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

M. Truffer (Referee)

truffer@gi.alaska.edu

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This is a very interesting paper with a careful analysis of borehole temperatures measured in Greenland. Such measurements are difficult to obtain and therefore rare, yet temperature is an important controlling factor for ice deformation rates. That alone makes the paper worth publishing. Furthermore, the authors come to important conclusions, showing that a previous warming period still exerts a noticeable influence on ice temperatures today. This has important consequences for the effect of current warming.

Before I recommend publishing, I suggest the following changes and additions to the

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Discussion Paper

paper:

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.

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?

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 don't 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?

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

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might therefore want to rethink that conclusion.

Some small details:

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 don't mean anything to the reader at this stage, and you introduce them in the next paragraph)

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

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

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

5178, l.20: What's sluggish ice. Do you mean slushy?

Table 1 seems superfluous, you don't 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.

Figure fonts are generally small

Martin Truffer

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