The Cryosphere Discuss., 3, C84–C89, 2009
www.the-cryosphere-discuss.net/3/C84/2009/

The Cryosphere
Discussions

Interactive comment on "The role of radiation penetration in the energy budget of the snowpack at Summit, Greenland" *by* P. Kuipers Munneke et al.

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Received and published: 19 June 2009

First of all, we would like to thank Mauri Pelto and the two anonymous reviewers for their valuable and constructive comments on the manuscript. In this interactive comment, we will reply to the points that were raised.

Referee 1:

1. Opposed to the suggestion by reviewer 1 to express diurnal averages of the energy balance component in MJ m^{-2}, we have chosen to retain the numbers in W m^{-2} to facilitate comparison with other literature. We made more explicit that the numbers represent diurnal averages of a flux, not a cumulative amount of energy per unit surface

area.

2. We believe it is possible that meltwater percolation will occur at Summit sometime in the forthcoming decade or two, and our institute is currently involved in a major effort to run a polar regional climate model, equipped with a snow model, over Greenland until 2200 AD in double-CO2 experiments. It would be interesting to see the implications for the snowpack at Summit in the coming century.

3. Section 2 is now subdivided into 4 small subsections, providing more structure in the presentation of the data. It is now explicitly stated that wind speeds were actually obtained using a Young wind monitor and not from the sonic anemometer.

4. The thermistor strings are constructed inside a 30 cm long plastic cover which is pushed gently into the wall of a snowpit. The only possible temperature distortions that we can think of are due to a different snow structure in the snowpit after it has been filled up again, or due to very small disturbances in the snow density around the end of the thermistor cover. We think that both effects are negligible.

5. From experience, we knew beforehand that the double-domed CM21 pyranometers are the instruments most susceptible to riming. Riming of the pyrgeometers did not occur during this period due to good ventilation and due to the black surface of the instruments. Rime accretion has indeed been observed on the sonic anemometer, but part of this could be removed in the morning by gently pulling the guy wires of the automatic weather station. We detected a very small (< 0.1K) difference between sonic temperature and air temperature at some mornings when sublimation of the rime extracted heat from the constantan thermocouple wire. We did not correct for this.

6. The distance between our measurement location and the BSRN location is less than 150 metres, and at the same side of the camp, in the so-called clean-snow and clean-air zone. We assume roughness, emissivity etc. to be uniform over vast expanses on the Greenland plateau, although strictly we have no knowledge about spatial variability of these quantities. The homogeneity of the area is however demonstrated by the fact

that, simultaneously, density profiles were obtained by a firn coring team some 8 km from camp, which matched very well with the density profiles we obtained at our site.

7. and 8. It proves difficult to accurately quantify the effects of the assumption of a constant density and snow grain size on the model results. However, in figure 7 we demonstrate that vertical variations in these quantities can lead to a different energy distribution in the snowpack. Temporally however, we let the snow density profiles relax towards measured density profiles that were obtained 7 times during the measurement campaign. As seen in the sensitivity experiment, an overall increase of snow density by 50 kg m^{-3} leads to a slightly increased difference between model and measured results. We have also done an experiment in which we only increased the density for the subsurface flux by 50 kg m^{-3} and let the radiation penetration density unaltered (not shown in the manuscript). The results of this experiment fall roughly in between the 'optimal run' and the enhanced snow density run as given in table 1, both for the surface temperature difference between model and measurements, and for the temperature profile as in figure 5. In summary, there is a small effect of this decoupling but it is minor, and certainly not related to the 0.45°C temperature difference at the surface.

9. The frequency of the density profile collection is now more explicitly mentioned in the manuscript. The interpolation error is estimated as follows: the density difference over 0.02 m is 20 kg m^{-3} at most, and so for a linear interpolation, the error on the interpolated point is presumably 10 kg m^{-3}.

10. We have renamed section 4 into “Results" and slightly rearranged the text. We opted against the suggestion of the reviewer, as we wanted to have a clear distinction between the description of the model in section 3 and model results in section 4.

11. We wanted to avoid that readers would get the impression that we only performed 8 sensitivity tests on the model results. Therefore, we chose to mention that we performed many tests. We only discuss these 8 tests as those are the essential ones, touching the most crucial assumptions for unknown model parameters, and the most

sensitive quantities in the input data. We also played with several expressions for snow conductivity etc. (as mentioned in the manuscript) but no results of these sensitivity tests are explicitly shown in the manuscript.

12. The snow sampling procedure has now been moved to section 2 as “Data". The description of the radiative transfer model is so short to our opinion, that we retained it in section 4.4.

Referee 2:

1. We have now added the measurement height of the Campbell CSAT3 sonic anemometer. The anemometer was mounted at 3.60 m above the surface. This height is in fact a compromise. Mounting it lower means that larger eddies could be overlooked. On the other hand, the shallow boundary and surface layers over Summit make that the constant-flux assumption of the Monin-Obukhov theory may be violated at this height already. This could partly explain the underestimation of sensible heat flux by the model as shown in figure 2. Unfortunately, we do not have additional information to explore this any further. Furthermore, we have added a few sentences on the performance of the anemometer during rime episodes. We did not encounter any data loss during rime episodes, as most of the rime flakes attached to the sonic beams were of very loose structure that could be removed by a gentle shake of the guy wires of the automatic weather station. The thermocouple measurements of the sonic anemometer were sometimes affected in the early morning, as sublimation of rime extracted heat from the constantan wire. This effect is very small however (< 0.1K) and we did not correct for it.

2. The snow sampling method has been moved to a separate section in the “Data" section (2). Regarding the status of the RT calculations using the DAK model, we have stressed once more in the text that the RT model is in principle physically more correct, as the solution of the RT equation in an inhomogeneous layer of particulate media (snow) is solved more accurately than using the two-stream equation from the Brandt

and Warren model, including multiple scattering within and between snow layers.

As mentioned in the manuscript, snow temperatures could be reproduced by assuming a snow grain radius of 0.1 mm while observed snow grain radii are between 0.1 and 0.5 mm. The use of the RT model shows that this is not necessarily a discrepancy, as vertical profiles of absorbed shortwave radiation are ‘in reality’ (i.e. calculated by the RT model) more irregular than the idealized profiles from the Brandt and Warren model.

All technical comments have been incorporated in the revised manuscript.

Interactive comment by Mauri Pelto:

Before discussing some interesting points by MP, we feel it is important to stress that the paper by Colbeck (1989) discussed by MP uses a wavelength-independent absorption coefficient for the extinction of shortwave radiation in snow. This practice, also seen in Schlatter (1972), was warned against by both Brandt and Warren in their 1993 paper. Brandt and Warren showed that, by taking into account the wavelength dependence of absorption into the snowpack, the depth at which radiation is absorbed is significantly reduced, therefore decreasing the solid-state greenhouse effect suggested by Schlatter. As a result, far more than 88% of the broadband radiation will be absorbed within the first 50 cm. As noted in section 4.3 of our manuscript, 63% of net shortwave radiation is already absorbed in the top 0.5 cm. The temperature gradients discussed by Colbeck and by us, are thus the temperature gradients in the part of the snow in which subsurface radiation penetration is mainly concentrated. In Colbeck's paper, this is the uppermost 0.5 m (for example, his figure 4), while in our study, it is mainly the uppermost few centimetres. The temperature gradients in those few centimetres are indeed much larger when radiation penetration is considered. Opposed to the questions raised by MP, one should not consider the temperature gradient between the surface and 0.50 or 0.75 m, as these will not change very much, and indeed decrease slightly as subsurface temperatures increase more rapidly due to downward diffusion of ab-

sorbed shortwave radiation. Profiles of absorbed radiation dQ/dz are shown in figure 7 and serve the same purpose as figure 8 from Van den Broeke (2008), namely to show the vertical distribution of the absorbed shortwave radiation as a source of energy below the surface. A figure similar to figure 8 from Van den Broeke would give limited additional information, as the vertical extent and magnitude of Q is much smaller at Summit than in the Greenland ablation zone that Van den Broeke (2008) deals with.

The suggestion to look into lag time of the peaks and troughs in figure 3a–b is an interesting one. We found that the model lags the observations by about 2 hours and 20 minutes on average at 0.10 m depth, when penetration of radiation is not included (figure 3b). When penetration of radiation is included, this lag reduces significantly, to 1 hour and 12 minutes. The inclusion of radiation penetration thus helps to bring the model and observations closer together.

Interactive comment on The Cryosphere Discuss., 3, 277, 2009.