The Cryosphere Discuss., 7, C2691–C2696, 2013
www.the-cryosphere-discuss.net/7/C2691/2013/

The Cryosphere
Discussions
Open Access

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

M. Truffer (Referee)

truffer@gi.alaska.edu

Received and published: 10 December 2013

In this manuscript Schaefer et al. use a finite element implementation of the Stokes equations to invert measured surface velocities for a basal friction coefficient. They analyze the observed speed-up of Franklinbreen, and show how it is connected with a decrease in the friction coefficient. They then analyze the influence of various heat sources. They conclude that several of the heat sources, most importantly firn heating and basal friction, greatly impact the basal temperature distribution, but they are unable to conclude on the role of these heat sources on the speed-up of Franklinbreen.

This is an interesting manuscript, it is mostly well written and reaches substantial conclusions. I recommend that it be published in The Cryosphere after some issues are addressed. These issues mostly concern the writing and presentation of the material,

and many suggestions are made below. I would place this somewhere between minor and major revisions. I would be willing to review a revision, but do not feel that I need to.

The authors might also find a recently published manuscript (Habermann et al., 2013, TC) interesting, where inversions for basal friction where made in the context of the retreat and acceleration of Jakobshavn Isbrae.

Parts of this paper are concerned with the thermal hypothesis for surge initiation, which, I believe, originally comes from Clarke et al (1984, J. Can. Earth Sc.). I wonder whether this is a bit of a distraction. The paper is not really able to contribute all that much to or against this hypothesis, so maybe too much emphasis is placed in the Introduction. Reading the Introduction the first time frankly left me a bit confused as to the goals of the paper. One of the issues is that there are glacier accelerations with quite different causes and symptoms: tidewater glacier retreats and surges. Tidewater glacier retreats are initiated at the glacier front and show a pattern of thinning, acceleration and retreat that propagates inland. Surges, on the other hand, are initiated farther upglacier, they propagate down with a wave of thickening ice, leaving thinner ice upstream. Franklinbreen is a marine terminating outlet glacier, so one could be left with the initial impression that its acceleration is like those of tidewater glaciers in Alaska or Greenland. But a look at the patterns of elevation change etc leaves one with the impression that it is behaving like a surge. The paper would benefit if this distinction was clearly made in the beginning of the paper.

More detailed comments:

p.5099 l.13: It's not clear to me what 'natural variability' refers to here. Are you refering to the 'tidewater glacier cycle'? In that case you should reference Meier and Post (1987, JGR). The Iken (1981) reference seems irrelevant in this context. l.16/17: the triggering of fast flow in tidewater glacier retreat and in surges is clearly different, at the very least in its spatial pattern. So you do limit yourself to some degree. It is not a bad idea to pursue general patterns, but here it leads to more confusion than insight. l.20: I recommend using 'basal motion', precisely to avoid associating a mechanism such as sliding or sediment deformation.

p.5100, l.10: I think an Alaskan reference would make more sense here, such as Kamb et al., (1984, Science), since that's where the hydrologically controlled surges were originally described

p.5101, l.28: You might better motivate the paper by stating here the reasons for assessing the evolution of temperature.

p.5102, l.6: IPY was the International Polar Year (not Geophysical)

p.5103 l.12: 2 cm is not a velocity error, it's a displacement error l.21-24: Why do you need to interpolate the data sets first? Can't your misfit functional just quantify the misfit to measurements in areas where you have them? Interpolating and then fitting removes the inversion one step from the original data that seems unnecessary.

p.5105, l.14: Appendix A does not serve a real purpose. These equations are stated in many textbooks and papers, and the only parameters given there are those of gravity and density.

p.5106, eqn(4): You probably don't want to redo calculations, but Cuffey and Paterson offer a more detailed discussion of A(T) that probably results in more accurate values.

p.5108 l.5: What is mu? l.20-23: How is this heat source applied? Is it a uniform source in the uppermost one, two, ? layers?

p.5109,l.9: Maxwell et al. (2008, J.Glac) might be an appropriate reference here, since they introduced this iterative method

p.5111 l.4-6: This sentence is difficult to understand. Please simplify. l.13-15: Same. Nested sentences, while I much like them, are best avoided in scientific writing, as they, after multiple readings, often still lead to confusion. l.24: Aren't you refering still to a

spin-up from deglaciated conditions? So surely, there wouldn't be air temperature and precipitation records for that long?

p.5112: l.9: The Seroussi reference deals with Greenland, and it is not clear that it would be applicable to surge-type glaciers. l.25: delete 'it was'

p.5113: top of page: It would be interesting how your mismatch between modeled and observed velocities compares to the mismatch that is expected from the velocity errors (see e.g. Truffer (2004, J.Glac.); or better, the book by Parker (Geophysical Inverse Theory)). The interesting thing to know would be whether you are able to fit velocities to within the observational error (avoiding overfitting, see e.g. Habermann et al., 2012) or whether the method does not find a solution for basal stickiness that fits the velocity data well. This would indicate other unresolved errors, such as those of the model (temperature, geometry, ...). The binning in the figure in Schaefer et al. (2012) is too coarse to assess this. You should also show on Fig. 3, where you choose your final solution. l.9-11: If I interpret the Schaefer et al (2012) sensitivity experiments correctly, than they address a uniform thickness change. I think it is also important to ask whether slope changes can make a difference. The thinning patterns shown in Moholdt et al. (2010) are quite uneven, and one could imagine that resulting slope changes make an impact. Can you address this?

p.5114: l.7: Why zero mass balance and not something close to observed? Zero mass balance will lead to thickening in the ablation area and thinning in the accumulation area that can quickly reach several meters and that would be compensated by surface balance in reality. l.16: You could use continuity arguments to make a quick qualitative assessment. If the glacier gets faster, it has to be thinner by continuity. If the bedrock data shows thinning and the velocity data shows slowing down, then the bedrock data has a problem.

p.5117, l.13: I believe you leave frontal positions constant for these simulations? You should say so.

p.5118: l.5: This is not shown in Sec. 4.1.1, it is shown in a previous paper. l.10-13: Sentence is missing a verb: are connected?

p.5119: l.5: It is not known whether there was an earlier equilibrium, is it? l.15: 'explained by various facts' or 'can occur for various reasons' l.21: 'to' -> 'than'

p.5120: l.4-6: I would interpret this exactly the other way around: To make an assessment whether a change in this heat source leads to changing basal conditions, you show that one would need to include several centuries of temperature data. l.8: what is Pmax? l.24: Such a comparison is not shown, but might be interesting to include. You only show that change in beta, but the reader does not know how v and tau change. l.26: I know this was shown in a different paper, but I'm somewhat baffled that the temperature distribution doesn't matter more. It affects viscosity greatly. If more insight can be offered here, I would greatly appreciate it. l.13: What do you mean by 'internal structure'?

p.5122, l.23/24: Can you really say that? Without an assessment of 100+ years of temperature variation, you have no basis for this statement. The glacier could react now to a change in firn heating that happened at the end of the LIA.

p.5123, l.9/10: Such a thickness change is also observed, perhaps not to that degree though.

p.5125, l.17: Do you think the sliding law is the problem, or the other unkowns that enter sliding relationships, such as the evolution of effective pressure?

Figure 1: The binning is strange with strange intervals and it is not clear at the upper end. White seems to indicate fast flow, but also missing data. What is the issue for 2011? The lower left shows very speckled fast flow and a separation from slow flow that looks too linear to be real.

Figure 2: What do the dots signify? Is it a reflection of mesh size?

Martin Truffer

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