The Cryosphere Discuss., 2, S325–S328, 2008
www.the-cryosphere-discuss.net/2/S325/2008/


The Cryosphere Discussions



# *Interactive comment on* "Partitioning of melt energy and meltwater fluxes in the ablation zone of the west Greenland ice sheet" *by* M. Van den Broeke et al.

**D. van As (Referee)**

dva@geus.dk

Received and published: 3 October 2008

Comments on PARTITIONING OF MELT ENERGY AND MELTWATER FLUXES IN THE ABLATION ZONE OF THE WEST GREENLAND ICE SHEET by M. van den Broeke, C. Smeets, J. Ettema, C. van der Veen, R. van de Wal, and J. Oerlemans

General comments

This manuscript provides a valuable and detailed discussion of the impact of energy fluxes on surface and sub-surface melt along a transect in the ablation zone of the Greenland Ice Sheet. Most importantly, in my opinion, the authors present very insightful results concerning the relative importance of sub-surface melt in ice and refreezing

in snow. Below I'll sum up a few of my concerns, which should not stand in the way of publication.

Specific comments

In section 2.1 there is no mention of measured ice temperatures, while in section 3.3 it is said that initial sub-surface temperatures for the surface energy balance calculations come from measurements. Even though the initial temperature profile does not affect the melt calculations a great deal, a clarification would be appreciated.

Section 3.4: The authors mention two ways to calculate the energy balance, one using measured surface temperatures ("more realistic"), and the other searching for equilibrium surface temperatures ("more objective"). Why do the authors choose one method for one publication, and the other for another? And you mention that surface temperatures of the two methods are within 1.5 K of each other. What is the root-mean-square difference? Is there a systematical component as well? I'm asking since one degree off has a large impact on the near-surface gradients, and therewith turbulent heat flux calculations.

Section 4.1: I don't agree that there is low accumulation at S5. The picture shows that the station is placed on an ice hill and any accumulation is bound to be eroded away. In between the hills there must be significant accumulation. What does the site look like in spring, and how much area-averaged accumulation would you estimate at S5 from your experience at the site?

Fig. 3: The accumulation at S6 in 2005 looks strange. Is this realistic?

Section 4.2: You only used ice ablation to validate the melt model, since you have information on snow density, making it difficult to translate the height change measured by the sonic ranger into meters of water equivalent. However, if I'm not mistaken you do use these SR data to add snow in your melt model, using a fixed density of 500 kg m-3. I realize that uncertainties will be larger in comparing measured and observed



**TCD**

2, S325–S328, 2008

snow melt due to this snow density issue, but why not try? Perhaps you can even fine-tune the results to find an optimal snow density. This way you might be able to produce a Fig. 7b for snow as well.

The end of the 2007 melt season is not included in the figures since the data was collected earlier. How far off are we approximately if we conclude that the melt season ends where the dataset ends?

Section 4.3: I'm a bit puzzled by the meaning of Fig 7a, showing the average flux size per time unit with surface melt. Fig. 7b is much more insightful, telling which fluxes caused how much of melt. Could you motivate why you chose to present results in the manner as you did in Fig 7a?

Section 4.3: Please state how much mass is lost/gained through sublimation/deposition/evaporation/condensation, or that it is insignificant.

Section 4.4: You mention that refreezing of melt water reduces the total run-off by 8% at S6. But refreezing in the model only takes place in snow, which is melted off every year. In my mind, your statement then implies that the ice horizon is reached later due to sub-surface refreezing, thus reducing total run-off. However, think that I read in an earlier section that refreezing only heats lower snow layers, it doesn't add mass. So if the snow is heated by refreezing, this means the ice horizon should be reached earlier in the season. Thus sub-surface refreezing should increase total run-off in the model. Where's the flaw?

Please state was happens to total melt amount if one doesn't use radiation penetration or melt water refreezing.

Technical correction

Figures 3, 4, 5, 7: Whereas in one figure S5 results are given in blue and S6 results in red, it is the other way around in another. Consider changing to a uniform colour coding.



**TCD**

2, S325–S328, 2008

Interactive comment on The Cryosphere Discuss., 2, 711, 2008.