Response to reviewer comments

Excess heat in the Greenland Ice Sheet: Dissipation, temperate paleo-firn and cryo-hydrologic warming

We appreciate the valuable comments of all three reviewers.

One point raised questions from all reviewers, and is discussed in the following. The remainder of the comments is answered below.

Common comment

page 5169, Title: "Excess heat"

Reviewer 1 and especially reviewer 3 don't like the expression "excess heat". We concur that the title was probably too catchy and that there is a problem, especially in light of the arguments of reviewer 3. In that sense, it might be more appropriate to change the title to **"Heat sources within the Greenland Ice Sheet: dissipation, temperate paleo-firn and cryo-hydrologic warming"**. Further occurrences of "excess heat" have been changed to "extra heat" throughout the manuscript.

Reviewer 1

Specific comments: Title: excess heat . . .: excess heat compared to what?

The title has now been changed to "Heat sources within the Greenland Ice Sheet".

Abstract: it seems that you have a normal state of a glacier in mind, and then introduce the deviations (excess). However, what is the normal state? Every glacier is flowing, produces strain heating (dissipation), has a history (paleo-firn?), contains a hydraulic system and crevasses (cryo-hydrological warming).

We think that it is clearly stated in the abstract that the "excess heat" is with respect a model run with heat diffusion and dissipation alone. We have changed this (and all other occurrences) to *"extra heat"*.

p. 5170, line 20: the five drill sites of Thomsen (1991) are not even approximately situated along a flow line downstream of Swiss Camp. They seem to lie on flow lines crossing a mountainous region near the margin and thus are warmer likely because the cold advection of inland ice is reduced substantially.

Agreed. We now write
five in the ablation zone of the Paakitsoq area, downstream of Swiss Camp

Furthermore we now show the locations of the Thomsen drill sites TD1-5 on the map in Figure 1.

p. 5171, line 5: the two temperature profiles may be very similar, but why should they agree with each other?

The model calculates ice-sheet conditions on the West-slope of Greenland, starting at the ice divide. Conditions of a flowline entering Jakobshavn Isbræ and on a flow line moving through Swiss Camp are similar, so we would expect that these temperature profiles are similar. We now write
is similar to instead of *agrees well*.

p. 5173, line 11: 5.6 kg is very little of course, however, is the water also readily available for the entire ice column below 1 m2? How does this compare with the 0.31 K per century heating rate on p. 5174, line 3, for strain heating?

We never claim that the water is readily available within the ice. Indeed that is the main part of the puzzle about the ice temperatures, since water can only penetrate into the ice through cracks, which is discussed in the Discussion (nothing changed).

p. 5175, line 18: refined to a coarseness sounds a bit contradictory, would resolution not be more adequate than coarseness? What is the resolution elsewhere?

We changed this to "resolution", and added a sentence on mesh resolution
The unrefined mesh consisted of 100 elements in the vertical, corresponding to a vertical mesh size of 20 to 7 m, depending on total ice column height.

Figure 1: please connect the points in the upper panel with lines (as you do in all other figures and panels). This makes it easier to read the figure.

The figure is actually harder to read with lines, since especially the red dots have varying values (nothing changed).

Figures 5-7, upper label of lower panels: supply indicates a heat flux rate, but you give a heat content (density)?

Agreed, "supply" should rather be "content". Changed to "Extra heat" in figures 5-7.

comments on the heat flow model: p. 5174, lines 12-19: do I understand correctly that you solve the transient equation within a block of ice moving along the flow profile. Is the solution for the flow line profile as a whole a stationary solution? How do you handle the upstream and downstream boundaries of the block of ice?

Exactly. The solution along a profile is not stationary, but time-dependent. The block is deformed to comply with the local ice thickness, and all thermal features (e.g. crevasses) are stretched accordingly.

At the upstream and downstream sides we prescribe a zero heat flux boundary condition. We now state this with

On the upstream and downstream faces a zero-flux boundary condition was prescribed.

p. 5175, lines 3-4 and lines 21-24: Dont you change the advective heat flow with neglecting vertical shearing? Are the analytical solutions of Vorkauf (2014) one- dimensional solutions? If yes, do they really test the omission of the vertical shearing?

Vertical shearing is only important at the base. There, we use the theoretical heat production rate due to vertical shearing, based on the shallow-ice approximation (i.e. Equation (2)).

The analytical solutions for the model test are one dimensional (in the vertical) and only test the vertical heat transfer and the moving-boundary problems.

We added a statement that heat production due to vertical shearing is included
Vertical shearing was neglected (i.e. $\dot{\varepsilon}_{xz} = 0$*) so that the block moved in plug-like flow, whereas dissipative heat production according to Equation (2) was included.*

Acknowledgements: do not acknowledge an author (M. Funk)

Acknowledgment deleted.

Reviewer 2 (Martin Truffer)

1) Model description: I would appreciate a little more details on the model. As I understand, the model initial state is a 1000 m long block centered at 450 km with an initial temperature taken from Funk et al. (1994). How exactly is the block moved? Do you move it with the center velocity, then determine the vertical stretch from the new ice thickness, and then stretch horizontally to maintain incompressibility? So there is an assumption of steady-state in terms of ice thickness? I believe that the ice temperature diffusion is only solved in the vertical direction, correct? That is, all heat sources are applied evenly across the horizontal dimension? When you state results for TD5, FOXX, and GULL, are they, strictly speaking, at different times, or do you do separate model runs, so the ice arrives at these sites at the current time? It seems that the difference between FOXX and GULL is more than 100 years, so this could matter.

The model solves the heat diffusion equation in a Lagrangian coordinate system in 2 dimensions (this is stated in the model description) (and in 3 dimensions in the moulin experiments by Vorkauf (2014), not shown here). The block is stretched horizontally and vertically such that its thickness (plus surface balance) always agrees with the local ice thickness. And yes, we assume a steady state ice thickness distribution along the flow profile.

The heat sources are applied evenly at the surface or the bed, but are 1D crevasses in the 2D model runs shown here, or 3D features (moulins) in the model runs by

Vorkauf. These features are stretched and compressed according to the ice deformation, and are the reason we used a fully Lagrangian description in the first place.

When the block arrives at the drill sites the temperature profiles are extracted. So yes, the results are at different times. Since surface conditions are assumed steady (except for the warm phase) this does not matter. The ice accumulated during the warm phase 1570-1730 has been melted away at the surface once it arrives at GULL.

We have performed additional runs with time-varying surface conditions, driven by GRIP temperature reconstructions, but could not achieve any better agreement between modeled and measured temperature profiles. So, the necessity of important heat sources within the ice is a robust result an cannot be due to the temperature history.

2) Discussion: There is room for expansion here. You point out the difference of the two FOXX profiles. Since you already have the model, why not elaborate on that? How long would it take for these temperature differences to dissipate, i.e. can you say something about the local nature and the timing of the heat source that would create such a large difference? Are these differences contrary to your discussion about very local heat sources from refreezing water in moulins?

We added a paragraph discussing the FOXX profiles, which, however, cannot be reproduced with any simple distribution of heat sources.
A noteworthy feature is the temperature difference between the two profiles at site FOXX (Fig. 2) in boreholes which are in 86 m *distance of each other. The temperature difference amounts to 4* K *in 200-300* m *depth, but the profiles intersect below 300* m *depth. Obviously, only a combination of very localized heat sources can produce such temperature profiles. No simple heat source patterns, nor the influence of active vertical moulins or horizontal conduits produce similar temperature patterns (Vorkauf, 2014). The observation of very different temperature patterns in neighboring boreholes cautions against interpretation of single boreholes in areas where englacial heat sources strongly affect the thermal structure of the ice.*

3) Discussion: It might be interesting to the reader to at least shortly discuss the results of Phillips et al. (2013). I realize that they dont do the same thing you do, but they look at the same area, albeit concentrating on recent changes higher up. But they use steady-state assumptions for their temperature calculation; your model results might give you some opinion about the validity of their conclusions? Or not?

We have added a paragraph discussing the relation to the earlier modeling study by Phillips et al (2013)
An earlier modeling study of thermal properties in the same area (Phillips et al, 2013) concluded that ice warmed by cryo-hydrologic features has an important influence on ice deformation rates, and is causing the increasing flow velocities observed at the surface. The main difference in modeling approaches is their assumption of

continuous heat sources spread over the entire ice thickness. Neither our data nor the interpretation with the heat flow model support this assumption. The ice in the lowest 200 m *of the ice column shows no signs of warming through cryo-hydrologic features, as the temperatures there can be explained by diffusion and dissipation alone, as shown in Figure 5. Since vertical shear deformation is highest at depth (Ryser et al, 2014a,b), temperature changes in the central ice body will have a minor influence on ice deformation rates, and therefore surface velocities.*

4) Discussion: You cite Table 9.1 in Duval and Schulson for the temperature dependence of the fracture toughness. I believe this is not the correct table. It makes a point about crack-tip loading rates for the measurements of fracture toughness. The relevant data is in Fig. 9.4 and shows a very small dependence of KIc on temperature. You might therefore want to rethink that conclusion.

This comment is certainly valid, and there is barely any temperature dependence for fracture toughness at static loading, so our original explanation was wrong.

However, crevasse penetration, driven by water-filled cracks, is a dynamic process with a locally increasing loading (through the moving water column as the crack propagates). So we think that Table 9.1 still gives relevant information. Critical loading rates are higher in warmer ice by orders of magnitude, thus precluding crack growth. We now write
The critical crack-tip loading rate at $-20°C$ is orders of magnitude lower than at temperatures approaching the melting point (Schulson and Duval, 2009; Tab. 9.1. We therefore suggest that water-driven crevasses stop their downwards growth once they reach warmer ice.

5171, l.14/15: I would just state that you analyze temperature profiles from new boreholes as well as some old measurements without mentioning locations (they dont mean anything to the reader at this stage, and you introduce them in the next paragraph)

Sentence shortened as proposed.

sec. 2.2: Could you state how long it took for measurements to be equilibrate?

We added a sentence
Depending on ambient ice temperature, the readings of temperature equilibrated to their undisturbed values after one to three months.

5173, l.5: value for C_i, should be $kg{-}1$

Thanks for spotting this stupid mistake. Changed.

5173, l.19: the assumption is SIA and n=3

Added.

5178, l.20: Whats sluggish ice. Do you mean slushy?

Changed.

Table 1 seems superfluous, you dont reference those runs that often. Fig. 7 caption: just state what the model run is, rather than a code that has to be looked up in a table.

We think that the table and the designations are still useful to clearly distinguish between the model runs. Additional explanations have now been added to the figure captions.

Figure fonts are generally small

Figure fonts have been enlarged in Figures 2, 3, 5, 6, and 7.

Reviewer 3

1) Excess heat. The papers title, Excess heat in Greenland Ice sheet. . . ., and numerous points throughout the text, state that Greenland ice has extra heat. I disagree with this formulation the ice has absolutely no excess heat. The conditions in the ice are what they are, and the disconnect between models and measurements is entirely due to underperforming models. This comment may seem like nothing more than a knit-picky criticism of wording, but I think there are deeper and important implications here. For some readers, the excess heat implies change related to a warming climate. The impact of these heating processes on ice temperature may certainly be increasing over time, but this study does not address temporal change in heating mechanisms or thermal profiles. Further, latent and strain heating processes should be important whether the climate is warming, cooling, or in steady-state these are fundamental processes to any ice sheet with ablation and percolation zones. The key finding here is that we have thus far failed to incorporate those processes into our modeling, and this analysis shows just how important they can be.

We concur that the title was probably too catchy. We have changed the title to "Heat sources within the Greenland Ice Sheet:..." and now use "extra heat" when the context is clearly stated, i.e. when compared to the reference model run with diffusion and dissipation alone.

2) Surface boundary conditions All model simulations use a prescribed surface temperature boundary taken from Funk et al., (1994). If Im not mistaken, this boundary condition was derived from a very early and preliminary assessment of surface climate. No real justification is provided in the paper for this boundary condition. The choice seems odd since there are other more recent datasets/model outputs that could have been used, and some would argue they are much more realistic and justified. Perhaps this is a detail worth considering, since the first thing that jumps out in the model v observation plots (Figs 5,6,7) is how much of this mismatch is due to a *surface boundary condition that appears to be way off? At first inspection, it looks as if a better surface boundary condition could do a lot to make the model output better match observations. At the very least, this issue needs discussion.*

This is, of course, a critical point. Surface mass balance and temperature reconstructions are available for the period 1840-2012 (Box et al., 2013; Box, 2013). This is too short to drive the model with realistic surface mass balance and temperature data. The SMB values of Funk et al. (1994) are very close (to within ±20%) to the values along the modeled flow line as reconstructed by Box et al. for the time span 1900-2000. The problem is worse for the firn temperatures. Since the relation between firn temperature and average surface temperature is very nonlinear due to melt water infiltration close to the melting temperature, a good parametrization would be needed. We think that this problem is outside of the scope of this paper.

Based on the above thoughts we decided to stick to the SMB and surface temperature parametrization of Funk et al. (1994). We added a sentence explaining the above *which are similar to the 1900-2000 values of a more recent reconstruction (Box, 2013).*

Repetitive bit of text. The description of the surface boundary appears twice: Dirichlet boundary conditions (prescribed temperature). . . is said on Page 5174, L23 and again on Page 5175, L29.

We now deleted the first occurrence.

P5178, L 23-28. Argument unclear. Our near surface temperature profiles (Fig. 2a) support this notion, with temperatures above 1 C at GULL, but colder near-surface temperatures at downstream drill site FOXX. It is likely this difference in surface temperature, and therefore the distribution of dust (dirty ice vs. cryoconite holes) that leads to the dark band visible in the upper ablation zone of the western GrIS (Wientjes et al., 2011).

During the summer season in the ablation zone, the very surface of the ice is at the melting point (one place is no colder than another) and its the size of the winter cold wave below the surface that differs. So I dont follow the argument being made here. Also, Wientjes et al. (2011) investigates the mineralogy of the dust (c.f., ice temperature) and so it must be clear that this reference is simply for the fact that there is a dark zone and is not being used to substantiate the first idea in the sentence.

The ice just under the surface matters for the fate of the surface melt water and the dust. Under cold near-surface conditions, encountered at FOXX, the ice is impermeable to water, and cyroconite holes collect all the dust. Under temperate near-surface conditions, encountered at GULL, the ice is rotten by radiation, water can permeate into the ice, and dust is spread out at the surface, leading to a brownish appearance of the surface ice. This difference in dust distribution leads, in our opinion, to the distinct appearance of the surface, and to the dark band visible in satellite imagery.

We now have cited (Wientjes and Oerlemans, 2010; Wientjes et al., 2012).

Figure 1. Can you show F450/F500 on this map for reference? These are important locations in the paper.

These points are too far outside of the map to be shown (50 resp. 100 km inland of Swiss Camp). So we don't see how these points could be added to the maps in any meaningful way. They are, however, indicated in Figure 3 which should give a good indication of their position.

Figure 2. Why not connect the dots? Strange that this plot is not connected (and therefore difficult to interpret), but all others are connected.

The figure is actually harder to read with lines, since especially the red dots have strongly varying values (nothing changed).

Figure 3. The surface cartoon depicting firn is confusing. I think, the dark blue band is a package with different age than the adjacent firn? Why isnt the firn below this (lower elevation) also warm. . . how could it be that the mid-elevations are melted but the lower elevations are not (is this the different age issue?). What exactly is the flow path that has moved this firn down flow to the borehole locations?

This plot indeed contains time information, and indicates the area where the firn that arrives at the boreholes TD5 was accumulated in the time span 1570-1730. We do not see how we could present this information in a different manner. We have adapted the caption accordingly and now write
The light-blue area indicates cold firn conditions, and the dark blue area between 460 and 475 km *the area within which the temperate firn that emerges at TD5 was accumulated between 1570-1730.*

Figure 5. Dots connected dotted lines indicate measurements. Unclear.

Typo. It now reads *Symbols connected by dotted lines ...*

Figures 5,6,7. All need A/B labels. Also, the depth scale between the upper and lower panels is totally different, making comparisons between the two panels difficult.

We have added A/B labels everywhere now.

The different depth scales are adapted to the data being shown. If same depth scales were used, the lower two thirds of the lower panels would be empty, since our measurements only go down to 700 m (nothing changed).

Figure 7. Gull has a very substantial amount of negative extra heat at middle depths (200-400 m), i.e., the ice is much colder than the reference run. Unless I missed

it, this is not explicitly discussed in the paper. This would seem to be an important result much in need of attention and discussion.

The comparison with the reference model run is shown in Figure 5, and there also GULL is much warmer than the model predicts. Only in the run mCEF (with crevasses), shown in Figure 7, the GULL temperatures are higher. This means that one would have to choose locations of crevasses more carefully, or add several such zones with different depth, an exercise we did not perform here.

References

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