Author response letter for "Importance of basal processes in simulations of a surging Svalbard outlet glacier"

The authors would like to thank the editor and reviewers for useful suggestions, feedback and guidance regarding the manuscript revisions.

Note that a bug was fixed pertaining to friction heat generation, and all simulations have been redone. All figures showing model outputs have also been redone. It should be noted that the only figure showing significant change from the original submission is the figure showing steady state basal temperatures (this is now Fig 8). This has some impact on the interpretation and discussion as described below.

1. Summary of changes necessitated by bug fix to code for friction heating at the glacier bed.

The original manuscript overestimated the contribution of friction heating at the bed. Correcting this has not had a significant effect on the simulated basal drag distribution. The main change is in the steady state basal temperature. Friction heat is now no longer sufficient to maintain pressure melting point at the bed in the steady state case with the 1995 danymics. Instead, the advection of heat downstream (ultimately across the ocean boundary) dominates in steady state. However, this is not as significant a change as it first appears. We have carried out a new transient simulation starting with a temperature distribution based on diffusion (i.e. the SS simulation as described in the manuscript) and run forwards using the 1995 derived basal drag coefficient and including advection. The bed temperature does not drop below pressure melting point on the century timescale, suggesting that the advection of heat is not critical on surge-event time scales.

The extra simulations are included in the section describing the temperature simulations. This section has been revised and modified extensively not only to reflect the changes and new simulations but also because the reviewers comments indicated that the motivation for this section was not clear in the original manuscript.

The discussion section where the temperature simulations are discussed has also been revised.

2. Response to reviewers.

Author responses are shown in italics.

Comment by M. Pelto

Gladstone et al (2013) provide an interesting and detailed analysis of the surge behavior of an outlet glacier of Austfonna Ice Cap in Svalbard. This paper is a pleasure to read. The model aids in providing an exceptional discussion that probes the potential aspects of surging, in this instance so well displayed in Figure 8. Field data alone cannot provide such insight. The most important point that needs more attention is the location of the ELA and various facies derived from various melt levels, versus the initiation of the main zone of speedup spatially. This could be displayed on Figure 3.

We've read the papers suggested by Mauri Pelto (thanks!). They summarise some relevant observations. But they also show that the firn extent and ela are highly variable (much change between 2004 and 2007 for example in one of the papers). Also the speed up of the B3 outlet glacier doesn't have a localised zone of initiation, at least not one that can be seen in our satellite data (or in the line of measurement stations of Dunse et al either). So I don't see that drawing the ELA location will add significantly to any arguments presented.

5826-6: A better Figure 1 identifying characteristics of Austfonna and B3 is needed this might be the place to refer to it.

We added a new simple figure 1 just to show the icecap geometry and label the location of Basin 3 to help set the scene. I'm not sure if this is helpful?

5827-7: Seems worth directly noting that the lower increase in velocity during a surge should necessitate a longer surge phase.

Added a sentence accordingly.

5827-10: Is this not always the case that surge speeds are much greater not just in Svalbard.

Yes I would think so, and here in this sentence we clarify that this is due to sliding. Maybe this will be obvious to most readers, but I think better to state the obvious than to lose potentially interested readers with no background in surging.

5831-18: How do results here and methods compare to Bevan et al (2007) Figure 3?

Our 1995 velocities are very similar to their Figure 3. I think the methods for deriving velocity from SAR images is very similar. We added a few lines explaining our methods in slightly more detail.

5837-6: Where is the fast flow initiated compared to the ELA or the facies noted by Dunse et al (2009) in their Figure 6. It is worth correlating the observed melt regions with the observed region of speedup.

It is very hard to say anything about this as the ELA varies a lot through time (I guess due to interannual variations in precip) and also there isn't a strongly regional initiation of speed up. It seems to occur all along the glacier (see Dunse et al recent papers). I believe T. Dunse and colleagues are continuing to collect high resolution data of the speed up and there may be future publications from that group that shed light on spatial patterns of speedup. But from the satellite data that are available to us for the current study, there doesn't appear to be an obvious region of initiation.

5837-18: Does the limited degree of winter deceleration indicate anything about the residence time of water in the till?

The winter deceleration tells us something about residence time of summer meltwater at the bed, in whatever mode of storage that happens to be. And going by the observations of Dunse et al 2012 this would appear to be weeks to months. But if this indicates a till residence time then what can be causing the interannual speedup? So we are speculating that much of the summer meltwater doesn't fully penetrate into the till, but rather pools at the ice-till interface. In other words we are suggesting that the till penetration time is longer than the ice-till interface residence time. Does this make sense? We may not be right – perhaps the summer meltwater does penetrate the till, and perhaps the till has a variable residence time which is increasing as till properties change... this doesn't seem very likely to me, but however you look at it the exact mechanisms at the bed must remain speculative and inconclusive for the time being.

5838-2: Worth pointing out the ELA in a few years such as 2003, 2004, 2005 and 2011. The AAR for the Basin 3 was certainly very low in a few of those years, which would be quite a change for the hydrologic system. Moholdt et al, (2010) note low AAR for Austfonna 2003-2005.

But are years of low AAR a feature of recent climate change or have they been happening periodically for centuries? We don't have the data for this, so any discussion about impact of low AAR years would be highly speculative. We've added a couple of lines to the discussion in which we mention the low SMB in 2004 from the Moholdt paper, but we don't feel any conclusions can yet be drawn from this.

5839-11: It appears to me that the velocities do not indicate a slow down near the coast, resulting from thinning induced heat advection in B3, if so this suggests this mechanism is limited in the face of higher water delivery to the bed and higher velocities.

I think some kind of misunderstanding has occurred here, though I am not exactly sure what! You are right that the velocities do not indicate any kind of slow down, near the coast or anywhere else. As far as we are aware, B3 is still accelerating. We are speculating on mechanisms for surge shut down (it has to shut down at some point!), and one negative feedback is that of heat advection away from B3 due to fast flow. In any case, this section has been rewritten, and I hope it is clearer now.

Figure 1: Not a good base map for the setting of B3 including elevations.

We provided a new fig 1 to show the surface elevations and location of B3.

Figure 8: Brilliant, just refer to it earlier during the discussion.

We refer to this figure in the first paragraph of the discussion.

Anonymous Referee #1

Received and published: 2 February 2014

Gladstone et al describe numerical ice flow experiments of Austfonna Ice Cap in Svalbard focusing on the flow of Basin 3, one of the fastest outlet glacier of Svalbard and suspected to have surge-type behavior. Their objective is to link observed changes in ice speed and possible surge-type behavior to basal conditions and processes. The paper is interesting and easy to read and provides some information about the state of the bed. Nevertheless I think the paper would benefit from more analysis and detailed simulations. The results are somewhat ambiguous and don't provide clear-cut evidence for what is happening at the bed of Austfonna. Some discussions regarding till behavior read like if till was included in the model when it isn't. Suggestions are given below in comments.

It is true that this study does not provide a proof, or even very strong evidence, for the processes governing sliding at the bed of B3, ASF. This is not our aim. Our aims are to demonstrate the problems with neglecting such processes and to present simulations that can inform the discussion on such processes. Determining the correct set of processes is a big problem, and may not be solved so easily!

We have added a few simulations (in the section on temperature), though it is not clear whether the reviewer will find these helpful or not.

Abstract 9-10 I could not find results about the 3 steady state temperatures for the three time periods. These results would be interesting to include. Is there an increase in the area of warm bed with time? Does that match the area of fast sliding/surface speed?

We now show the 2011 steady state basal temperatures as well as the 1995 ones, and try to provide a clearer discussion of the relevance of the temperature simulations. As for the warm bed over time... this is now discussed more clearly along with the implications of the steady state assumption.

Abstract 15. Why use the word 'error'? Why not use 'difference' since your are not checking against data here.

Well, they can't both be right, so at least one of them is in error. Probably they are both in error. Which is most in error depends on the real future evolution of the B3 outlet glacier, which is something we don't know. I think error is a stronger word than difference, so saying there is an error of that order of magnitude is a stronger statement. The point is that simulations that don't represent the relevant basal processes are certainly in error when they try to make predictions about surging glaciers. We want to make it clear to readers that forward simulations of potentially surging glaciers are in error if evolution of basal conditions is not represented.

Abstract 17- Some specific conclusions about feedback mechanisms would be welcome here.

We've modified this sentence to relate more directly to our discussion.

p5827 You indicate that Svalbard glaciers surge phase is longer and velocities (relative to quiescent phase) not as high as in other surge-type glaciers. Can your simulations and feedback mechanisms explain why Svalbard glaciers behave as such? Can you state later in the discussion some hypotheses or directions for further work?

Comparing to other regions is not really the focus of this work. Here are my "spur-of-the-moment" thoughts, but I think the paper is already speculative enough that we don't want to start speculating in another direction (also I don't think our simulations really inform on comparisons with other regions, though we could potentially set up some such simulations as the focus for a new paper if there seems to be interest). I think small surging mountain glaciers must be very different- you aren't going to build up much basal sediment due to the aspect ratio, and once a surge starts the high driving stresses (due to steep slopes) are going to lead to massive velocity increases. So I think the key may be in aspect ratio and bed composition. But I'd need to do a lot more research into other surge type glaciers before I could be confident enough to write about them in a peer reviewed journal!

My main current focus is Antarctica, and I think there are some interesting parallels between B3 and some of the Siple coast ice streams. So perhaps a more pertinent comparison would be between B3 and the Siple coast ice streams. Why don't they surge? Well, perhaps they do in their own way, but on longer time scales. Perhaps the mechanisms leading to surging in ASF lead to ice stream switching in the Siple coast, due to different geometric constraints.

But to get back to the point, these are all interesting directions to take in future work, but I do not feel qualified to comment on the differences between B3 and other surge-type glaciers at the present.

p5828 18. How can your simulations guide future till model development? I could not see a discussion of this later in the manuscript.

The last part of the conclusions section was intended to point the way for further development. But it was pretty vague, so we've added a sentence to make the last paragraph of the conclusions more explicit in this respect.

p5830 21. Can you indicate what year that surface elevation was taken from?

The surface elevation we use comes from a map published in 1998 by the NPI. I don't know which exact years surveys of ASF were carried out that contributed to the map. There is some general information on NPI mapping of Svalbard here:

http://www.npolar.no/en/themes/mapping/svalbard.html

If it is essential to have further information about the origins of the map then I can try contacting the NPI to find out more. However, I don't expect our simulations to show a strong sensitivity to modest changes in surface elevations.

p5832 9. You use C earlier for basal drag coefficient and now.

We have changed this to use C everywhere.

p5832 9. It would be interesting to see how the distribution of matches areas of the bed that are at the melting temperature for both 1995 and 2011 (and may be 2008 to better see the evolution, see comment on abstract).

There are new simulations and figures in the temperature section, but bear in mind that the steady state basal temperature distribution is not a good guess for the real distribution once the surge has started.

p5832 21. A figure of just $_b$ and ice thickness would be very useful. No numbers are given so it's hard to get an idea of the absolute values of the basal shear stress. Would be extremely interesting.

We have changed around the figures somewhat. We now show the basal shear stress as well as the driving stress – shear stress difference. We show surface elevation in the new fig 1. We don't show thickness explicitly, but you can get some idea from the 3d plots. Perhaps these changes are helpful?

Figure 4. Does the pattern of changes if the mesh resolution is increased? Looks like variations of are occurring over single elements.

We have run the simulations on different meshes, though only one is shown. There are not significant changes. We now show a figure of just the 3D mesh showing more clearly the refinement over the B3 outlet glacier (previously we had the velocity and mesh on the same plot which may have obscured the mesh refinement somewhat). Some of the smaller features comprise 2-3 elements. These appear to be robust to mesh changes. Note there is also some sensitivity to the regularisation parameter in the inversion, which effectively applies a certain level of smoothing. But however you look at it, the 2011 velocity field is noisy, and small features in the velocity field (which are within the error range) can be captured by the inversion. I think that although we give brief mention to these features we should best keep our main focus on the larger scale patterns (as it is currently) which are clearly neither artefacts of the data or the modelling process. The smaller features are not central to our discussion or conclusions.

p5832 10. "Basal sliding ...". That sentence seems out of place since that can't be deduced from the figure which shows only the basal sliding coefficient.

I have added "(not shown)" to this sentence to imply that this is indeed a model result even though it cannot be deduced directly from the figures we have shown (as you point out).

Would you like me to add a plot showing the difference between the upper and lower surface velocities? This would be pretty awkward to do in practice, due to the unstructured mesh, and might require redoing some simulations, but we could find a way if it was felt this was needed. Showing a plot of both the surface and basal velocities next to each other would be straightforward. Personally I think it is an expected result, and therefore simply stating it is sufficient. If it was in some way surprising that surface and basal velocities were similar then it would be more important to show the results that support this.

p5834 8-9. Don't start a new paragraph for this lone sentence.

Ok, changed.

p5834 10-13. 100 years seems short for temperatures to reach steady state. Some back-of-the-envelope calculations on the diffusion and advection time scale would be useful as indicators.

Actually you are right that this is a short timescale for steady state. We now suggest in the revised text that the 1995 temperature distribution should be somewhere between the simulated no-dynamics steady state distribution and the simulated 1995-dynamics steady state distribution. In fact the real *temperature distribution, were it known, might contain some interesting information about the surge history.*

p5834 16. Is this a temperature only simulation? If this is then I don’t see the usefulness of it.

The point of the temperature simulations is to provide sensitivity experiments to investigate relative impacts and timescales of thermodynamic processes. We've extensively revised and enhanced this section to make the relevance of all temperature simulations clearer. See the discussion also.

p5838 12-13. 1995 and 2011 model calculations indicate a reduction in the value of $_b$ under B3 (would be nice to see figures of it). If till is present this means higher water pressure in 2011 caused by more water at the bed as mentioned. I don’t follow the argument that this water comes from basal friction. This would imply that the ice is sliding faster but then what caused that? This looks like a circular argument to me. Seems to me that the water must be coming from the increased melting at the surface (warmer temperatures in the 2000s).

tau_b is now shown in the figure with the stress difference.

The phrase “circular argument” is actually not all that different from “positive feedback”, which is what we are getting at here. A positive feedback cannot make meltwater from nothing, but can enhance it once it has started. We are suggesting a gradual change during quiescent period leading to a threshold (at which the bed reaches pressure melting point) beyond which positive feedbacks relating to sliding kick in. I've read this text again and I still think it is clear as it is. Note that the later discussion has been extended, and our hypothesis about the gradual build up leading to a thickness threshold should now be clearer in both the temperature simulations section and in the discussion.

We've also slightly extended the discussion on relevant observations about changing surface conditions, but it would be very speculative to say that the water for the speed up has to come from surface melting. Certainly some water comes from surface melting, during the melt season, but the system also has its own internal feedbacks. I don't think we have enough information to say whether this surge onset was caused by internal cyclicity or changing climate.

p5838 14. If till has been deforming for some time then the argument of overconsolidation does not hold: the till is in critical state and no longer dilates. I don’t understand why the till would be overconsolidated in 1995 more so than at other time periods.

If I understand right (I am new to the concept of consolidation and overconsolidation): we would expect a till to be normally consolidated to the pressure it is under. If this pressure reduces (due to an increase in pore water pressure due to in situ melting, for example) the previously normally consolidated till would become over consolidated (the till hasn't changed, it is just the pressure that has changed). So if we hypothesise a quiescent phase in which the till is normally consolidated and has little or no pore water, this till would become overconsolidated during the onset of a surge when the water pressure increases and effective pressure drops. This is why the till might be overconsolidated in 1995, given the evidence for lack of motion in the 80s. It is rather speculative, but does it not make sense? If I have misunderstood the consolidation process, please explain the errors in my argument.

If the till has been deforming slowly for some time with a fairly constant (and low) water pressure, it may well be in a critical state. But this does not preclude further dilation (though it may preclude dilatant hardening). The critical state is a function of water pressure. If water pressure increases (perhaps due to increase of water generated in situ by friction heating) then the critical state will have a higher porosity and lower yield strength.

Having said all this, I am not convinced that discussing consolidation really adds to the discussion, so I've removed the relevant lines.

p5838 16-19. This sentence should be rephrased to say first that the highly variable basal stress is due to the noisy surface velocity field which may be due to sticky spots. Could the noisy stress field be due to the coarseness of the mesh (see comment above)?

We have rephrased this accordingly.

p5838 26- I don't understand the reasoning here. Both simulations SS and SSNG have the same ice thickness so how can one say something about the link between bed temperatures and the gradual thickening of ice? Results of SSNG are obvious: lower basal heat flux implies lower bed temperatures. I think these two runs (without advection) don't provide much information. It would be more interesting to run full thermo-mechanical simulations with the geothermal heat flux halved.

Actually, the above statement (lower basal heat flux implies lower temperatures) is not obvious at all. It certainly holds for ice below the pressure melting point, but if you reduce the heat source for melting ice it is not at all obvious whether the ice will remain at pressure melting point but with a lower melt rate, or stop melting altogether and drop below pressure melting point. The point of these simulations is to show that the threshold between melting at the bed and no melting at the bed is within the bounds of uncertainty given the poor constraints on geothermal heat flux. We have tried to make this point clearer in the paper.

p5839 3-. There is something strange about Figure 7 bottom left: in that simulation, fast ice is observed where the bed is cold, something that is highly unlikely. What causes this effect? Can you reconcile the inversion model (low values of under fast moving ice) with the 'cold spot'? I don't understand how advection of warm ice by sliding would make the bed cold where it's moving fastest (given that there is no change in ice thickness in all models presented on Fig. 7).

This is a sensitivity study. It is a steady state temperature simulation with rapid velocities. On long timescales the advection of heat to the ocean dominates over friction heating. This section has been much revised, hopefully it is clearer now. Note that we are not suggesting that this is a real state that we would find the icecap in.

p5839 15. Change 'errors' to 'differences'.

See earlier response.

Fig. 7. Is there significant differences in ice thickness (and thus pressure) that it would be better to plot the temperature relative to the melting temperature (corrected for pressure) to see exactly the extent of the warm bed?

All temperature plots are now relative to pressure melting point at the bed.

Anonymous Referee #2

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This paper puts forward the importance of evolving basal properties in numerical simulations of ice flow. Sensitivity experiments are applied to the Austfonna ice cap, a well-suited study site, which displays both seasonal, and annual flow variations. The manuscript is generally well written. However, although I fully support the overall aim of the paper, the connection between the various analysis presented is not always obvious. A more careful description of the reasoning and use of model output would help strengthen the manuscripts 'conclusions. Below are my suggestions and comments.

We've tried to make the role of the simulations clearer and more pertinent to the discussion, especially the temperature simulations.

(l.28, p. 5828) It is not completely clear, how the present study can guide till model development -

Probably best to stay in line with the conclusions as stated on lines 24-27, p.5839).

The conclusions are now slightly more specific in terms of recommendations for future model development, following on directly from the discussion.

- I would suggest clarifying the goals and approach at the end of the intro. There is currently a bit of an imbalance, with a strong motivation given for the transient (basal stress) experiments, and virtually no mention of the steady state (temperature) runs.

We've tried to make the last part of the introduction a bit clearer and more balanced.

The link between the two sets of sensitivity experiments is not obvious as it stands.

Actually, I don't think there is a strong link between the transient experiments and the temperature experiments. The point of the transient experiments is to demonstrate that significant errors arise by taking the commonly used approach of prescribing Weertman sliding with a fixed drag coefficient. The temperature experiments are more aimed at informing the discussion about processes affecting sliding.

This should now be a bit clearer from the last part of the introduction. Also, the section on temperature simulations has been revised and extended and is hopefully clearer now.

-p.5833: How different are the SS temperature calculated for 1995 and 2011? If they are similar, then perhaps add a sentence to make this clear. Otherwise, aren't you possibly using fields that are not consistent with each other? For instance, during the temperature inversion, I would expect the amount of basal drag to directly affect basal sliding and thus the temperature distribution in the ice. I see your point (trying to isolate the basal drag effect), but still think that it would have been cleaner to run the experiments with the "complete set" of initial conditions obtained respectively for 1995 and 2011. I suspect your conclusions would remain the same. Alternately, you could show a timeseries, of thickness/vel, averaged over B3, to support the inference that the "mismatch" between temp field and basal conditions do not influence significantly the model output (e.g., are there any "jumps" in variables early in the experiment??).

The temperatures have very little impact on the simulations because motion is dominated by sliding, and in the model we do not yet have temperature dependency in the sliding law. I made an animation of the transient simulations (not shown) and there are no jumps in thickness or velocity. I have also run transient simulations with temperature evolving and using the corresponding temperature fields for initialisation. There are arguments both ways when it comes to setting up sensitivity experiments – isolate one aspect for comparison or try to be self-consistent within each simulation. Luckily for us it makes a negligible difference! I don't think showing a time series would enhance the paper, but I can do it if the reviewers/editor feel strongly that it should be added.

-l. 26-27, p.5838: I do not see how you can draw conclusions from the steady state simulations to explain the theory of bed reaching Tpmp and trigger for the surge phase. Moreover, it is not clear from Figure 7 bottom right, that the inclusion of sliding/advection reduces the temperature throughout the fast flow area – The text needs clarifications, and/or use different color scale on the Figure.

We keep the same colour scale so that all sub plots are directly comparable with each other.

This section has been extensively revised, so hopefully the motivation and logic is now clearer. In particular see the paragraph on threshold behaviour early in the temperature simulation section.

Note that the effects of advection are shown most clearly in the bottom (now centre row with the new plots) left subplot rather than the right one. I've double checked the figure caption and it seems clear to me.

-Discussion on till mechanics (section 4): If the till has been deforming for some time, you may assume that is it in a critical state. In that case, dilatant hardening would not be expected to play a key role (Iverson, 2010, page 1107 for example). Consider removing the discussion related to dilatant hardening.

This is a good point, though dilatant hardening may be relevant at the start of a surge. This might delay the start of the surge. We added a line to indicate that dilatant hardening is not likely to be relevant after the surge onset.

-l. 18-19, p. 5839: The statement that “water drains in efficient channels without penetrating significantly into the sediment” seems in contradiction with earlier statement of evolving till properties (l. 15-19, p.5837), and needs to be rephrased.

There is no contradiction here. We are suggesting that the seasonal melt discharges through efficient channels at the ice-till interface. This is the meaning we aim to convey in both of the sentences that the reviewer highlights. If the reviewer would like to say which of these sentences in in error we could look at re-phrasing it. But having re-read both sentences now I see no contradiction.

-l.18, p.5827: “In this respect”, capital “I”

This is capital in our latex source file. A Copernicus formatting issue? Or latex bug?

-l.28, p.5827: Clarke 1987 and Tulaczyk, JGR 2000a should also be cited here.

Ok, done

-p. 5829 / p. 5832: inconsistency in variable name for the basal drag coefficient (C/beta)

We now use C throughout.

-l. 13-14, p.5833: It is not clear to me, what you mean by “Forcing is approximately present day”. Climate forcing?? Or do you mean that initial conditions corresponds approx. to present day conditions?

Well, the initialisation and forcing is described precisely in the following two paragraphs. Initial geometry and smb, which is the main climatic forcing. Also surface temperature is kept constant. We've modified this line slightly and added a line to the smb paragraph to aid clarity.

Figures:

-Fig.1 and 2: use km for your UTM x/y coord.

done

-Fig.1: Those are presumable the winter velocities?

Yes, modified caption

-Fig.2: could you add contour lines of the modeled velocities, for comparison?

We've reorganised the figures a bit. We now show velocity contours in Fig 5. We wanted to introduce the observations before showing model results.

-Fig.3: the colorbar is tiny!

We've tried to make the colourbars a good size for all figures.

-Fig.4, 5, 6: for added clarity, split the panels; enlarge the colorbar and move outside the colored map.

The application we use fr analysis, Paraview, doesn't allow for whitespace between the panels. I've tried exporting the panels individually and putting them together in Powerpoint with whitespace between them. It isn't perfect, but hopefully it is sufficiently clear now.

-Fig.7: move the colorbar (as above), and consider changing the colorscale for improved clarity on temp distribution for RHS.

We want to keep the same colourscale on our subplots to facilitate comparison. I think the colourscale is pretty clear. Not sure how it could be better? The large red regions are all at pmp, so we can't expand the colour scale there.