

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

C. Cox et al.

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1 Comment:

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 melt water 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).

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1.1 Author's Response:

It is true that our results could be used to get a general sense of the depth partitioning of melt water refreezing, but our results contribute much more than just that. Our refreezing values are some of the first values obtained for Greenland using in-situ data, and, in addition to providing insights into surface mass balance, they aid in our understanding of melt water infiltration processes.

2 Comment:

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.

2.1 Author's Response:

We disagree that our approach is Lagrangian because we do not know the compaction that is occurring in the snowpack. Since the bulk of the snowpack remains below 0, we expect the compaction to be small and the difference between the Lagrangian approach and the Eulerian approach is probably small. However, this is an assumption that we make since we do not have compaction data and we have edited to state our assumption explicitly.

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2.2 Changes to manuscript:

Original (5491, L9): This formulation assumes that density (ρ) and heat capacity (C_p) do not change over time. This is reasonable because, at most of the sites, the input of melt water is minor compared to the water equivalent of the firn column. This assumption may break down for the lowest sites (H3, H4).

Revised: This formulation assumes that density (ρ) and heat capacity (C_p) do not change over time. This is reasonable because, at most of the sites, the input of melt water is minor compared to the water equivalent of the firn column. This assumption may break down for the lowest sites (H3, H4). Densification due to compaction of the firn is also assumed to be minimal within our seasonal timescale.

3 Comment:

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

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

For $\alpha \approx 4 \cdot 10^{-6} \text{ m}^2 \cdot \text{s}^{-1}$, $z_0 \approx 1 \text{ m}$ for $t_0 \approx 3 \text{ days}$ (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 $\approx 4 \cdot 10^{-6} \text{ Hz}$. 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

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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 $\approx 5 \cdot 10^{-4}$ Hz complicates the picture too much.

3.1 Author's Response:

We find this comment to be a bit confusing. If we are understanding correctly, the argument is that the time scale of thermal diffusion is important in assessing the legitimacy of the one dimensional approach. We agree with this and provide an analysis of the timescale of diffusion for a melt water pipe (5490 L28). However, Morris is focused on the frequency of the temperature change and suggests we apply her analysis to surface temperature variations. The characteristic frequency of the piping does not need to be investigated with sophisticated analysis because high frequency (narrow spikes in temperature) variations have low energy content while lower frequency variations produce temperature perturbations that spread out on a timescale that is much shorter than our seasonal analysis (per our earlier analysis). It is unclear why we would apply her analysis to surface temperature variations as we use our temperature data to track heat flux at 1m.

4 Comment:

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.

4.1 Author's Response:

One of the main benefits of our method is that we do not actually need to capture high frequency temperature fluctuations. Any latent heat released inside our domain, even a very narrow spike that isn't visible in the temperature data initially, will spread out to the other sensors and contribute towards the overall increase in temperatures in the profile. When this heat finally reaches the domain boundaries we are able to account for it since we are tracking heat flux at the boundaries. There is some potential that the ending temperature profile would not have the resolution to detect a narrow spike in temperature that just happened to occur less than a couple of hours before the temperature measurement and was located in between two sensors. However, we chose the end dates for our analysis based, in part, on review of the entire dataset. Any major refreezing event would have been obvious in the temperature profiles measured several hours after the profile chosen for the analysis end date. It is therefore unlikely that any significant refreezing was unaccounted for due to the sensor resolution.

5 Comment:

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

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preferential pathways for water. Mentioning this, with a little discussion, would help the reader.

5.1 Author's Response:

Our sensor measurements show that after emplacement of the temperature string, the temperatures in the disturbed firn rapidly stabilize to temperatures equal to that of the surrounding snow. Since snow crystal metamorphosis is driven primarily by temperature, the cold snow that was used to backfill the borehole should begin to quickly evolve to a density and structure similar to the surrounding undisturbed firn. Any disturbance to ice layers within the firn caused by drilling would not create a heterogeneity that is significantly different than the inherent heterogeneity in the firn. We have added a short section to describe the method in more detail.

5.2 Changes to manuscript:

Original (5489 L2): Sensor spacing is 0.25 m from 0 to 5.5 m depths and 0.5 m from 5.5 to 10 m depths. The sensors were installed with reference to the surface at time of installation.

Revised: Sensor spacing is 0.25 m from 0 to 5.5 m depths and 0.5 m from 5.5 to 10 m depths. The sensors were installed with reference to the surface at time of installation. After temperature string emplacement, the boreholes were backfilled with fine grained, cold snow, and our temperature measurements show rapid thermal equilibrium with the surrounding undisturbed firnpack (For details see Harper et al. 2011).

Added reference: Harper, J., N. Humphrey, T. Pfeffer, and J. Brown (2011), Firn stratigraphy and temperature to 10 m depth in the percolation zone of western Greenland, 2007-2009, INSTAAR Occasional Paper, (60).

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6 Comment:

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.

6.1 Author's Response

The MAR comparison is meant to give our results some context and we assess whether the discrepancy could be due to not including the first meter of firn in our analysis. Beyond that, we leave detailed analysis of local scale MAR outputs to future studies.

7 Comment: P5486 L1

The abstract reads rather more like an introduction than a summary of results and would benefit from a rewrite.

7.1 Author's Response:

Will review.

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8 Comment: P5486 L22

Perhaps better to say models “suggest” something rather than “show” something?

8.1 Author’s Response:

The word ‘suggest’ implies that the model output is unclear or vague, but the conclusions in the references are fairly clear cut.

9 Comment: P5486 L24

To be precise, remote sensing shows an increase in the area and time period over which melt occurs, not necessarily the amount of melt.

9.1 Author’s Response:

Agreed. Reworded.

9.2 Changes to manuscript

Original: Although the increase in melt is clear from the remotely sensed data, the increase in melt water leaving the ice sheet is not as well constrained.

Revised: Increases in the areal and temporal extent of surface melt, evident from remote sensing, support model based increases in surface melting. However, the increase in melt water leaving the ice sheet is not as well constrained.

10 Comment: P5488 L3

Latent heat diffuses in the snow not in the temperature profile.

10.1 Author's Response:

Reworded.

10.2 Changes to manuscript:

Original: Latent heat released during refreezing diffuses into the firn temperature profile as a thermal perturbation that can be quantified using a conservation of energy approach.

Revised: Latent heat released during refreezing diffuses through the firn causing a thermal perturbation in the temperature profile that can be quantified using a conservation of energy approach.

11 Comment: P5490 L3

Better to separate the equation and definition of variables.

11.1 Author's Response:

The goal of writing out the equation is to rephrase the end of the paragraph above in order to introduce the reader to the mathematics of our approach.

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12 Comment: P5490 L27

Useful to state the values of the parameters

12.1 Author's Response:

For 1m pipe spacing, any range of realistic values for the parameters will result in a timescale of a few days. We therefore find it unnecessary to give exact values.

13 Comment: P5491 L6

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.

13.1 Author's Response:

Again, we do not utilize a Lagrangian approach. However, it is true that 'z' should not be depth below the surface. An adjustment has been made to clarify.

13.2 Changes to manuscript:

Original: The change in heat content over the summer melt season can be quantified from the changes in profile temperature (z = depth) using:

Revised: The change in heat content over the summer melt season can be quantified from the changes in profile temperature (z = depth from the top of the profiles) using:

14 Comment: P5492 L9

The authors assume that ρ is constant in time, but clearly this is not the case if melt water 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.

14.1 Author's Response:

Densification can be estimated from melt estimates (assuming all the melt water refreezes). However, the spatial distribution of refreezing must be accounted for somehow, otherwise the density at a particular location could be almost anything between the starting density and ice. Our temperature profile data does give some indication of the location of refreezing events, but the resolution is insufficient to determine exact ice location and thickness. Given that the refreezing is not uniform and the distribution of ice lenses unknown. It is unrealistic to conduct a detailed analysis of density changes in the firn from the data we have. However, some back of the envelope calculations can be performed to get an idea of the magnitudes of density changes. For example, at CP, if the total refreezing quantity is uniformly distributed over the first layer of our domain, the density change is on the order of 20 kg m^{-3} . At H2, the total refreezing is much higher, but the water is also shown to penetrate much deeper. Distributing the water at H2 over the first 5 meters of the domain results in a density change of about 30 kg m^{-3} . Both of these are density changes are similar in magnitude to the density variations using in the Monte Carlo trials. We can therefore conclude that density changes may not play a significant role for the majority of the firn pack included in our analysis. Densification due to compaction is addressed in an earlier comment.

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15 Comment: P5492 L18

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.

15.1 Author's Response

Revised to make methods more explicit.

15.2 Changes to manuscript:

Original: The boundary temperature gradients in Eq. (3) are approximated by taking the gradient of the two sensors closest to the 1 and 10 m bounds.

Revised: The boundary temperature gradients (dT/dz) in Eq. (3) are approximated using the temperature gradient of the two sensors closest to the 1 and 10 m bounds. For example, the temperature gradient at the upper boundary is $\frac{T_{(1.25,t)} - T_{(1,t)}}{0.25}$.

16 Comment: P5491 L20

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.

16.1 Author's Response:

Data were not smoothed at any point. Edited to clarify numerical method used.

16.2 Changes to manuscript

Original: Figure 2a shows a time series of net heat flux at site H2. High frequency variations on the order of 0.5°C are a result of random electronic noise in each temperature measurement. Since this noise is random, the integrated flux derived from the gradients is not biased.

Revised: Equation 3 is approximated by numerically integrating net heat flux using the trapezoid rule. Figure 2a shows a time series of net heat flux at site H2. High frequency variations on the order of 0.5 C are a result of random electronic noise in each temperature measurement. Since this noise is random, the integrated flux derived from the gradients is not biased.

17 Comment: P5493 L5

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.

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17.1 Author's Response:

(A similar comment was made in Anonymous Review 1. The response is the same. The revisions are detailed in that review.) Upon reviewing the winter test results, it appears our original discussion on this topic was oversimplified, but the general conclusions are nonetheless the same. Both H1 and H163 show similar “refreezing values” around -1 cm w.e. corresponding to the December thru April time period. A negative value means that there is less heat as determined by the temperature change than would be expected from the heat lost at the boundaries of the domain. So, the heat lost through the boundaries needs to be increased in order for it to balance with the change in profile temperature. This is accomplished by increasing the boundary density and thereby increasing thermal conductivity. Our uncertainty is on the order of 1.5 cm w.e., so neither H1 or H163 are significantly different from zero. H165, H2, and H3 have winter “refreezing values” on the order of -2cm w. e. before tuning the boundary density to 600 kg m⁻³. These three sites also have unusually low measured densities at the boundary as compared to the rest of the profile, while H1 and H163 do not. So, overall we should have specified that H165, H2, and H3 are the only sites requiring density tuning and they are also the only sites with inconsistently low density values at their boundaries. We have rewritten the paragraph to account for these issues.

18 Comment: P5493 L23

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

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18.1 Author's Response:

The details of the calculation of heat flux are now included in response to a previous comment. Noise is not removed before calculation, but the impact of the noise is assessed in the Monte Carlo error analysis. I made a slight edit to clarify where we differentiate data with noise in it.

18.2 Changes to manuscript:

Original: Since our method differentiates discrete data when the heat flux is calculated, we assume that the largest errors stem from amplification of data noise by differentiation.

Revised: Since our method differentiates discrete data when the heat flux is calculated (numerical approximation of Equation 2), we assume that the largest errors stem from amplification of data noise by differentiation.

19 Comment: P5494 L5

What about systematic errors?

19.1 Author's Response:

Systematic errors from sources such as sensor calibration or drift can not be quantified by the Monte Carlo analysis above, and indeed, remain as an uncorrectable potential error in our calculated refreezing values

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20 Comment: P5494 L19

“corresponding to” is not the right verb here.

20.1 Author’s Response:

The temporal domain over which we apply our method varies between sites, but the concept has been surprisingly difficult to describe in a simple manner. I made some minor edits to try and clarify the sentence.

20.2 Changes to manuscript:

Original: Unfortunately, data quality problems prevented all refreezing quantities from corresponding to exactly the same time period (see Table 1).

Revised: Unfortunately, data quality problems at some of the sites reduced the time period over which our method could be applied (see Table 1 column 4).

21 Comment: P5494 L25

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21.1 Author’s Response:

Will fix.

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22 Comment: P5496 L20

Why are sites T2 and T1 colder than their neighbors?

22.1 Author's Response

Our analysis of the near surface temperatures is mostly qualitative due to the uncertainties in temperature readings resulting from surface exposure and solar radiation. Figure 4 shows that in 2008, most sites had near surface temperatures close to zero and therefore lack significant cold content that would initiate refreezing. However, near surface temperatures at sites T1 and T2 in 2008 show colder temperature throughout the melt season. This may simply be due to the higher elevations of these sites.

23 Comment: P5499 L23

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

23.1 Author's Response:

We argue that piping does introduce significant complexity not accounted for in either snow models or parameterizations currently used. Both models and parameterizations assume melt water moves through the firn in a uniform manner when it is actually heterogeneous. However, despite this heterogeneity, a one dimensional approach is nonetheless appropriate because the major structures associated with piping, ice lenses, are in the vertical dimension. Although the process is termed “piping”, the vertical “pipes” themselves have less of an impact on the dominant temperature structure

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of the firn pack compared to both the ice lenses and vertical heat conduction from the surface. I have made some changes to the conclusion to better summarize our position.

23.2 Changes to manuscript:

Original: The calculated refreezing quantities reveal a transition from complete refreezing of melt water at higher elevations to eventual runoff of melt water near an elevation of around 1500m, up to 40km inland from the ELA. Even where complete refreezing does occur, a significant portion of the overall refreezing takes place at depths greater than 1 m. This may be a result of piping of melt water to much greater depths than would otherwise occur by uniform infiltration. Since heterogeneous infiltration is not currently accounted for in snow hydrological models, these in situ refreezing values provide an important source of snow/firn model validation. Our results show that piping of melt water significantly complicates the relationship between total refreezing and simplified theoretical approaches to predicting refreezing capacity. Thermal profiling for the lower accumulation zone can be used to both quantify melt refreezing, as well as help to locate important zones such as the runoff limit.

Revised: The calculated refreezing quantities reveal a transition from complete refreezing of melt water at higher elevations to eventual runoff of melt water near an elevation of around 1500m, up to 40km inland from the ELA. Even where complete refreezing does occur, a significant portion of the overall refreezing takes place at depths greater than 1 m. This may be a result of piping of melt water to much greater depths than would otherwise occur by uniform infiltration. Since heterogeneous infiltration is not currently accounted for in either snow hydrological models or simple theoretical parameterization, these in situ refreezing values provide an important source of snow/firn model validation. Finally, our results also give some indication that lateral movement of infiltrated meltwater, in some cases from prior melt seasons, may be significant in

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this region of Greenland, complicating the classic understanding of percolation zone processes.

24 Comment: P5500 L25

“with our”?

24.1 Author’s Response:

Rephrased to make sentence less confusing.

24.2 Changes to manuscript:

Original: We have used these density based K values, that are internally consistent without temperature data, in our modeling of the summer melt/refreezing calculations.

Revised: We utilize equation A1 in our refreezing analysis to calculate thermal conductivity values from averaged field density measurements.

25 Comment: Fig 2a

Why the sudden drop in early July?

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25.1 Author's Response:

Q (grey region, panel A) is the total heat gained by the profile due to heat conducted through the domain boundaries at 1m and 10m depths. It is calculated by the integrating the time series of net heat flux (q_{net}). The drop in q_{net} mid June is associated with a refreezing event near the 1m boundary that sharply elevated the temperature near the boundary creating a strongly negative net heat flux for a short period of time. In other words, the temperature at 1.25m depth became much warmer than the temperature at 1m and heat was conducted upward out of the method domain. Added a sentence to the caption to clarify.

25.2 Changes to manuscript:

Original: (a) Net heat flux through the top and bottom of the domain (see panel b) from 1 June 2008 to 1 August 2008 at site H2. Q is the integral of the time series (see Eq. 2).

Revised: (a) Net heat flux through the top and bottom of the method domain (see panel b) from 1 June 2008 to 1 August 2008 at site H2. Q is the integral of the time series (see Eq. 2). The sharp drop in q_{net} mid June is the result of a refreezing event within the domain near the 1m boundary. Refreezing increased the temperature gradient at the boundary and heat was conducted out of the domain (negative q_{net}).

26 Comment: Fig 3

Different line styles could be used for 2007 and 2008.

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26.1 Author's response:

The figure will be edited to make the different lines more distinct using separate colors.

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