The Cryosphere Discuss., 8, C2335–C2340, 2014
www.the-cryosphere-discuss.net/8/C2335/2014/

Interactive comment on "Quantifying meltwater refreezing along a transect of sites on the Greenland Icesheet" *by* C. Cox et al.

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Received and published: 12 November 2014

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1 General comments

This interesting paper derives estimates of the amount of meltwater that is refrozen over the summer period at sites in the percolation zone of the Greenland Ice Sheet. The

authors use temperature measurements in the upper 10 m of the firn to estimate the change in sensible heat content in a $\approx$ 9 m sub-surface layer. They attribute increases in heat content to (1) a positive net influx of sensible heat across the layer boundaries and (2) release of latent heat from a net influx of meltwater which refreezes within the layer. Processes in the surface layer, i.e. the upper 1 m or snow, are not considered. This allows the mass/energy budget for the sub-surface layer to be simplified, on the grounds that the effect of solar radiation will not be significant at depths $\geq$ 1 m. Since the mean annual accumulation is said to be of the order of 1 m, the authors are in effect estimating the amount of summer meltwater that travels through the winter snow and into snow accumulated during previous years. This is worth doing, as it tells us how far the mass in an accumulation layer (something we can measure) differs from the surface mass balance (something we want to know).

The model used is 1-dimensional and based on the assumption that, as far as the energy budget is concerned, the snow can be treated as a medium of density $\rho(z)$, where z is depth. The location of latent heat sources within the layer is not specified, however, so this is a "lumped" rather than "distributed" model. Since the temperature sensors move downwards with the snow, a Lagrangian rather than Eulerian approach is implied. All this is perfectly reasonable, but the theory needs to be explained rather more rigorously so that the reader can have confidence in the results.

The crux point is the argument that horizontal variability can be neglected. I think it is the **magnitude** of the thermal diffusivity, α, that is important in judging whether the spacing between latent heat sources needs to be taken into account rather than "the diffusive nature of heat conduction" (p.5488 l.7). The relation between length and time scales, z_0 and t_0, for thermal conduction in homogeneous snow with no internal sources is

$$z_0 = (\alpha t_0)^{1/2} \tag{1}$$

For $\alpha \approx 4.10^{-6}$ m^2 s^{-1}, $z_0 \approx$ 1 m for $t_0 \approx$ 3 days (c.f. p.5490 l.28). In other words, if the horizontal spacing of pipes is of the order of 1 m, the 1-D model is appropriate for temperature fluctuations with frequency lower than $\approx 4.10^{-6}$ Hz. The authors need to show that fluctuations in surface snow temperature at frequencies higher than this (for example diurnal fluctuations) are damped out by the time they reach the upper boundary of the sub-surface layer. I think it might be possible to demonstrate this using the data that they have, by showing a spectrogram (see, for example Sergionko et al., 2008, Annals of Glaciology 49 p.91) for temperature at the 1 m level, unless the high-frequency electronic noise at $\approx 5.10^{-4}$ Hz complicates the picture too much.

The change in sensible heat over time period Δt is calculated from the differences between temperatures T_j measured by a vertical string of sensors at the start and end of the period. The question is, whether high frequency variations in latent heat input could mean that the T_j do not give an adequate representation of the temperature profile. The appropriate length scale is the spacing of the sensors ($z_0 \approx$ 20 cm) and hence $t_0 \approx$ 3 hours. The temperature profiles shown in the companion paper (Humphrey et al. 2012 JGR doi:10.1029/2011JF002083) show refreezing events on this time scale producing narrow peaks which are only just resolved by the sensors. So this could well be a problem. The answer might be to smooth each T_j over a period of about a day, at the start and finish of the time period, before calculating the change in sensible heat.

Finally there is the problem that the thermistor strings were installed in 9 cm boreholes back-filled with fine-grained cold snow. Humphrey et al. consider that the thermistor wires acted as preferential pathways for heat conduction but the boreholes were not preferential pathways for water. Mentioning this, with a little discussion, would help the reader.

The authors' estimates of refreezing are significantly lower than the levels predicted by the MAR model. They seem to be rather hesitant to suggest that the MAR model may be wrong, but it is surely important to probe into this discrepancy. Do they think the

problem lies in their analysis or in MAR? If in MAR, is the meteorological component not predicting surface conditions correctly or is the snow model inadequate? It should be possible to tease this out given their data. For example, one could ask whether the MAR surface temperature series bears any relationship to the observed surface temperature series. And so on.

2 Specific comments

- p.5486 l.1 The abstract reads rather more like an introduction than a summary of results and would benefit from a rewrite.
- p.5486 l.22 Perhaps better to say models “suggest” something rather than “show” something?
- p.5486 l.24 To be precise, remote sensing shows an increase in the area and time period over which melt occurs, not necessarily the amount of melt.
- p.5488 l.3 Latent heat diffuses in the snow not in the temperature profile.
- p.5490 l.3 Better to separate the equation and definition of variables.
- p.5490 l.27 Useful to state the values of the parameters
- p.5491 l.6. The Lagrangian approach could be made more explicit by using a water equivalent depth variable, say q, to denote position within the layer, rather

than z. Equation (1) is not correct if z is depth below the surface.

- p.5492 l.9 The authors assume that ρ is constant in time, but clearly this is not the case if meltwater penetrates the layer. Rather than make the vague comment that this effect is negligible except possibly at H3 and H4, why not state the maximum melt expected (say 0.5 m w.e) and say what proportion this is of the w.e. of the layer? Furthermore, the layer is deeper at the end of the period than at the start so, even without influx of meltwater, the layer will densify. This again needs to be quantified.

- p.5492 l.18. The authors do not define their terminology but I assume dT/dz is meant to be a material derivative. This needs to be made explicit. Again the effect of temporal variation in ρ needs to be quantified.

- p.5491 l.20. Integration over time appears to involve smoothing a fairly noisy series of values of dT/dz. The authors need to explain exactly what they have done rather than rely on Figure 2a.

- p.5493 l. 5. There were 11 sites, 10 of which had winter data. Of these 6 had refreezing less than 1 cm w.e. and 4 greater than 1 cm w.e. So 40% need further explanation? This paragraph could do with a rethink. I would discuss the sensitivity of all estimates (winter and summer) to uncertainty in snow properties.

- p.5493 l.23 The authors really need to explain the numerics behind the calculation of heat flux. Do they remove the noise before calculating the gradient?

- p.5494 l.5. What about systematic errors?

- p.5494 l.19 “corresponding to” is not the right verb here.

- p.5494 l.25 analyses

- p.5496 l.20. Why are sites T2 and T1 colder than their neighbours?

- p.5499 l.23. It seems rather odd to say piping significantly complicates things after arguing a 1-D model is adequate.

- p.5500 l.25 “with our”?

- p.5508 Fig 2a Why the sudden drop in early July?

- p.5508 Fig 3. Different line styles could be used for 2007 and 2008.