The Cryosphere Discuss., 3, S28–S32, 2009
www.the-cryosphere-discuss.net/3/S28/2009/

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: 4 March 2009

Dear Mauri Pelto,

First of all, thank you for your effort in commenting on our manuscript, it is much appreciated, and will definitely help to improve the end result. We agree with your general comments in that the intermittent nature of the observational records stands in the way of drawing clear conclusions concerning the year-to-year variability when it comes to energy-balance and ablation calculations. However, we do not agree with your assessment that our results are substantial nor important. The manuscript starts out by presenting and comparing observational records, which to some might resemble a field report, agreed, but in our opinion weight is added as: - results are for three loca-

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tions in three different climate zones where no AWS data has been gathered before, - with longer time series than all other AWS records in the Greenland ablation zone, with the exception of the K and JAR transects, - presenting important features that have not been mentioned in (English) literature before, such as the Piteraq and summertime stratification in the southeast, - and giving rare observationally-based ablation estimates in Greenland, and explaining the regional and temporal differences by going into the details of the surface energy budget. We realize that it is hard to compete with the K and JAR transect datasets in terms of continuity, but we argue that our dataset is very valuable, and adds to our understanding of regional differences in the Greenland ablation zone. We have the impression that your focus mainly goes out to the surface energy and mass budgets, as all of your suggestions concern these topics. However, please be aware that the manuscript deals with the South Greenland climatology, of which the energy and mass budgets are components. This means that a detailed study of the energy budget is necessary, but attention should go out to other facets of the South Greenland climate as well. Naturally, in a revised version of the manuscript we will try to put the energy and mass budgets more into the perspective as offered by other publications. See also below.

MP: 1)Tedesco et. al., (2008) using ssm/i data generated energy fluxes using a MAR model for the same region. From Figure 3 of that paper it seems evident that the MAR results point to a higher sensible heat flux at TAS1, higher net long wave fluxes at Nuuk2 and higher net shortwave fluxes at St71. The MAR results are for the 2003-2007 period, and provide a long term mean for comparison. I may have misread the location of TAS1 on the MAR maps, but this is the point. The MAR results should be compared to the AWS energy model results of this paper. This would require locating the three sites on one of the MAR maps. In comparing the MAR results to Table 3 the results seem to agree.

Authors: We believe you are referring to Fig. 2 of the Tedesco et al. (2008) TC paper. Please correct us if we are wrong. Unfortunately, that paper only shows 2007 anomaly maps so can not suit our goals of validation. Furthermore, comparing mean values of different periods will produce differences too large to explain in terms of temporal variability or inaccuracy of one of the two methods. And then there is the matter that the MAR model does not fully resolve the narrow ablation zone of the Greenland Ice Sheet, at least not down to the level where we can use it to compare to our point measurement. What the Tedesco study could provide very well is a spatial distribution of the energy fluxes, although the mean values were not included in the TC paper. We appreciate any suggestions on how to place our station results in a better perspective of spatial variability.

MP: 2) The Van den Broeke et. al., (2008) paper use AWS results to look at energy fluxes at three stations moving inland from the southwest GIS margin. This paper is referenced, but the results are not sufficiently compared. Van den Broeke et. al.,(2008) find that net shortwave fluxes increase inland, this is in agreement with the observations of higher net shortwave flux at Nuuk2. Van den Broeke et. al.,(2008) also note the sensible heat decline with inland distance, does this fit the results from St71- St 72, or TAS1 versus TAS3 however limited or for the three stations. It is noted that temperature increases with altitude at TAS1 versus TAS3, does this impact sensible heat flux change?

Authors: We agree with you and will produce a more thorough comparison with the K-transect data in a following version of the manuscript. It will be difficult to compare vertical gradients in surface energy fluxes however, since our three ‘transects’ did not produce sufficient data to calculate energy flux gradients. The striking temperature gradient at Tas in summer may well impact the vertical gradient in the sensible heat flux. We do have wind speed at both stations which does not show a gradient, so we could assume that over a melting surface and for an unchanging surface roughness the sensible heat flux at the Tas transect will increase with elevation. But since the decrease in surface roughness with elevation is one of the primary causes of the sensible heat decrease with elevation at the K-transect it is very difficult

TCD

3, S28–S32, 2009

to compare, and we will not include this in the manuscript.

MP: 3) Since the paper focuses considerable attention on the technical difficulties, the specific limitation and problems associated with the pressure transducer ablation measurement system need to be discussed. It cannot at present be determined how reliable or extensive the measurements obtained are. Given the problems it is appropriate to comment on what design changes if any would have provided a more resilient, redundant and reliable measurement system.

Authors: We prefer to stay away from too many technical details for sensors that we do not show results from – except for giving a rough estimate for St71 in the text. In the near future we will have a manuscript ready that shows a successful 4-year pressure transducer record in the Melville bay area, which seems a more appropriate moment to list problems and solutions concerning the pressure transducer. We hope you agree. However, we will attempt to give a more balanced discussion of the technical difficulties in the current manuscript.

MP: 4)The albedo results in Figure 9 indicate the ability to determine the length of the ablation season. There should be some satellite imagery to confirm the snowline position at least for one location in one season. The lack of accumulation assessment is problematic. Based on the results what is suggested for gathering such information using an AWS system.

Authors: When using satellite data to determine the snowline position we run into the same problems as using our own albedo data, as there is no distinct drop in albedo for two of our three locations when the last of the wintertime snow pack is melted away. For the St71 station it would be possible to determine the moment the ice surfaces using satellite images, as here the ice has a low albedo, but for this location there is no doubt concerning the start of the ice ablation season. (From a more general point of view, we believe satellite data should be validated using ground measurements, not the other way around.) The lack of accumulation data is indeed undesirable, which is why we are forced to only discuss the net ablation (i.e. ice ablation) in the manuscript. The approach that was used in measuring the surface height changes at our AWSs was solid in theory, by using a sonic ranger on the AWS to measure snow height, in combination with a pressure transducer measuring ice ablation. The problem is that the sonic ranger is more delicate than most sensors, and the pressure transducer needed further testing. We did get some of the time series: the sonic ranger at Nuuk2 showed a large snowfall event in the spring of 2004, and the one at Tas1 reveals accumulation late 2004 and 2005. The pressure transducer at St71 gave reasonable values in 2004-2006, but we do not have enough faith in the absolute values to place a figure in this manuscript. All in all, we decided we did not have enough mass balance observations to be able to draw conclusions from. However, in a future version of this manuscript we will include a few words on the sonic ranger results as mentioned above. In our current station design we have further developed the pressure transducer and use two sonic rangers (one on the station and one on stakes). Results are promising, partly because we change sonic ranger membranes each year where possible.

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