

Interactive comment on “Climatology and ablation at the South Greenland ice sheet margin from automatic weather station observations” by D. van As et al.

D. van As et al.

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We thank both reviewers for their carefully stated comments on our manuscript. To resolve most of the larger issues with the manuscript, we propose to remove the results dealing with the mass balance, as the conclusions drawn are not well constrained by observations. This would involve renaming the manuscript to 'Near-surface climatology at the South Greenland ice sheet margin from automatic weather station observations', and reformulating the aim of the paper, which needed changing anyway. After these changes the manuscript will be fully focused on the near-surface atmosphere and its interaction with the ice. To add to this, we propose to add a figure and more extensive discussion on measured along-slope temperature gradients. Also, we propose to add text to better inform the reader of data reliability, and to better explain the occurrence

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of data gaps. Below we will respond to the comments of the reviewers in detail.

Response to reviewer 1

R1: ... The only point that I would like to see more clearly in the manuscript refers to the nature of their modeling. Unfortunately there are no continuous recordings that enable an extensive model validation. In the MS the authors still state that there is a "comparison" between model and measurements. What they do is, however, more a kind of putting their results into a context by discussing measurements from other sites (which can actually be quite distant even if "nearby" on an ice sheet scale). I do believe that the modeled energy balance characteristics are close to the "real" characteristics, but please state more explicitly that the modeling at this stage helps to interpret, but does not verify the atmosphere-ablation links.

A: We recognize that model validation though comparison to mass balance measurements is vital in ice melt studies, and that the lack of it in our study adds to the uncertainty on the outcome. We agree that our calculations are usable to compare energy budget components between sites, but not to discuss the surface mass budget in detail. Since also reviewer 2 questions the reliability of our results in particular when it comes to the mass budget, we believe it is necessary to remove the results and discussion dealing with the mass balance. Instead, we will confine our discussion to the regional differences in the energy budget, and carefully state the limitations of interpretation.

SPECIFIC POINTS

R1: Multiple times it is talked about atmospheric or meteorological "parameters" (e.g., abstract). I think you should say "variables". Particularly in conjunction with modeling a "parameter" is more associated with one constant value (e.g., surface roughness length).

A: To avoid confusion we will change accordingly.

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R1: Have you thought of measuring LW_out at the stations? It would provide a validation chance for the model (radiative surf. temp. vs. simulated surf. temp.) in the given case S58 of no ablation recordings, since there is at least some variability of surf. temp. I'm sure the authors are aware of this, though, as the first author applied this strategy in past studies.

A: The stations we are currently placing on the Greenland ice sheet in the Promice campaign do measure outgoing longwave radiation, as indeed it is an important variable to use for SEB model validation. The first author was not involved in placing the stations discussed in the manuscript, but believes the choice of longwave radiation sensor was based on balancing accuracy and cost: The Eppley PIR is a higher grade instrument than the Kipp & Zonen CNR1 that we and others currently use, so potentially allowing more reliable SEB calculations. However, as the cost of an Eppley is relatively high, it was chosen to install the bare minimum, i.e. only one to measure LRin.

R1: P118/L23: "Changes in ice dynamics...": Isn't the link between dynamics and mass balance based on a mutual feedback? Indeed, two sentences later you state that surface melt (a mass balance component) impacts ice dynamics considerably. I think you should modify the opening sentence.

A: We will do so, thank you for pointing it out.

R1: section 2.1: Do I understand right that there is no data logger at the stations, and all data are directly transmitted to an office?

A: The data was stored locally in Campbell CR10X loggers. In addition to this, values were transmitted four times per day. These transmissions provide up-to-date records without visiting the stations, and a back-up in the event of not being able to retrieve data in the field. We understand the confusion reading back our writing, and we will modify the sentence.

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R1: section 2.3: What is the model time step of the calculations? Input data are available six-hourly, so was the model run at six-hourly steps or with hourly interpolated data? If I missed the information please ignore this comment....

A: Related to the previous point: our observations were performed every hour. The model time step uses 15 minute data obtained through interpolation. This is done to allow a small spatial sub-surface grid spacing and computational stability of the heat conduction calculations. Six-hourly data can not be used for SEB calculations as it does not resolve the daily cycle sufficiently. We will mention these time steps in a future version of the manuscript.

R1: P124/L11: suggest "exceeds" instead of "is limited by" R1: P126/L12: just write "or assumptions"; as a modeler you always try to impose reasonable assumptions, not "false" assumptions. R1: P126/L20: "temperatures above freezing point" instead of "positive temperatures" (since the latter could be any temperatures in Kelvin and differ between _C and _F).

A: We will make the changes as suggested in the three points above.

R1: P126/L21-24: Is there a reference that supports the explanation through the large-scale circulation?

A: The sentence you refer to suggests the influence of shielding by mountainous terrain and continentality. These are well-known features, but we will attempt to support this statement better.

R1: P137/L13: Wintertime precip. is used in the discussion, but the model only refers to summertime and does not use precipitation input. please clarify;

A: This statement was made in relation to the importance of timing of the beginning of ice melt: little wintertime accumulation allows net ablation to be larger as ice melt can start earlier. This section is likely to be removed from the manuscript entirely following the comments on the mass balance assessment by both reviewers.

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R1: P138/L22-23: write "removes of ice from the mass budget", as you only show in the related table the negative case (and usually the response is not entirely linear around zero). R1: P139/L2: Would starting a new paragraph with "The importance of the timing" be appropriate?

A: Both comments, see above.

R1: Table 3: The way of computing the statistics is clear, but the associated discussion in the text is a bit confusing due to the chosen measures, e.g., can a factor contribute $> 100\%$ to a process? I think to discuss the relative importance of energy balance components for melt either (a) each the negative components and the positive ones should together have -100% and $+100\%$, respectively - then you can discuss the relative importance of factors providing, and of factors removing energy for melt. Or (b) all fluxes are viewed as magnitude only, i.e., $|Q|$, then the relative importance would also emerge nicely. For Nuuk this would thus look as follows. SRnet: 63% (gain), LRnet: 16.8% (loss), SH: 17.9% (gain), LH: 2.0% (loss), SSH: 0.3% (gain). - - If my suggestions do not make sense, or others do not see your way of interpretation as problematic, the original may also be acceptable.

A: The percentages in Table 3 are meant to identify which components are more important than others in relation to melt energy, and are not meant to give their contribution to melt. We understand your objections and agree that it would make more sense to have a balance that adds up to 100%. We therefore will follow your second suggestion, by calculating absolute contributions to melt.

R1: Fig. 3: It would probably help to give for each station the number of available months in the caption;

A: The number of months can be deduced from the amount of circles plotted in the figure. Since the second reviewer made the same suggestion we will state which data was used for the figure.

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R1: Fig. 5: Please provide a period or number of data in the caption;

A: We will.

R1: Fig. 9: The caption only refers to the upper panel. Please complement caption for the lower panel.

A: The caption does state that the figure deals with albedo and ablation, which we will change into albedo (upper panel) and ablation (lower panel).

TECHNICAL COMMENTS

A: We will address all technical comment provided by reviewer 1.

Interactive comment on The Cryosphere Discuss., 3, 117, 2009.

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