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The Cryosphere Discussions

Interactive comment on "Investigating changes in basal conditions of Variegated Glacier prior and during its 1982–1983 surge" *by* M. Jay-Allemand et al.

Anonymous Referee #1

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My recommendation is that this paper should be published in The Cryosphere after some modifications, described below.

The study uses inverse methods to investigate the conditions beneath Variegated Glacier in the build up to one of its surges, and during the surge itself. Observations of the shape of the glacier and its flow-speed are used to infer the drag at the base of the glacier, and how it changes over time. The main tools used to perform this inversion are a previously published set of field observations, collected during 25 separate measurement campaigns, a finite element model of Stokes flow, and a recently published algorithm that allows iterative solution for the basal friction parameter. This parameter

defines the slipperiness of the subglacial sediment or rock. By performing the inversion on each of the 25 datasets, a spatio-temporal history of the changes in basal friction is inferred. These changes in drag are then interpreted in terms of the basal water pressure beneath the glacier. Finally, a prognostic simulation of the surge is performed in which drag is varied, but the surface of the glacier is allowed to evolve.

Overall, this is a valuable piece of research that represents an advance in quantifying what happened at the base of this glacier in the build up to its surge, and while the glacier was actually surging. The subject matter is certainly appropriate for the Cryosphere, and the research is timely: even though the data considered here are now several decades old, and finite element models of Stokes flow have been used to model glaciers before, this study represents the first application of this particular inverse method (Arthern and Gudmundsson, 2010) to real glaciological observations. As such, the study is a valuable demonstration that this inversion technique is applicable in practice. The considerations given here to regularising the inverse problem are also likely to be of interest to other researchers employing similar methods. The paper is well written and clear, and the clarity and choice of figures seem appropriate to me, subject to some alterations suggested below.

The main problems that I can identify occur in the section that converts values of friction parameter into water pressure. This is certainly well motivated, because effective pressure plays such a crucial role in most theories of how subglacial hydrology couples to ice flow to initiate surges. However, I am not convinced that it is really possible to get all the way to maps of subglacial water pressure as implied here. The fundamental problem is that one inferred parameter (the basal friction parameter, β) is used to estimate the unknown water pressure via a relationship that itself contains unknown parameters (specifically the maximum-slope parameter, C, and a parameter related to the drag over the unpressurised subglacial system, A_s). It is true that some attempt is made to bound these quantities, but this section is much less convincing than the earlier sections of the paper.

Substantial modifications

Some specific concerns that need to be addressed are:

1) A value of $C = 0.5$ is used, but the theoretical range of this parameter is from zero, if obstacles causing cavitation are extremely flat, to much larger than unity, if they are extremely steep steps. There does not seem to be any compelling reason to choose $C = 0.5$ as is done here. If C were doubled, the effective pressures would be different, and so would the water pressures.

2) Even allowing for uncertainty in choice of C, there are further uncertainties introduced by the choice of the other free parameter A_s. Here, this is constrained by assuming that the largest effective pressures within the time series are equal to the normal traction on the bed (i.e. that the water pressure is then zero). While this does provide a physical constraint upon A_s, it does not allow it to be identified uniquely. It seems quite possible that even the largest effective pressures in the time series are actually quite close to zero, relative to the normal traction (i.e. the glacier is at all times fairly close to floatation). Again, this would change the values of water pressure plotted in Figure 6.

There are two ways these issues could be addressed.

1) The section on solving for water pressures presented in Figure 6 could be replaced with a qualitative description of how water pressure would have to vary to explain the changes in the friction parameter, i.e. when and where it would most likely have to increase, or decrease, without discussing the quantitative values of pressure. The rest of the paper is strong enough to publish without deriving quantitative values for the water pressure.

2) If estimates of water pressure are to be included I think the inversion needs to be much more sophisticated than it is at present. Since the problem of recovering water pressures is ill-posed, due to the unconstrained parameters (C and A_s), it should

be treated as a formal inverse problem. Prior information regarding the distribution of these parameters should be incorporated into the inversion for pressure. It would be much better to acknowledge that there are a range of pressure maps consistent with the available observations and prior parametric uncertainties, rather than just presenting one map. In a more complete inversion, other information such as the observations of water pressure recorded in boreholes (Kamb, 1985) should also play a role. I suspect that a full investigation along these lines would contain enough extra material for another paper, but it could perhaps be included here if presented succinctly. In that case I would not consider the changes to be minor, and a further review would be appropriate.

Minor changes

P 1462, Line 4. Replace ‘consisting in’ with ‘consisting of’.

P 1462, Line 11. Replace ‘wave length’ with ‘wavelength’.

P 1462, Line 13. Replace ‘periodical’ with ‘periodic’.

P1464, Line 2. ‘When a threshold amount of geometry change’ could be clearer. Does this refer to thickness change, or slope change?

P1465, Line 24. Repeated ‘the’. Both occurrences should be deleted.

P1466, Line 13. Replace ‘agreements’ with ‘agreement’.

P1470, Line 1. This cost function does not just penalise mismatch on the surface, but throughout the volume (see alternative expression given by Equation 3 in Arthern and Gudmundsson, 2010), better just to say ‘expresses the mismatch between the two models’.

P1470, Equation 12. Typographical error in equation: should be α', not β'. Also, notation of intermediate step is ambiguous.

P1471, Line 6. Replace ‘now writes’ with ‘is now’.

P1471, Line 11. 'The addition of a regularisation term ensures existence of a global minimum'. Not sure why. If J_0 is unbounded below surely J_{tot} could be too? This statement needs to be clearer, or deleted.

P1473, Line 7. Arthern and Gudmundsson (2010) showed that noise could also produce oscillations that are not on finest resolvable scale, depending when iterations are stopped.

P1473, Line 20. When the regularisation parameter is increased from 0 to 10^6, the mismatch with surface velocities increases from 5% to 10%. One choice for the regularisation parameter would be to maintain this discrepancy within the error on the surface velocity observations. Are there any estimates for this accuracy? If so, they should be included, and could perhaps be used to guide selection of the regularisation parameter.

P1473, Line 25. Hansen advocates (fairly strongly) the use of a log-log plot when drawing and interpreting the L-curve. Here a log-linear plot is used. I would recommend changing this to a log-log plot. It is not clear to me that the elbow in this curve would be so apparent: if it is not, the reasons for that should be discussed. It would also be helpful to include assumptions behind this approach. In what sense is the recovered regularisation parameter optimal?

P1474. Replace 'ponderation by' with 'weighting by' or 'multiplication by'.

P1481, Line 6. Not sure that use of 'with high accuracy' is justified here as this would require independent verification.

Interactive comment on The Cryosphere Discuss., 5, 1461, 2011.