The Cryosphere Discuss., 3, S100–S112, 2009
www.the-cryosphere-discuss.net/3/S100/2009/

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.

D. van As et al.

Received and published: 27 March 2009

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

———————————————————————————-

Response to reviewer 2

R2: 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.

A: We agree with reviewer 2 on all points above and suggest the following. First of all, we will restate the aim of the manuscript, as this indeed points merely to ablation, which is in fact certainly not the sole aim. Thank you for pointing this out. Even more so, we suggest to remove the section dealing with the surface mass budget, as all reviewers rightfully mention that conclusions are not based on observations sufficiently. The surface energy budget will have to be presented with more precautions, as we have no solid means of verifying our calculations, and should more reflect a regional intercomparison study, highlighting differences in atmospheric forcings between our three regions. We do not fully understand which data is addressed when the reviewer mentions that the reliability of the data is questionable. All sensors on the station – except the pressure transducer – are widely used over snow and ice surfaces with great success. The variables were corrected where necessary, using the best methods known to the authors. Inaccuracies are reported in the manuscript, and a sensitivity study is performed to identify the impact of inaccuracies on the surface energy balance. We believe that the comment of the reviewer might be referring to the fact that our calculations are not validated by mass balance measurements. We hope that removing the mass balance section will give the reader a greater overall confidence in the quality of the results. Finally, we agree that the manuscript will benefit from going further into detail concerning the difficulties of obtaining a continuous dataset. Such an exercise might also demonstrate to the reviewer that the data gaps are less abundant than he/she thinks. For instance, our energy balance calculations require several measurements: temperature, humidity, wind speed, shortwave radiation and longwave radiation. One failing sensor causes a gap in the SEB calculations, but not in the data series of the other variables. The reasons for having data gaps are numerous, such as piteraq wind damage, the lack of funding for logistical cost related to weather station maintenance, and using strict criteria for data quality before considering data to be publishible. We will address the nature of our data gaps more thoroughly, and discuss the issue further below.

SPECIFIC POINTS:

R2: 1. In the Abstract it is stated that "during summer no pronounced daily cycle in nearsurface 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.

A: We agree that we should state this more carefully and will make appropriate changes to the text. We do not see why the reviewer believes that there is a large uncertainty in the measurements (the inaccuracies reported in Table 2 are valid for all our measurements), nor why one or more summers of near-surface measurements would not be sufficient to draw conclusions from (several weather station studies in the past describe only a few weeks of summer measurements).

R2: 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 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.

A: As stated above, we fully agree with you; the aim of the paper as mentioned at the end of the introduction does not agree with the results later on in the manuscript. We will reformulate the aim accordingly.

R2: 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?

A: We will more clearly state which data is available from which stations over which period, and give a more detailed explanation of the occurrence of data gaps. Naturally we did not modify any data, but performed the corrections as mentioned in the methods section consequently and uniformly. The comment on the percentage of data needed to be able to calculate monthly means is welcome and the value will be included in the text; we used a threshold of 50% below which we considered the available data to be non-representable for the whole of the month. Nearly all months presented in the figures have a full data coverage.

R2: 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?

A: All stations were identical with a few minor exceptions as already shown in Table 2. The two stations in each transect were fully identical. As we do not use the secondary stations to a great extent in the manuscript it was chosen to leave these stations out of the discussion and focus on the stations that the manuscript deals with. However, to avoid confusion we will include a few words on the matter.

R2: 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.

A: The intermittent nature of the measurements will be explained in more detail as explained above and will be further addressed in the following comment. It is a good idea to include a figure of one of the stations; we will do so. This will help to explain the set-up of the system, including the wooden boards. These work very well, and in the way described in the text. The station's feet do not melt into the ice, keeping a fixed instrument height in summer, and avoiding possible large stresses on the tripod by changes in the ice surface which are know to be able to break the steal wires used in the design. However, on no occasion we found that these boards prevent the ice from melting underneath. So the station lowers with ice ablation, but does not melt into the ice. The figure and a few additional words will help to understand.

R2: 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.

A: We will add to the text that the data was stored locally by Campbell CR10X loggers, which are widely used for the same purpose. The reason why our data records are more intermittent than those of other AWSs elsewhere is because conditions ARE harsher in the southern ablation regions. For instance, measuring at temperatures far below freezing such as in the accumulation zone demands less from your sensors as there will be no cycles of moisture freezing and thawing on the sensors. Especially the sonic ranger membranes experience noticeable degradation due to this. And the wind speeds in the southeastern ice margin ARE higher then elsewhere on the ice sheet. The Kulu station in the US GC-net was placed in approximately the same region as our Tas stations, but taken down after a short deployment as the station was suffering from wind damage. Precipitation IS largest in the south(east)ern regions of the ice sheet, on occasion covering stations for the most part, causing a wet snow pack to completely cover radiation sensors, logger enclosure and battery box, and pulling down on sensor cables during compaction. The high accumulation is presumably the reason for finding the domes of the radiation sensor broken on at least one occasion. Ablation IS higher than in most ablation regions of the GrIS, causing surfaces to differ a great deal from the start of the ice ablation season to the end, with ice hummocks of more than one meter high migrating past the stations (TAS and Nuuk), and crevasses nearby (Qassimiut region). To be more specific we can mention that station 72 was simply discontinued, partly as the station was placed in the accumulation zone. The 2002 data gap of St71 in Fig. 2 is because the station was not found upon return and is likely to have fallen into a crevasse, which is a risk when placing a station in spring when the surface is snow-covered. The lower Nuuk station was placed in the lower regions of a fast-flowing calving glacier. Photos of the deployment of the stations reveal a heavily crevassed terrain. The first author does not know why this glacier was chosen for AWS placement, but it is no surprise that after not being able to revisit the station the two following years, it was impossible to find and retrieve in 2006. The upper Nuuk station has been operational for its entire deployment period, though the uneven surface made the tripod legs bend until the station collapsed a few months before retrieval. The Tas3 station only has a short data record as a new station needed to be placed in 2006 after not being able to find Tas2. The upper station produced a data gap in 2005 due to a failing data logger. So there have been many different types of set-backs and the challenges are great, but that should not prevent us from measuring in these places. Instead, it gave us ideas on how to improve our station design, and continue the data series.

R2: 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.

A: This is true. By taking out the mass balance section we only partially resolve the issue, as the surface energy balance calculations still remain unvalidated. We will make it very clear in the text that even though we use a model that includes most aspects of the air-surface interaction and reliable data that the total balance can not be validated by surface mass balance measurements. The link to the mass balance will be kept small, and the focus will go out to the regional differences in the SEB assuming equal surface roughness and initial ice temperatures at all three locations.

R2: 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?

A: The SR instruments that were used at the stations are considered to be a higher class instrument than those currently used at most other AWSs on Greenland to the first author's knowledge. All SRin data undergo the tilt correction as for every SRin measurement we have a tilt measurement. For small tilts and heavy cloud covers the correction will be small, but for larger tilt values and clear skies the correction will become necessary. Measurement errors in LRin and temperature will not affect the outcome of the correction much, but the uncertainty whether the cloud fraction obtained from them is representable. Other studies have investigated this in detail, and it is out of the scope of this manuscript to investigate further. Other AWS studies use similar correction methods without reporting the details of the tilt correction, sometimes because tilt itself is not measured continuously. We discuss the tilt correction to some detail because it is crucial to include in the data processing. There is only so much you can do to removes errors from datasets, and we believe we have sufficiently dealt with this by using the mentioned correction method.

R2: 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.

A: The validation of the calculations remains an issue indeed, but less than for the mass budget as surface temperature is not a free variable for most of the modelled periods, as it is 0 C. One of the main reasons for having Table 4 is to be able to give confidence in our approach. The model itself was thoroughly validated over snow surfaces in past publications, but because of the lack of validation possibilities we offer an error estimate much like an ensemble study: choosing the model parameters as accurate as you can and see what the impact of a reasonably-sized error in input variables or model assumptions is.

R2: 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?

A: Figure 2 is meant to present temperatures for the ablation zone. No data has been modified, and all the corrections are listed in the methods section. Gaps in the data set have been discussed above. We agree that the temperature inversion in southeast Greenland has not been thoroughly discussed, but briefly mentioned. We will include a figure on the temperature gradients in the three regions and quantify these gradients. We are unsure what you mean by relative calibration data. Measurement inaccuracy is given in Table 2. We will not speculate on how far the temperature inversion continues inland.

R2: 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).

A: We find to plot relative humidity more insightful in the context of this manuscript as it shows that there is little difference between the values measured at the three regions, as well as that there are nearly constant values during the mean summer day. A plot of specific humidity at 2m and at the surface will show the gradients that serve as input for the latent heat calculations, but changes in specific humidity will almost completely reflect changes in temperature. The fact that the sign of the gradient determines the sign of the latent heat flux is common knowledge. We will make an effort to clarify the text dealing with the latent heat flux. Also, only for wind speed a few monthly means are missing that prohibit the plotting of the full yearly cycle. The choice not to include standard deviation, but the actual mean values over several years, was made to give

TCD

3, S100–S112, 2009

Interactive Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

a better insight in the spread and availability of values. As discussed before, we will include a better overview of the available data.

R2: 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 "nighttime" (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.

A: Both the solid and dashed lines in Fig. 3 are calculated, as stated in the caption. Temperature, humidity and wind speed are not measured at exactly 2 m above the surface, so we used the surface energy balance model to calculate screen-level values. Fig. 4 and 8 indeed cover the same period. The reviewer is paying attention in detecting the sub-freezing surface temperatures while melt continues at night. But the confusion is caused by averaging; an overcast night with a melting surface, followed by a clear night without melt show both sub-freezing temperatures and melt after averaging. We will make this clearer in the text, as we will do with figure 3.

R2: 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.

A: The reviewer is correct in noticing this. As discussed above we will reformulate the aim (and title) of the manuscript to agree with the results.

TCD

3, S100–S112, 2009

Interactive Comment

R2: 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?

A: Agreed, but this problem will be solved by taking out the mass budget section and revising the conclusions where needed.

R2: 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?

A: We agree that strictly speaking we use the term 'significant'; wrongly in the conclusions, albeit only twice. We will rephrase. That the wintertime accumulation and thus timing of the start of ice melt is important to the net mass balance is not a new finding in glaciology and is not intended to be presented as such. We wanted to confirm this for the regions we show measurements of. We will have to rephrase this so that it more clearly describes our findings for Greenland: that year to year differences in net ablation depends on wintertime accumulation, amongst other factors.

TECHNICAL COMMENTS:

R2: 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"?

A: We will change the sentence to avoid problems.

R2: 2. P125/L23: PROMICE as an acronym is introduced but is not explicitly defined though indirectly referred to on P120/L1.

A: True, thank you for pointing this out.

R2: 3. Fig. 1. GC-Net AWSs should be labeled as it is implied that they are in the text (P139/L5).

A: The GC-net stations will no longer be mentioned in the text after removing the text on mass balance and Fig. 10.

R2: 4. I would also favor seeing a larger font used for axis titles for all Figures (as suggested by Referee #1).

A: We will increase the font sizes.

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