The seasonal cycle and interannual variability of surface energy balance and melt in the ablation zone of the west Greenland ice sheet by M. van den Broeke and others.

Answers to reviewer's comments

First let me thank the reviewers for their time and effort to assess this paper, it is much appreciated.

Comment by M. Pelto One key point that is touched upon that I believe bears further analysis is the conditions during particularly high melt periods at S5 and the role of SHF during these periods. The need is made apparent by the authors comment that, "...associated melt climate as presently observed at S5 will become representative for large parts of the marginal ice sheet that are currently adjacent to the ocean." How persistent are the high SHF values at S5, such as in the summer 2006? In Figure 8 there are two months where SHF is nearly 100 MJ/month (July 2005 and July 2007). You noted that variations in SHF are key to interannual variations in ablation at S5, can you provide a better measure of this relationship for S5 for May-September for the various years. Fig. 4: There are six 10-day periods when ice melt at S5 exceeds 50 kg m-2 day-1. Though they are not common these periods can account for a considerable percentage of the annual melt, around 15% for a single such period. What is the key energy source during the S5 high melt periods? It would be useful to have more information on these intervals, though they are few and statistically an insufficient data set to draw robust conclusions at this point. It is expected that the conditions at S5 would become representative of larger areas of the ice sheet, and these are the key melt events. The exceptional melt amounts suggest specific events of a duration that would be useful if it could be defined.

Reply To illustrate in more detail the conditions that lead to high SHF values at S5, we have included a new section *3.4 The role of large scale and regional circulations* and new Figure 12, showing high-temporal (hourly) resolution time series of wind, temperature and SEB components (see end of this document). Based on this figure, we discuss the circulation conditions that lead to the large SHF values in the marginal ice sheet. In this new section, we also quantify the correlation between SHF and melt at S5 (r = 0.59, explained variance 35%).

Comment by Reviewer 1 Section 2.1, paragraph 3. It isn’t entirely clear how you determine the snow depth and ice surface horizon. Measured albedo is a poor at differentiating old snow from ice for the reasons you explain, so presumably you use the previous end of summer ice surface as a base horizon to determine the snow depth at the start of the melt season (May). Please clarify.

Reply Has been clarified. The sentence now reads: *The first step is to determine whether ice or snow is present at the surface; for this, a combination of albedo (*$SW_{in}/|SW_{out}|$*) and surface height measurements is used, where we use the previous end of summer ice surface as a base horizon to determine the snow depth at the start of the melt season (May).*

Comment by Reviewer 1 Page 783, lines 15-25. If this is so, then the measured albedo input to the SEB model will be high (mixed snow/ice) relative to the albedo at the sonic height ranger (ice) for most of the ablation period. In which case, one would expect the lowering measured by the sonic height ranger to be faster than that calculated by the model, but if anything Fig. 4a shows the opposite. Please provide an explanation.

Reply If our explanation is correct, then indeed we use an albedo for an area that is larger than seen by the sonic height ranger, which would result in underestimated local modeled ice melt. It is however just one of the measurement uncertainties and it is difficult to isolate this uncertainty from the other uncertainties, and to assess its quantitative impact on modeled ice melt rate. Moreover, it is just a suggested explanation for the gradual decrease in albedo; to make this more clear, we have added *probably* before *caused*.

Comment by Reviewer 1 Section 2.2 SEB Model. It is understandable for the sake of conciseness that the previously published SEB model equations are not repeated here. However, locating the published sources is not straightforward since model components have been published in more than one earlier paper, some of which are not referenced in this section. Hence, please provide a list of papers for the full set of model equations and parameterisations in the first paragraph of this section.

Reply We have now included the sentence: *For a more detailed model description, the reader is referred to Van den Broeke (2004, 2008a, 2008b, 2009b).*

Comment by Reviewer 1 Spatial representativeness of results. On the one hand it is stated on page 792, lines 17-18 *'the AWS observations used in this study are only locally representative'*, and the question of their spatial representativeness is left open in the Summary. On the other hand, the discussion of Table 2 on Page 787 (point flux values at AWSs) and Figure 11 (point values joined by straight lines across space) imply continuous variation or trends and that point values can be extrapolated to large areas. Probably it is safe to make some cautious extrapolation of the point values, but better consideration of what else might affect flux values in unmeasured areas should be made, for example albedo, z0, air temperature and wind speed, possibly cloud. Related to this point, it would be useful to see a plot of temporal variability of z0 for the lowest site (similar to Fig. 2).

Reply This is a valid point, the question of spatial representativeness is always a hard one. But our statement in the summary refers to the fact that our AWS are located in a specific part of Greenland, namely the dry and sunny southwest, which can hardly be called representative for e.g. the wet southeast and northwest. In the paper we have only made extrapolations in cases where no unexpected trend reversals are seen between S5 and S6 on one hand, and S6 and S9 on the other. As this is the case for all (SEB) variables, we assumed it was justified to join the points by lines, for visual guidance. By including error bars in Figure 11, we hope that the reader can judge for himself whether a trend between points is significant or not.

Comment by Reviewer 1 Page 781, line 24, replace 'operates' with 'has operated'.
Reply *Done*

Comment by Reviewer 1 Page 784, line 11, is '[Wm- 2]' supposed to appear after the equation?
Reply *Deleted from Equation, text now reads: 'All terms are in W m*$^{-2}$ *and defined positive when directed towards the surface. Here we briefly repeat the main model characteristics;'*

Comment by Reviewer 1 Page 786, line 12, replace 'hat' with 'that'.
Reply *Done*

Comment by Reviewer 1 Page 787, line 15 and Figure 5. Is melt frequency based on hourly or daily data? Please clarify.

Reply *These are based on hourly data, has been added to text and caption.*

Comment by Reviewer 1 Page 788, line 11 contradicts Page 787, line 22. Does melt commence in May or June at S9?
Reply *Thank you for pointing this out, the line on page 788 '…melting does not occur until June, when albedo starts to decrease and variability increases…' has been deleted.*

Comment by Reviewer 1 References. Give volume and page numbers for Van den Broeke et al., 2009b.
Reply *Done, thank you.*

Comment by Reviewer 2 (X. Fettweis) Pg 785, line 16: What is the threshold used to detect the melt events in the SEB time series? If Melt > 0 is used as threshold, I think this threshold could be too sensitive in respect to the precision of the measurements and errors in the SEB model and perhaps a threshold corresponding to the uncertainty in the estimation of the melt amount could be better.

Reply In the SEB model, melt is assumed to start whenever the model surface temperature T_s (retrieved from solving the SEB equation) exceeds 273.15 K, i.e. it is not based on observations. Observations of surface temperature are rather used as validation (Fig. 3). Of course the model does not always perfectly predict melt at the same time as the observed surface temperature (see for instance new Fig. 12b but also Fig. 3 at melting temperatures). But choosing a threshold for melt different from T_s > 273.15 K would be unphysical and introduce an unwanted bias; rather we have chosen to make a detailed comparison to observed ice melt and 'observed' T_s. As can be seen, the errors are acceptable.

Comment by Reviewer 2 Pg 787, line 2: It is not clear in Table 2, without a read in depth of the text, which values come directly from observation and for the SEB model. Therefore, I suggest to add an indication if the values come from the SEB model or not. It could be a star "*" at the end of the number.

Reply To make this clearer without compromising the readability of Table 2, we have added to the Table caption: *Similarity theory is used to obtain temperature, humidity and wind speed at standard heights from AWS observations. All SEB values are derived from the SEB model apart from SW_{in}, SW_{out} and LW_{in}, which are from (corrected) observations.*

Comment by Reviewer 2 Pg 787, line 25: In addition to the monthly melt frequency, it should also be interesting to discuss and to show in Fig 5 the monthly melt amount in W/m2.

Reply Average monthly melt energy was already presented as the black line in Fig. 7 (multiplied by -1 for better readability).

Comment by Reviewer 2 Pg 791, line 15. The authors say that the SW_{net} explains the melt variability at S6 and S9. But is it due to changes in albedo or changes in cloudiness? I think that SW_{in} and SW_{out} should be separately discussed and shown in Fig 11.

Reply Because melt is driven by SW_{net} in the higher ablation zone, and albedo strongly depends on melt (wet snow metamorphosis much more rapidly lowers albedo than dry snow metamorphosis), SW_{in} and albedo are strongly negatively correlated, i.e. not independent.

There is therefore no simple way to distinguish the effects of anomalies in SW_{in} and albedo, and for clarity we prefer to present the integrated SW effect, i.e. use only SW_{net} as independent variable.

Comment by Reviewer 2 Idem for LW_{in} and LW_{out}. I think that an increase of LW_{in} could be masked by the only use of LW_{net} because an infrared incoming radiation increase induces a warming of the surface inducing a higher LW_{out}. In addition, the MAR model seems to suggest for the whole GrIS (see Fettweis (TC, 2007)) that the increase of LW_in plays a larger role in the interannual variability than suggests Fig 11. However, as said in the conclusion, how are the observations here representative for the whole GrIS variability?

Reply With respect to non-melting conditions, surface temperature indeed reacts very sensitively to changes in LW_{in}. But to make plot 11 we only considered melting conditions, i.e. we summed the energy components during melt. Because surface temperature/LW_{out} is fixed during melt, this graph can also be read as dependence on LW_{in}, offset by a constant value. This means that melt on average decreases with increasing LW_{in}, and that is in turn because SW_{in}/SW_{net} and LW_{in}/LW_{net} are strongly correlated (Fig. 10) but the SW_{net} effect dominates. That is why for melting conditions we feel that the discussion is complete as is. For the question about representativity: see our answer to reviewer 1.

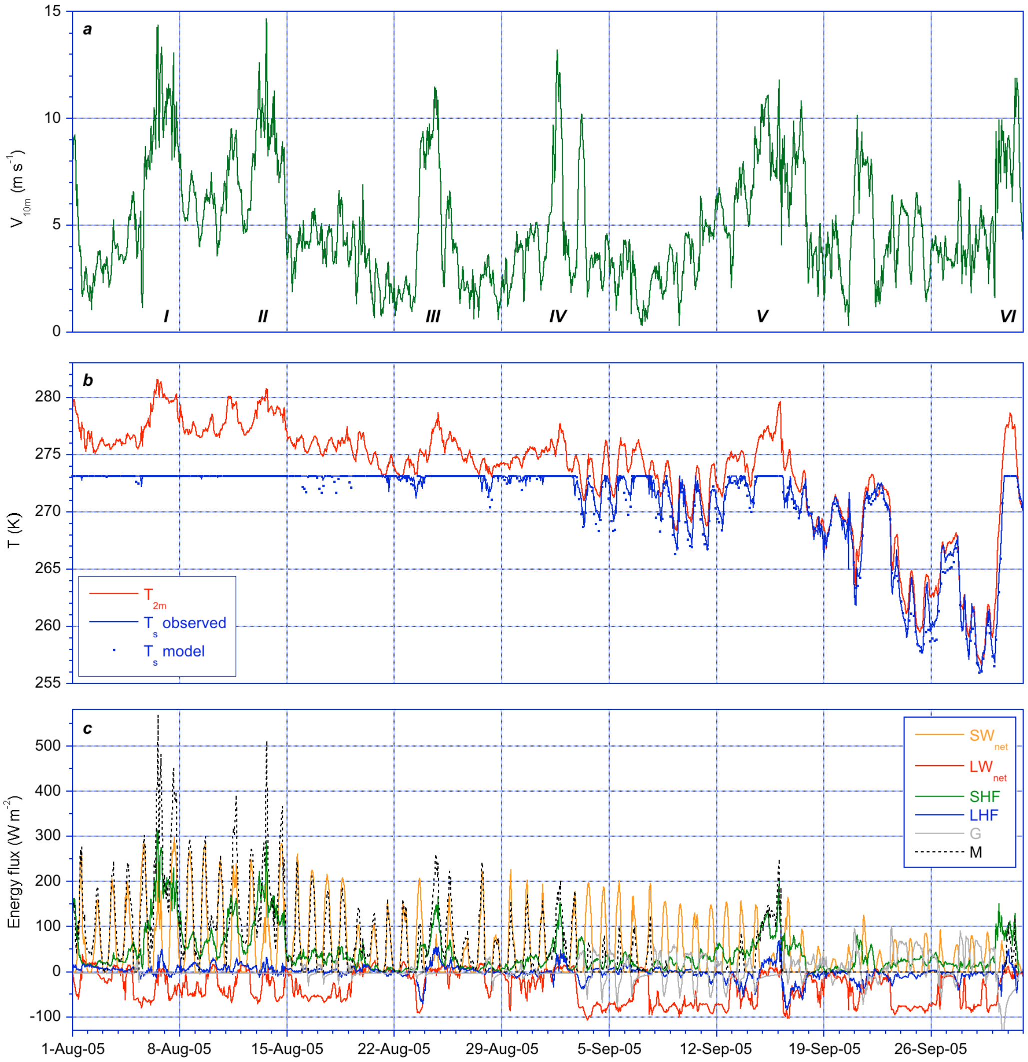

Figure 12: Hourly averages at S5 for the period 1 August – 30 September 2005 of (a) 10 m wind speed, (b) 2 m temperature and observed (lines) and modelled (dots) surface temperature T_s, and (c) modelled surface energy components.