The Cryosphere Discuss., 8, C2337–C2340, 2014
www.the-cryosphere-discuss.net/8/C2337/2014/

Interactive comment on "Excess heat in the Greenland Ice Sheet: dissipation, temperate paleo-firn and cryo-hydrologic warming" *by* M. P. Lüthi et al.

Anonymous Referee #3

Received and published: 13 November 2014

Two full-thickness borehole temperature profiles from Greenland are compared to modeled temperature profiles as a means of investigating thermal processes in the ice sheet. The temperature profiles were collected at two sites located down-flow of Swiss Camp, west-central Greenland. Initially, the model output yielded considerably colder ice than the borehole temperatures. The authors then add various heat sources to their model runs to minimize the difference between modeled and observed. Heat sources include (a) enhanced strain-heating in Wisconsin age basal ice, (b) latent heating in a temperate paleo-firn, layer and (c) latent heating in deep, water-filled crevasses. Results indicate that substantial heating from these processes, particularly latent heating in firn and crevasses, is required to bring the model output closer to observations. This

implies these processes are important, and should be better included in numerical simulations.

Rather than model with the objective of curve matching, the goal of this work is to investigate how heating mechanisms might contribute to observed ice temperature. This objective is nicely conceptualized and well stated in the paper. The data and results make important advancements to both observational and modeling investigations of Greenland. I believe the paper would make a solid contribution to The Cryosphere. However, I have several recommendations for improvement before publication.

1) Excess heat. The paper's 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.

2) Surface boundary conditions All model simulations use a prescribed surface temperature boundary taken from Funk et al., (1994). If I'm 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.

Other comments:

» 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.

» 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 it's the size of the winter cold wave below the surface that differs. So I don't 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.

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

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

»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 isn't 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?

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

» 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.

» 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.

Interactive comment on The Cryosphere Discuss., 8, 5169, 2014.