

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.

Anonymous Referee #1

Received and published: 15 October 2008

Referees comments on Van den Broeke et al

"Partitioning of melt energy and meltwater fluxes in the ablation zone of the west Greenland ice sheet"

This is a very interesting and important paper. It uses an excellent AWS data set to investigate, through energy balance modelling, variations in energy flux and mass flux partitioning in the ablation zone of the west Greenland ice sheet. The glaciological community needs to combine field data and mass balance modelling to help predict the current and likely future runoff regime of the Greenland Ice Sheet (GrIS). This paper produces some fascinating results revealing how the partitioning of energy fluxes

varies between different parts of the ablation zone (lower, middle and upper) with subsequent implications for the contribution of runoff from different components of the local hydrological system. The match between the observed and modelled melt-rates is extremely impressive and the authors should be encouraged to extend this work (where possible!) across the GrIS.

The paper should undoubtedly be published. There are some weaknesses with the model description which should just require clarification plus a number of other specific queries which are outlined below. I would also like to see the authors compare the runoff results from their detailed energy balance model with output from a simple PDD model. I realise that this may be time consuming and certainly don't think it is essential to incorporate into this paper. However, it would be very valuable to the community to see how poorly (or well) a simple PDD model performs as it is likely that such models will continue to be used across Greenland whilst the distribution of AWSs is so sparse.

Queries/issues to address.

P714, 23-26. The claim regarding "problems associated with ill-functioning sensors could be adequately addressed" is a little vague. Are all the issues concerning post-processing and data correction dealt with in detail in Smeets and Van den Broeke? If not, it would help to have a little more detail/explanation here (or at least some indication of the % of data that required adjustment – I suspect it is very small and if this is the case, would give much less concern for what might be seen as a rather brief dismissal of data quality issues).

P715, 2-3. Over exactly what time period is the surface height data missing for S6 in spring 2005? The snow-depth prediction at S6, as reconstructed from the melt-model (Fig. 3), appears to generate a snowpack that is disproportionately deeper in 2005 relative to snow-depth at S9 when compared with the additional three years when data is available. Is there any explanation for this? In addition, it would help if the dates the sonic height ranger was not working were either added to the captions for Figs 3 and

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



5 or the actual Figs were amended to make clear where the data is real as opposed to interpolated/modelled.

P715, 17. Gs will presumably be dependent on the temperature profile of the ice? Is this known from thermistors and if not, how sensitive are your calculations to this omission? This issue recurs on P717, 8-9 when you state that "the snow/ice temperature profile is initialised using measured ice temperature data" – measured where?

P716, 19-20. It is not clear what is meant by "a 20 day running mean from the AWS profiles". Whilst this may be dealt with in the submitted Van den Broeke paper, an extra line (few words) would help if only to clarify that the "AWS profiles" referred to here and used to calculate z_0 are wind speed profiles at x number of anemometers. In addition, it would be good to add z_0 values to Table 2, especially since the mean wind speed at S5 is the lowest yet the SHF is highest because of surface roughness.

P717, 27 - 718, 1-2. Snow density is kept constant at 500 kg m⁻³ – can you be more specific as to what (and when) are the observations made from which the snow density is prescribed? It is not clear why this chosen value "ensures that snowmelt stops and ice melt starts at the correct time". For example, what effect would selecting a snow density of 400 kg m⁻³ have on your results?

Section 3.4, P718. It is not clear why two different methods have been used in two different papers (using the same data) to calculate M? If the method used in this paper is more "objective", are you simply saying that the previous method used is not as 'appropriate' even though your results give you "confidence in both methods" (i.e. in hindsight, do you now recommend this approach?). It would certainly help if a little more detail was provided to explain how T_s is calculated in this paper. The description in lines 12-14 is not really adequate, especially the statement "This equation is then solved for T_s by bisection in a 15K search space around the value of the previous time step".

P720, 11-12. Is there any simple explanation for the small melt overestimate at S5 in

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



2005? Given the excellence of the model performance elsewhere, it would be interesting to know if further interrogation of the raw AWS data might reveal a single sensor error.

P720, 14-21 and Fig 6. Is Fig 6 really the best way to qualitatively assess the melt-model at daily/sub-daily time-scales. A plot of modelled and observed melt-rates (averaged every few (4?) hours) through time would give a better indication of the diurnal melt-cycle and the differences between the modelled and observed ice melt. In its current form, Fig 6 does confirm additionally the excellence of the model as revealed in Fig 5 but perhaps hides some of the more interesting differences/similarities at shorter timescales. For example, the sonic sensor suggests that the surface rises (accumulates) on many nights, a feature not replicated by the model. Is this 'accumulation' real (water vapour condensation and refreezing (on clear nights?)) or simply an issue to do with the sonic sensor. Either way, it's a really interesting observation.

P722, section 4.4. 1) I am slightly confused by the refreezing procedure and what actually happens in your model to the meltwater which refreezes. At S6 for example, "refreezing in snow consumes about 10% of the melt energy" (P712, 13 and P722). I presume this refrozen meltwater is melted again leading to its subsequent removal? At S9, you state that "refreezing consumes about 1/3 of the total melt energy" (P722, 23-24). Again, I presume that the meltwater is generated, refrozen and melted a second time thereby resulting in removal of the whole snowpack with no annual accumulation from refreezing. A couple of lines (or extra words) just to clarify exactly what is happening here would help in section 4.4 and in the caption for Fig. 8.

2) I would like clarification on the internal ice melt procedure in the model. At S5, about 23% of all ice melt occurs below the surface. Down to what sort of depths does the model suggest this melt is occurring? The volume of melt is equivalent to an integrated $\sim 1000 \text{ kg m}^{-2} \text{ a}^{-1}$. If real, this amount of ice melt has the potential to considerably reduce the density of the subsurface ice with obvious implications for your assumed ice density and thus the runoff volumes as determined from your sonic height ranger

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



measurements. I am no doubt missing something obvious here but clarification in the text would help. The same issue is clearly also relevant to S6.

Minor points/typos Introduction. Given the importance of refreezing on the mass balance of the GrIS and the relevance to this paper, it would seem appropriate to reference the seminal paper by Pfeffer in 1991 that highlighted this issue.

P713, 12-25. When were the AWS established? - worth including in the summary. P714, 3. "field area ON 23 August;" P718, 9. "all value OF Ts;" P718, 17. delete extra "the" Fig. 4. Can colours be reversed to keep consistent with Figs 3 and 5.

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

TCD

2, S345–S349, 2008

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

