on the requirements for observational data which is important for data collection. I therefore see this paper, despite its complex theoretical touch, highly relevant for a rather wide readership in cryosphere science.

Regarding the need of future numerical ice sheet models for improved and better constrained basal properties and the rapidly growing availability of surface measurements from remote sensing, this paper is of crucial importance and a highly valuable contribution to 'The Cryosphere'. The manuscript is almost publishable as it is and I therefore recommend to accept this paper for publications with only minor revisions. Below some general comments and issues to consider for the final version of this paper.

Specific comments

Two points to note for the comments below:

1 I agree to most points of the referee M. Truffer (RC S147) and will not repeat any of the minor issues such as spelling and typos.

2 I would not consider myself as expert of transfer functions or Bayesian inference. I tried to follow and check the equations below but in some instances I did not succeed, but assumed they are correct.

Some specific comments that may be addressed and help to improve this paper:

- I may not have understand this properly, but I wonder how the prior estimate is obtained in a real world case to an accuracy of 10%. As far as I understand the given 'stationary auto regressive process' (p. 420) describes the covariance (given by $\lambda_{\bar{b}}$, $\lambda_{\bar{c}}$ and $\sigma_{\bar{b}}^2$, $\sigma_{\bar{c}}^2$) but not the prior estimates of basal topography and basal slipperiness itself (I hope I am right here). At least some mean value has initially to be guessed. For the basal topography I can see accuracies 10% as realistic given some radio echo-sounding data is available (or whatever method is available for an initial guess) but for the basal slipperiness I wonder how an initial guess within 10% can be obtained, in particular as basal slipperiness can vary spatially significantly (orders of magnitudes, slippery- and sticky-spots) beneath ice streams. I may have misunderstood how prior estimates are obtained here, but maybe it needs some clarification and more explanation on this.

- Applicability to ice streams/glaciers: The presented method and limitations on retrieval of basal properties are as I understand applicable for ice streams as it deals with 'small perturbations' in basal slipperiness and topography to some mean. I wonder what can one get out of this study for the case of valley glaciers where basal topography and slipperiness do vary one order of magnitudes in cross- but also along-flow directions. Are the general conclusion expected to be similar, or can one not tell? I guess in the case of valley glaciers the slipperiness is often rather low (below 5) so the considered examples in this paper may not be relevant for glaciers anyway. Also, how does it look like for areas near the margins of ice streams, is it still applicable? Often inversions are done right to the margins where transversal effects may become important.

  This comment about the applicability to glaciers has been motivated by mentioning the term 'glaciers' in several instances in the paper. In the introduction, this is fine, but on p. 416, L20 it states that 'We consider the problem of ... from

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measurements of ... on GLACIERS'. Should it not say '... on ICE STREAMS'?. Otherwise one may get the impression that this is all applicable for glaciers as well.

- Sensitivity to basal slipperiness: A further question on the applicability is the sensitivity of the found conclusions to basal slipperiness. This paper deals with ice streams and therefore high slipperiness (I think most examples use slip ratios of 500). I wonder what the lower limit in terms of slipperiness is at which the above conclusion break down. Or how do they change with decreasing slipperiness. One should probably be able to deduct that from the transfer function forward model (Gudmundsson, 2003), but it may be useful to shortly discuss or repeat it somewhere in this paper.

- It is a very interesting and useful finding that vertical surface velocities are not really needed for a reasonable inversion. In the 2-D case (along and transverse flow considered) two directions of surface flow can be measured. How would the inversion behave when only one flow direction component is observed? This case sometimes appears for interferometrically derived velocities when only the satellite look-direction component has been determined. Is this still enough information for a reasonable inversion?

- Also, for real remotely-sensed data, the 'measurements' available have often gone through some processing such as smoothing, re-interpolation onto some grid, thus the raw measurement may not be available for inversion. Also, in particular for interferometrically derived velocities the errors of the measurements are spatially unlikely to be independent (bias, offsets or spatial trends due to difficulties in referencing to zero flow). This means that real measurement errors may well be correlated and/or not normally distributed as assumed in this study. How would such offset errors or correlated errors affect the conclusions here? How realistic is the assumption of uncorrelated and normal distributed measurement

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errors for real world data?

- Slip ratio c: I agree with reviewer Truffer (RC S147) that the use/definition of slip ratio (slipperiness) C is not consistent through out: p. 417, L 9: $u_b = c \cdot \tau_b$; p. 425, L 19-20: $c = u_{sliding}/u_{deformation}$
- Zero surface mass balance: Not clear to me how this exactly works with zero mass balance. If an ice mass has zero mass balance it will just flow away and flatten until it is infinitely thin. I assume in the case considered here, a finite section of an ice stream is considered with a fixed in-flux at the vertical upper boundary and a fixed (the same) out-flow at the lower boundary, then a steady state surface will exist with zero mass balance. May need some clarification.
- I could not really follow how one gets from equation (16) to equation (17), I think a bit more information on where it comes from (what these standard arguments are) would be useful, even if it is just a reference. This probably links to a further issue of how the conditional probability is defined P(b,c/u,v,w), again some more introduction into the Bayesian approach (early on, p. 415) explaining at least some principles may be helpful for the non-expert reader (see also referee comment RC S147).
- P. 432, L21-22: is this a consequence of eqnuation (29)?
- Figures: All figure fine, but not always clear what slip-ratios have been used for these experiments. Has for the figure 3, 4, 5 and 6 also a slip-ratio of 500 been used as for the figures 1 and 2?
- a list of symbols/variables would be helpful.
- Readability and typography/font of equations: I did not find the font/typography used in this paper effective for displaying equations, for example, without looking twice I found it hard to see if there is a tilde or hat above the quantities. This is

not criticism to the paper/author but rather to the choice of font/typography by the journal itself.

Technical corrections

- equation (4): the first $\hat{s}$ should probably be bold.

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