

Interactive comment on “Assessment of heat sources on the control of fast flow of Vestfonna Ice Cap, Svalbard” by M. Schäfer et al.

Anonymous Referee #1

Received and published: 2 December 2013

Review comments on "Assessment of heat sources on the control of fast flow of Vestfonna Ice Cap, Svalbard" by M. Schäfer et al.

1. General comments

This paper presents numerical experiments on temperature and flow speed conditions of a relatively large ice cap on Svalbard. Full Stokes thermo-mechanically coupled model is applied to reproduce observed surface velocity field. A special feature of this modeling work is the inversion of the drag coefficient of the basal sliding law. Spatial distribution of this coefficient was computed by minimizing the cost function. A series of numerical experiments was performed to reproduce ice temperature measured in a borehole drilled interior of the ice cap. The sudden speed up observed in Franklinbreen was simulated with special attention on the effects of different heat sources (strain heat,

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friction heat, and firn heating) on the fast flow. This is a continuation of the authors' previous work (Schäfer et al., 2012), in which similar model was applied to the same region. The improvement from the previous work is the inclusion of thermodynamics in the model and prognostic experiments.

Basal conditions are important in the studied ice cap because of the fast flowing outlet glaciers. Thus, the combination of inverse modeling and full Stokes 3D thermo-mechanically coupled modeling is valuable to better understand the dynamic and thermodynamic structure of the ice cap. The validity of the model framework and numerical schemes are supported by the previous work (Schäfer et al., 2012). Nevertheless, the paper is not well focused on the subject appeared in the paper title. For example, a large part of the text is dedicated to the borehole temperature profile, which is not related to the fast flowing feature. I also found the experimental results are not well presented. For example, not much explanation is given to Figures 5 and 6, and the reason why those two locations are highlighted in Figure 6 is unclear. The aim of the prognostic simulations is also not clear. Accordingly, no conclusive interpretation is given for the role of each heat source in the fast flow condition. The authors state in Conclusion that firn heating is important, but advection of heat from the surface firn layer to the bed takes too long to explain the speed up in Franklinbreen. I also find the manuscript is not well balanced, i.e. experimental results (4.2 and 4.3) are very briefly mentioned as compared to the lengthy description on the model setup (3 and 4.1).

I list below my major concerns followed by more specific and technical comments. I hope the manuscript will be improved to more focused, better organized and concise paper, which meets the standard of publication.

1. Major concerns

(1) Main focus of the paper

The model is well prepared and a number of numerical experiments were performed. However, questions to be solved by the modeling are not clear. I expected the trigger-

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ing mechanism of the speed up at Franklinbreen would be discussed in the context of englacial and subglacial temperature regime. In the experiments, however, ice temperature field is driven by basal flow conditions estimated by the inversion of surface speed, rather than taking ice temperature as the control of fast flow. The role of the strain heat, friction heat and firn heating in the fast ice flow is discussed only in a qualitative manner, and it does not reach clear conclusions. The experiments to reproduce the borehole temperature provide an insight into the influence of temperature and surface melt on englacial temperature field. It might be interesting if the paper is focused on the effect of firn heating on thermal regime of a polythermal glacier.

(2) Sliding under freezing basal condition

It appears that the model allows basal sliding even when the bed temperature is below freezing temperature. I assume that the drag coefficient in the freezing zone is sufficiently large, but is this controlled in the model? It is not realistic if the model predicts fast sliding over a region well below freezing temperature. Terms "cold based sliding" (page 5121, line 17 and 19) and "submelt sliding" (page 5121, line 1; page 5124, line 27) are used, which give an impression that the model allow significant sliding over a frozen bed. Hindmarsh and Meur (2001) and Seddik et al. (2012) allow sliding at subfreezing temperature, but its magnitude decreases exponentially as temperature decreases below melting temperature. This point should be addressed properly.

(3) Surface and bed elevation

DEM reported for 1990 is used to reproduce the surface velocity field in 1995, 2008 and 2011 (page 5113, line 4). It is argued that surface elevation change is small, but ICE-Sat data are usually sparse in space. If the focus of this paper is fast flowing glaciers, accurate surface elevation is crucial. Using the same DEM limits the detailed discussion especially on the rapidly changing Franklinbreen. Sparseness of bed elevation data is also problematic (page 5114, line 14-18; page 5122, line 19-24; page 5123, line 11-13). Bed geometry is clearly important in fast flowing glaciers, and it is directly

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related to the basal conditions estimated in this study. I suggest the authors to present the surface and bed elevation data used in this study and discuss their influence on the results more in detail.

(4) Presentation of the results

The output of the 3D thermo-mechanically coupled model is complex. To discuss such results for a certain objective, the data set has to be processed and presented specifically for each purpose. Unfortunately, most of the plots just present computed values over the ice cap (Figures 2, 4, 6 and 8), and no further processing was performed. Moreover, the focus of the paper is fast flowing features, but only Figure 9 is prepared for this purpose. The readers would expect more of this kind of plot, and more direct and quantitative evidence of the authors' argument. For example, I am interested in englacial temperature structures of Franklinbreen before and after the acceleration, as well as in the fraction of basal sliding and internal deformation.

3. Specific comments and Technical corrections

Abstract: Abstract should be more focused on your work and achievements. The first paragraph describes the back ground of the study, and it occupies half of the abstract. Results are given in the last paragraph, but it does not convey the significance of the study. It should be restructured to be concise and efficient.

page 5098, line 28: "purely temperature dependent sliding law" is not accurate and sounds odd.

page 5099, line 13: Citing Iken's work (1981) here is not appropriate.

page 5100, line 19-20: "... even close to pressure melting point, i.e. that undergoes some form of basal sliding." » This is not clear to me.

page 5101, line 14: "... this approach allows neither ..." » allows investigations on ?

Page 5102, Research area and observational data: A set of figures explaining the

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geographical setting of the study site is needed. A part of Figure 1 gives the location of the ice cap, but no information is given for surface and bed elevations.

page 5103, line 28: "From 2008 to 2011 no large changes occurred," » There is a change at the southwestern corner. Is this due to a larger data gap?

page 5106, line 13: "climate mass balance" » Why not surface mass balance?

page 5106, line 18: "n and t are normal and ..." » n should be defined right after it appeared in line 14.

page 5106, line 18: "beta" is named as the "basal friction parameter" here, but as the "basal friction coefficient" in the same paragraph (line 20), and later called as "basal drag coefficient". Please be consistent.

page 5107, line 10-11: "a mean heat conductivity" » of what?

page 5107, equation (8): "." in the right hand side of the equation should be "dot".

page 5107, equation (10) and (11): » citations?

page 5110, line 22: "verified in (Schäfer et al., 2012)" » verified (Schäfer et al., 2012)

page 5111, line 4: "In the distribution ..." » This sentence is not clear.

page 5111, line 13-26: I find this paragraph is lengthy. I understand that steady state temperature solution was assumed because of lack of data for spin-up experiment.

page 5112, line 25: "small temperature dependency" » of what?

page 5114, line 8-10: It is not clear what is guaranteed by the short time step.

page 5114, line 22: "does not lead to visible improvements" » Can you show a statistic value? Observational DEM tend to produce unrealistic velocity variations, thus smooth surface may be required to estimate more accurate spatial pattern of the basal friction.

page 5115, line 12: "... , Fig. 7 (brown line ...)" » (Fig. 7, brown line ...)

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page 5115, line 15 and 17: "(close to) equilibrium", "quasi equilibrium" » Do you need to stress the solution is not in an equilibrium state?

page 5116, line 13-17: This sentence is too long and unclear.

page 5117, line 5: What are the mass balance and surface temperature conditions during the 30 year simulation?

page 5117, line 14: Do you assume the mass balance is constant from 1995 to 2008? What about temperature?

page 5118, line 6: "largely unaffected by temperature distribution" » No data are given to justify this statement.

page 5118, line 10-12: This sentence is not clear.

page 5118, line 18-24: I wonder how the authors can judge whether signals are artifacts or not. Uncertainties in the surface and bed elevation also influence the results.

page 5119, line 3: "ice core" » "borehole"

page 5119, line 12: "1920ies" » ?

page 5119, line 15-17: "the uncertainty in the basal drag coefficient strongly impacts ..." » Are you sure about this? The temperature pattern does not influence on the pattern of the basal drag coefficient (page 5113, line 20-22). Any sensitivity test?

page 5120, line 6-8: It is difficult to read this sentence without the definition of Pmax.

page 5120, line 17-22: This part is not clear.

page 5121, line 4: "importance" of what?

page 5121, line 9: "contribution" to what?

page 5121, line 11-12: Please refer Figure 5 to justify your argument.

page 5121, line 19: "cold based sliding" » Do you mean the glacier is sliding over a

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frozen bed? Or do you attribute this unrealistic result to the sparse bed elevation data?

page 5121, line 25: Isn't it obvious to get strain heating where ice is sliding?

page 5121, line 11: "0.06pm0.12" » Is this an error of the thinning rate? Please clarify.

page 5121, line 11: "reduction of the acceleration" » Do you mean the acceleration has reduced when the thickening rate decrease? The mechanism you explain here is not clear.

page 5122, line 25: "indirect impacts affecting the englacial hydrology" » Indirect impacts on what? What kind of englacial process?

page 5124, line 10-15: This sentence is too long. Please consider to rewrite.

page 5124, line 24: "sub-melt sliding Hindmarsh and Meur, 2001)" » Something wrong with the brackets.

page 5124, line 3-25: What is the aim of the two experiments (2) and (3)? Is it to see the response time of the ice temperature to the change in the basal condition? Is it for the justification of the steady state assumption? I rather expected the mechanism of the speed up in Franklinbreen is discussed with these experiments. I suggest the authors to set questions to be solved by numerical experiments, and provide discussion more focused on this question.

page 5124, line 26 – page 5125, line 11: I understand that spatially varying drag coefficient is important to reproduce the velocity structure of the study site. I agree with this, but the discussion on "a simple temperature based parameterization" is confusing to me. The sliding law employed in this study is a simpler version of Weertman-type sliding law (equation (8) in Seddik et al., 2012). The only difference is the coefficient is tuned in this work, whereas the other works take a constant value. This paragraph gives an impression that the sliding laws in the other works are wrong, but this is not true.

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page 5125, line 16-17: "Since the change from 1995 to 2008 is significant for future simulations," » What do you mean?

page 5125, line 25-: "Uncertainties in the basal. . ." » This sentence is not clear.

page 5126, line 2-3: "could be a trigger for speed-up" » What do you mean by "could be"? It is confusing because you already ruled out the firn heating from the triggering mechanism (5.3.2).

Fig. 2: Here and other plots, please provide (a), (b), (c) . . . for each of the sub plots.

Fig. 3, legend: "initialisation run with ideal T profile" » "initialization", What do you mean by "ideal T profile"?

Fig. 4, right: Is this the correction relative to the initial ice thickness in (%)?

Fig. 5: The unit in the plot is no correct. "C" » "degree C"

Fig. 5, caption: "The last figure remains unchanged whether . . ." » What do you mean?

Fig. 6: The stake number above the plot should be removed.

Fig. 8, caption: "Difference in change of surface elevation" » "Change in surface elevation"?

Fig. 9: Please provide a scale. It is hard to distinguish the lines delineating the sliding area when the paper is printed.

Interactive comment on The Cryosphere Discuss., 7, 5097, 2013.

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