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The Cryosphere Discussions

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

Anonymous Referee #2

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GENERAL COMMENT:

The purpose of this paper is to characterize ablation and to provide a climatology for the southern part of the Greenland ice sheet (GrIS) from automatic weather station observations. As stated by the authors, the paper "aims to contribute to the knowledge of melt in the GrIS margin" (P120/L11). However, this aim is not entirely reflected in the results, which are divided into 4 sections: 1. Temperature, humidity and wind, 2. Piteraq winds in Southeast Greenland, 3. Summer surface energy budget and 4. Surface mass budget. There is no separate Discussion, with the authors instead providing their interpretation of different atmospheric and ice sheet processes in the

Results section. The conclusions described by the authors are of interest, in particular their recognition of the importance of wintertime accumulation on mass balance, but the question that begs asking is whether the data presented actually allow such a conclusion to be made.

The major issue with this paper, as discussed already in this forum, is whether the data are of a high enough quality to warrant publication. It is accepted that it is of interest to characterize this part of the Greenland ice sheet, as data are sparse and information is much needed, but this referee shares the opinion of others that the reliability of the data is some what questionable. For this paper to be published I believe the authors should consider making a greater effort to be transparent about the limitations of the data set being used, and to provide specific details of the data gaps (see specific comments below) - only then will we be in a position to assess whether the conclusions presented are warranted. My underlying concern though is that this may still not be enough as it appears that no reliable ablation or accumulation data have been obtained at the measurement sites.

SPECIFIC POINTS:

1. In the Abstract it is stated that "during summer no pronounced daily cycle in near-surface atmospheric parameters is recorded in the three regions" (P118/L9). In my view this requires more careful wording - how do you define "pronounced" and "parameters" when referring to daily cycles. I would argue that a number of variables shown in Figure 4 and 8 show diurnal variability - and that reference to "pronounced" and "parameters" is a little misleading. Given the uncertainty and duration of the measurements I wonder if it is wise to make such a general statement about the southern part of the GrIS.

2. As stated above, the primary aim of this paper is "to contribute to the knowledge of melt in the GrIS margin". Is the focus in this paper really on melt? Much of the paper is devoted to observations of atmospheric variables (in particular wind events), but given

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the shortage of data, it is uncertain as to whether this falls into the broad category of a GrIS climatology. The authors may wish to consider rewording the aim of their paper to ensure their results reflect it, or alternatively, focus more of their results on addressing their present aim.

3. Some specific points about the description of the Methods follow. To clarify some of the uncertainty a detailed Table that carefully outlines data quality (percentage of data obtained and how) is deemed almost essential for readers to show what proportion of data are modified or corrected in some way. For example, it is not clear how much data are required to calculate a monthly total - is some threshold used or are all data available if a monthly total is calculated?:

3.1 It is stated that "in each region a second station was placed to be able to determine along-slope gradients" (P121/L2). Are the second AWSs designed in the same way as the "primary" AWSs - I am assuming that this is the case? However, this is unclear and the caption for Table 2 suggests that they might have been different as not all AWSs are included in the caption. For these reasons it is unclear what "along-slope gradients" are actually being monitored - all variables?

3.2 It might be useful to show an image of the typical AWS - the description of the set up starting on P121/L14 is not very clear (e.g. "small wooden boards with long bolts running through were mounted underneath the feet of the tripods to keep them fixed, without melting into the ice by solar heating" - how did this actually work given the magnitude of melt and how did it affect the level of the instruments?). Some clarification here might allow readers to assess why the data records are so intermittent.

3.3 "Measurements were taken every full hour" (P121/L23). How were your instruments logged - what data loggers were used? This is an important detail as it remains confusing as to why so much data were lost. It cannot be entirely linked to harsh atmospheric conditions as AWS elsewhere on the GrIS are exposed to similar conditions but have continued to log data. How much of the data are hourly, and how much is from transmissions? How were the data transmitted and why were you so dependent on these - did the data loggers fail to store data if power was lost? Some of this information is given in Figure 2 but it is not sufficient to explain the number of data gaps in your time series.

3.4 "In combination with a sonic ranger mounted on the AWS measuring snow accumulation/instrument height this provides a year-round record of surface height variability" (P122/L5). It is acknowledged that you had problems with this design, and that is accepted, but the implication of this is that no measured accumulation or ablation data are obtained, which prevents any robust model validation. This issue is not directly addressed in the paper and needs to be more clearly and honestly outlined to readers, as "year round data" are clearly not available.

3.5 It is not clear how much SRin data are corrected (P123/L9-L26). What proportion of the data is being corrected, and though sensitivity is assessed (Table 4), does this really test the issue of AWS tilt? How robust is your estimate of cloud fraction given the uncertainty in LRin and T measurements (P123/L19-L23)? It is also not clear whether the SRin data are of a high enough quality to be used for energy balance modeling, especially given its importance as an energy source. Finally, you note "after applying this correction method to SR for tilt, large errors may still occur" (P123/L25). Do they or do they not occur?

3.6 Finally, the surface energy balance calculations are clear (except for model time step) and have been carefully described elsewhere by the authors but the uncertainty in this study is the inability to validate the output. In particular, surface temperature, which is solved iteratively, cannot be validated. This creates uncertainty in the sign and magnitude of the calculated terms (see below): thus, it is difficult to ascertain how "sensitivity of the model results to input accuracies or false assumptions" (P126/L12-L13) can be treated, and whether Table 4 reflects the uncertainty of ablation processes adequately.

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4. As stated above, Figure 2 does not provide sufficient detail to ascertain the amount of data lost, modified or corrected. Given the uncertainties, how confident are you in the vertical T gradient at Tas1? This is an interesting finding but in the absence of any relative calibration data or discussion of the magnitude of this gradient, how robust do you think this finding is? If it does occur, are you willing to speculate how far this inversion extends and do you think you are near the top of it?

5. Why do you use RH and not specific humidity in Figure 3 given the importance of absolute humidity gradients in determining the magnitude and sign of LH? This comment is made as the discussion concerning the variable nature (and sign) of LE at your sites is not entirely clear on P134/L14-L25. The sign of LH varies between sites and seems of interest, and though temperature is an important control (as noted), the change in sign of LH must be linked to changes in moisture gradients. This variability would be clearer if you showed specific humidity rather than RH. Also, it is not clear how much data are used to generate any of the multi-year averages (Figure 3) but is obviously limited given your acknowledgement that standard deviations cannot be generated (P129/L3).

6. Figure 4 shows surface temperatures at all sites. These must be modeled temperatures; is that correct? I think this point should be made clearer in the text and Figure caption. They are of interest as Figure 4 implies a freezing surface at "night-time" (periods of low sun angle) in summer, but on P135/L8 it is stated that "on average melt energy values are positive at night resulting from considerable noctural melt during overcast conditions". Why does Figure 4 suggest freezing, while Figure 8 (lower, right panel) and your discussion imply melting at these times? Are these the same periods; the captions suggest both are averages for summer? If this uncertainty does exist it raises questions about the sign and magnitude of all fluxes, as modeled surface temperature governs them all explicitly.

7. It is not entirely clear how section 3.2 (Piteraq winds in Southeast Greenland) is linked to the aim of the paper, which is to "contribute to knowledge of melt in the GrIS

margin". The character of these winds is of interest but the material presented is not entirely relevant to the stated purpose of the paper. Further justification for including these findings might be warranted in the final paragraph of the Introduction.

8. It is difficult to assess the accuracy of your modeled surface mass budget estimates without robust in situ measurements of accumulation and/or ablation (section 3.4). It is not clear how much observational data are available for validation; is it possible to provide this information more clearly and to refer to it more explicitly in a Figure or Table that shows model versus measurements? In the conclusions it is summarized that ablation near the southern tip of the ice sheet is "up to six meters per year" and "a few meters of accumulation occur in wintertime" (P140/L4-L5) - these estimates are a not particularly well constrained - do your data really provide us with new information?

9. Your frequent use of the expression "significant" in the Conclusion (P139-P141) implies your statements are statistically supported, which is misleading as this is not the case. As stated above, it is tempting for you to conclude wintertime accumulation is of crucial importance for the amount of net ablation each year but do the limited data really support this? I agree that this is quite a plausible conclusion but the lack of detailed measurements of snowfall frequency and intensity at all other times, including summer, could be considered by some as important in preventing you from making such a statement. Thus, is it really in our interest as a community for such a statement to be published without it being more carefully scrutinized?

TECHNICAL COMMENTS:

1. P118/L25: I would avoid making the statement that surface run-off has been "proven" to have a considerable impact on ice dynamics through basal lubrication. I think there is strong evidence to suggest there is a link between surface melt and dynamic processes but are we ready to make statements to suggest that it is "proven"?

2. P125/L23: PROMICE as an acronym is introduced but is not explicitly defined though indirectly referred to on P120/L1.

3. Fig. 1. GC-Net AWSs should be labeled as it is implied that they are in the text (P139/L5).

4. I would also favor seeing a larger font used for axis titles for all Figures (as suggested by Referee #1).

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