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The Cryosphere Discussions

Interactive comment on "Partitioning of melt energy and meltwater fluxes in the ablation zone of the west Greenland ice sheet" *by* M. Van den Broeke et al.

M. Van den Broeke et al.

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Answers to comments on the paper:

'Partitioning of melt energy and meltwater fluxes in the ablation zone of the west Greenland ice sheet' by M. R. van den Broeke and others.

First I would like to thank the reviewers for providing their extensive and insightful reviews, which have helped to improve the paper. We have streamlined the text in the paper and added two figures and a section to clarify several points. The revised manuscript has been sent to the editor.

Answers to comments by Mauri Pelto

Q: Are there any quantifiable observations of surface flux? Have there been any changes in surface roughness at S5 or S6 from 2003-2007? If so this could be an important potential change in the melt energy for such a region. I have noted considerable surface roughness increases after several years of persistently high ablation in various sections of the ablation zone of temperate alpine glaciers.

A: Indeed, there are direct observations of SHF and even of LHF; in spite of the considerable technical challenges, our technicians have succeeded in collecting year-round eddy correlation data from the ablation zone at S5 and S6. These data have been partly reported in: Smeets, C. J. P. P. and M. R. van den Broeke, 2008: Temporal and spatial variation of momentum roughness length in the ablation zone of the Greenland ice sheet, Boundary-Layer Meteorology 128, 315-338. They do however only cover one year of observations so are not suitable to deduce an interannual trend. Based on those results, we have developed a method to use our two-level measurements for z0 calculation over the longer four-year period. The first results do not suggest that there is a trend in the summertime roughness, but these results need more refinement and are too premature to report here.

Q: 712-25: It is noted that the narrow ablation zone has been the location of the largest changes in ice flow. Must be careful with this as the greatest ice flow changes are in the ablation zone of marine terminating outlet glaciers, not simply in all areas of the ablation zone.

A: Noted, thanks. Has been replaced with: 'and where recent changes in marine terminating glaciers have been the most pronounced'

Q: 714-10: What about the surface conditions at S6. In fact I would like to see Table 2 include a measurement if one exists for surface roughness at the three locations.

A: An average end-of-summer value of z0 has been included in the revised version of Table 2.

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Interactive Comment

Q: 718- 15-20: Need a Figure for this observation of M, or of the observed versus modelled Ts.

A: This would need three additional figure frames (three sites), which I think is not justified given the fact that the focus of this paper is on mass balance. During melting conditions, surface temperature is constant and does no longer relay information about excess energy. So we decided not to include these in the paper.

Q: 720-8-10: I like the use of the 10 day average melt rates. I have found ablation difficult to quantify accurately from weather records at the daily level. Is this part of the reason for this time period?

A: Indeed, daily melt rates are at the limit of the accuracy of the sonic height ranger and cannot be used for a proper comparison.

Q: 721-20: Any measures over long period of changing surface roughness that would affect SHF at either S5 or S6?

A: Not likely, but based on the information we have it cannot be excluded either. Is being worked on, but trends in melt are not the scope of this paper.

Q: 721-: Return to the advantage of this method to the degree-day model of Braithwaite.

A: Obviously, having all necessary data at hand enables us to do the full SEB calculation. The degree-day method has been designed for situations that these data are not available, e.g. to calculate melt in ice sheet models. It is therefore neither very useful nor fair to compare the methods, as they have very different applications.

Q: Figure 1: A more zoomed view of the three measurement sites.

A: Because none of the other reviewers commented on this, we decided to keep the figure as it is.

Q: Figure 4: The melt models greatest relative errors are for melt rates below 10 kg/day makes sense but explain.

A: We added: 'The largest relative errors are found for 10-day periods with low melt. The reason for this may be that for these low melt amounts ($<$ 10 cm), the measurement uncertainty becomes relatively large and dominates the comparison.'

Q: Figure 5: Why is S6 melt so much lower than S5 in 2004?

A: The ratio of total melt S6/S5 is not significantly different in this year compared to the other years, about 35%. The 10-day peak values however are lower than in other years, the main reason being the absence of a prolonged period ($>$ 10 d) of sunny weather that coincides with a 10-day averaging period.

Q: Figure 6: I doubt hourly rates are either important or reliably determined, not that this changes the results of this figure.

A: Agree, plot is meant to show that daily cycle in melt rate as suggested by the sonic height ranger is indeed reproduced by the model.

Q: Figure 7: Add a figure showing the difference for 2007 surface energy balance versus the longer term results, for at least one site, this could be 7(c).

A:We feel that a time series of four years is not sufficient to define a climatology; with which to compare 2007 data. For the moment we feel that the discussion as is provided now suffices: 'Note the exceptional melt in 2007, where melt and runoff are greater than previous summers by a factor of 2 and 2.5, respectively. Anomalously sunny conditions accelerated snow melting in July and revealed the dark ice surface at the beginning of August. The difference is most pronounced at S9, where SWnet dominates the energy balance during melt (Fig. 7).'

Answers to comments by Dirk van As

Q: In section 2.1 there is no mention of measured ice temperatures, while in section 3.3 it is said that initial sub-surface temperatures for the surface energy balance calculations come from measurements. Even though the initial temperature profile does not affect the melt calculations a great deal, a clarification would be appreciated.

A: The following sentence has been added to section 2.1: 'Ice/snow temperatures are not being continuously measured at the AWS sites, but a single measurement effort was done in 2000 when 10 m ice temperatures were measured at all sites along the K-transect. These data have been used to initialize the model.'

Q: Section 3.4: The authors mention two ways to calculate the energy balance, one using measured surface temperatures ("more realistic"), and the other searching for equilibrium surface temperatures ("more objective"). Why do the authors choose one method for one publication, and the other for another?

A: In a paper that deals e.g. with just turbulent heat fluxes or radiation fluxes, it is not necessary to solve the full energy balance and we chose the most direct method to calculate these fluxes by using observed values of Ts. In a study that deals with the full energy balance which also yields melt, such as the one under consideration here, we cannot use 'measured' Ts to determine when melting takes place because, as we tried to explain in the paper, there is no objective way to decide when the measured Ts indicates melting. To make this more clear we have changed the sentence into 'A more objective method to detect and quantify melt..'.

Q: And you mention that surface temperatures of the two methods are within 1.5 K of each other. What is the root-mean-square difference? Is there a systematical component as well? I'm asking since one degree off has a large impact on the near-surface gradients, and therewith turbulent heat flux calculations.

A: The mean differences for the hourly means of Ts are -0.26, 0.24 and 0.22 K and the RMSDs are 1.26, 1.24 and 1.00 K at S5, S6 and S9, respectively. It is true that small errors can have large consequences for the turbulent fluxes, but not when melting takes place, when Ts is fixed. We clarified this by adding 'Van den Broeke and others (2008c) show that modelled and observed Ts agree well, with average differences $<$

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0.3 K and RMSD $< 1.3\ ^\circ$C, giving confidence in both methods. Furthermore, when melt occurs, Ts is fixed and calculated melt rate is not affected by uncertainties in Ts.'

Q: Section 4.1: I don't agree that there is low accumulation at S5. The picture shows that the station is placed on an ice hill and any accumulation is bound to be eroded away. In between the hills there must be significant accumulation. What does the site look like in spring, and how much area-averaged accumulation would you estimate at S5 from your experience at the site?

A: This is an interesting point. We have decided to add two photos of the situation as new figure 4, and to add the following discussion to section 4.1: 'Because at S5, the sonic height ranger stands on a small ice hill, it will not register snow that collects in the surrounding gullies. Fig. 4a shows an aerial view of the accumulation pattern close to S5 in April 2008, at the end of winter and before melting started. It shows that a shallow snow cover is present in the gullies surrounding the ice hummocks. The image taken on the ground (Fig. 4b) confirms that the ice hummocks remain snow-free while the surrounding gullies are filled with a shallow snow layer. The lack of a continuous snow cover on the tundra hills in the background suggests that winter accumulation is indeed small in this area, and certainly less than sites higher up along the K-transect. As a result of the low winter accumulation, ice melting at S5 starts as early as May and continues well into September.'

Q: Fig. 3: The accumulation at S6 in 2005 looks strange. Is this realistic?

A: This has been explained in section 2.2: 'Surface height data at S6 are missing for the spring of 2005; for this period snow height, onset of melt and disappearance of the snowpack were estimated using the melt model described in the next section. Individual missing snow height data were linearly interpolated.'

Q: Section 4.2: You only used ice ablation to validate the melt model, since you have information on snow density, making it difficult to translate the height change measured by the sonic ranger into meters of water equivalent. However, if I'm not mistaken you do use these SR data to add snow in your melt model, using a fixed density of 500 kg m-3. I realize that uncertainties will be larger in comparing measured and observed snow melt due to this snow density issue, but why not try? Perhaps you can even finetune the results to find an optimal snow density. This way you might be able to produce a Fig. 7b for snow as well.

A: Actually, we have done this: by assuming the model calculated amount of snowmelt to be correct, combining this with the changes in snow depth gives us an indication of the snow density, with 500 kg m-3 as a result. It is then no longer justified to use this as model validation, because the model was used to obtain the result. We added a sentence to section 3.3 to clarify this: 'This value was obtained by assuming the modelled snowmelt energy to be correct and combining this with observed snow depth changes.'

Q: The end of the 2007 melt season is not included in the figures since the data was collected earlier. How far off are we approximately if we conclude that the melt season ends where the dataset ends?

A: Judging from figure 2, which includes the stake measurements which coincide with the station visits, an additional 20-40 cm of ablation occurs at S5 after the end of August.

Q: Section 4.3: I'm a bit puzzled by the meaning of Fig 7a, showing the average flux size per time unit with surface melt. Fig. 7b is much more insightful, telling which fluxes caused how much of melt. Could you motivate why you chose to present results in the manner as you did in Fig 7a?

A: The reason is that melt rate and melt duration both influence total melt, so we decided to include information about both. For instance, an interesting insight provided by these figures is that is that net SW radiation is on average a smaller contributor at S5 than at the other stations, because melt at S5 already begins when the solar zenith angle is still large and similarly ends late in the year. In spite of this, melt rate is highest

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at S5 because of the important role of the sensible heat flux.

Q: Section 4.3: Please state how much mass is lost/gained through sublimation/deposition/evaporation/condensation, or that it is insignificant.

A: In new section 4.5, we added: 'In this study we focus on the partitioning of the meltwater flux. Rain is not detected by the AWS nor modelled, while sublimation constitutes a small mass loss in the ablation zone, on average 1.3% (S5), 1.0% (S6) and 2.0% (S9) of the total melt flux over this four-year period.'

Q: Section 4.4: You mention that refreezing of melt water reduces the total run-off by 8% at S6. But refreezing in the model only takes place in snow, which is melted off ever year. In my mind, your statement then implies that the ice horizon is reached later due to sub-surface refreezing, thus reducing total run-off. However, think that I read in an earlier section that refreezing only heats lower snow layers, it doesn't add mass. So if the snow is heated by refreezing, this means the ice horizon should be reached earlier in the season. Thus sub-surface refreezing should increase total run-off in the model. Where's the flaw?

A: It is true that the model does not keep track of mass in the snowpack, and we treat snow depth as input. When we mention 8% reduction in runoff, we mean that 8% of the total meltwater that is produced at the surface does not run off.

Q: Please state was happens to total melt amount if one doesn't use radiation penetration or melt water refreezing.

A: We added a new section 4.3 'Effect of radiation penetration' and new figure 8 to clarify the effect of radiation penetration, which typically melts the ice down to a depth of 0.5 m. Not taking refreezing into account can be simulated in several ways: or we assume the snowpack to be isothermal at 0°C throughout the year, or we assume meltwater to runoff at the surface of the snowpack, or we assume that meltwater percolates through subzero snow without refreezing. All three scenario's are physically unrealistic and would impact on the surface energy balance, so we prefer not to confuse the reader with their outcome. As the model stands now, refreezing already is dealt with in a rather schematic way, and it would take a serious refinement of the model and more (snow) observations to justify a sensitivity test of this sort.

Q: Technical correction: figures 3, 4, 5, 7: Whereas in one figure S5 results are given in blue and S6 results in red, it is the other way around in another. Consider changing to a uniform colour coding.

A: Thank you for pointing this out, when sites are compared in a single graph we now stick to a uniform colouring: S5 blue, S6 red, S9 green.

Answers to comments by Anonymous referee #1

Q: P714, 23-26. The claim regarding "problems associated with ill-functioning sensors could be adequately addressed" is a little vague. Are all the issues concerning post-processing and data correction dealt with in detail in Smeets and Van den Broeke? If not, it would help to have a little more detail/explanation here (or at least some indication of the % of data that required adjustment; I suspect it is very small and if this is the case, would give much less concern for what might be seen as a rather brief dismissal of data quality issues).

A: Data loss at the AWS over this period is $<1\%$, but the data having received some form of correction is much larger. However, a single percentage cannot be given, and the reader is referred to the referenced papers for details. In the paper by Smeets and Van den Broeke (BLM) an extensive description is given how temperature and humifity data were treated. In Van den Broeke and others (JGR, 2008) the radiation data treatment has been detailed.

Q: P715, 2-3. Over exactly what time period is the surface height data missing for S6 in spring 2005? The snow-depth prediction at S6, as reconstructed from the melt-model (Fig. 3), appears to generate a snowpack that is disproportionately deeper in 2005 relative to snow-depth at S9 when compared with the additional three years when data is available. Is there any explanation for this? In addition, it would help if the dates the sonic height ranger was not working were either added to the captions for Figs 3 and 5 or the actual Figs were amended to make clear where the data is real as opposed to inter polated/modelled.

A: To clarify this, we have added to section 2.2: 'Surface height data at S6 are missing from mid-January to mid-June 2005. Melt energy from the energy balance model (see next section) in combination with a constant snow density of 500 kg m-3 have been used to reconstruct the date and magnitude of the maximum snow depth.' We also added to the caption of Figure 3: 'Surface height data at S6 are missing from mid-January to mid-June 2005, and have been reconstructed using output of the energy balance model (see text).' We agree with the reviewer that this generates a deeper snowpack at S6 than would be expected from other years, but at present we have no way of validating/rejecting this result.

Q: P715, 17. Gs will presumably be dependent on the temperature profile of the ice? Is this known from thermistors and if not, how sensitive are your calculations to this omission? This issue recurs on P717, 8-9 when you state that "the snow/ice temperature profile is initialised using measured ice temperature data"; measured where?

A: The following sentence has been added to section 2.1: 'Ice/snow temperatures are not being continuously measured at the AWS sites, but a single measurements effort was done in 2000 when 10 m ice temperatures were measured at all sites along the K-transect. These data have been used to initialize the model.'

Q: P716, 19-20. It is not clear what is meant by "a 20 day running mean from the AWS profiles". Whilst this may be dealt with in the submitted Van den Broeke paper, an extra line (few words) would help if only to clarify that the "AWS profiles" referred to here and used to calculate z0 are wind speed profiles at x number of anemometers. In addition, it would be good to add z0 values to Table 2, especially since the mean wind speed at

S5 is the lowest yet the SHF is highest because of surface roughness.

A: To clarify this point, we have added end-of-summer z0 values to Table 2 and the following description to section 3.2: ’Because wind speed, temperature and humidity are measured at nominal heights of 2 and 6 m, the gradients can be used to calculate the surface roughness for momentum, z0, using similarity theory. To reduce the uncertainties associated with z0 determination form only two measurements levels, but still obtain temporal information, we adopted a 20-day running mean of z0. It is shown in Van den Broeke and others (2008b) that at S5 and S6, z0 reaches its maximum value in late August (˜0.01 m) and the minimum value in March (˜10-4 m).’

Q: P717, 27 - 718, 1-2. Snow density is kept constant at 500 kg m-3; can you be more specific as to what (and when) are the observations made from which the snow density is prescribed? It is not clear why this chosen value "ensures that snowmelt stops and ice melt starts at the correct time". For example, what effect would selecting a snow density of 400 kg m-3 have on your results?

A: By assuming the model calculated amount of snowmelt to be correct, combining this with the changes in snow depth gives us an indication of the snow density, which resulted in the adopted value of 500 kg m-3. So using this value is consistent with the waning of the snowpack in early summer. Because snow depth is prescribed in the model, using a value of 400 kg m-3 would not change the timing of snowpack disappearance, it would only influence subsurface heat conduction which depends on density. To clarify this, we changed the last sentence of section 3.3: ’Prescribing snow depth in the model ensures that snowmelt stops and ice melt starts at the correct time.’

Q: Section 3.4, P718. It is not clear why two different methods have been used in two different papers (using the same data) to calculate M? If the method used in this paper is more "objective", are you simply saying that the previous method used is not as ’appropriate’ even though your results give you "confidence in both methods" (i.e. in hindsight, do you now recommend this approach?). It would certainly help if a little more detail was provided to explain how Ts is calculated in this paper. The description in lines 12-14 is not really adequate, especially the statement "This equation is then solved for Ts by bisection in a 15K search space around the value of the previous time step".

A: There is a good reason to choose different methods in different papers. In a paper that deals with just turbulent heat fluxes or radiation fluxes, we chose the most accurate assessment of these individual fluxes by using observed values of Ts. In a paper that deals with the full energy balance and melt, such as the one under consideration here, we cannot use 'measured' Ts to determine when melting takes place, as explained in the paper, because there is no objective way to decide when the measured Ts indicates melting (because of the uncertainty in Ts and the upper limit of Ts when the surface melts). We rephrased and extended the paragraph to better reflect this.

Q: P720, 11-12. Is there any simple explanation for the small melt overestimate at S5 in 2005? Given the excellence of the model performance elsewhere, it would be interesting to know if further interrogation of the raw AWS data might reveal a single sensor error.

A: We have made a large effort to further improve the agreement, but found no single factor that can be blamed for the melt overestimate in 2005.

Q: P720, 14-21 and Fig 6. Is Fig 6 really the best way to qualitatively assess the melt-model at daily/sub-daily time-scales. A plot of modelled and observed melt-rates (averaged ever y few (4?) hours) through time would give a better indication of the diurnal melt-cycle and the differences between the modelled and observed ice melt. In its current form, Fig 6 does confirm additionally the excellence of the model as revealed in Fig 5 but perhaps hides some of the more interesting differences/similarities at shorter timescales. For example, the sonic sensor suggests that the surface rises (accumulates) on many nights, a feature not replicated by the model. Is this 'accumulation' real (water vapour condensation and refreezing (on clear nights?)) or simply an issue to do with the sonic sensor. Either way, it's a really interesting observation.

A: In our opinion the measured rising of the surface during the night (by 1 cm) is not real, but reflects the uncertainty of the measurement. It may be a temperature effect. The reason for showing this plot is that there is potential in the model also to model sub-daily melt rate variations, but we consider a more detailed study of these time scales outside the scope of the paper. It would also require more detailed observations.

Q: P722, section 4.4. 1) I am slightly confused by the refreezing procedure and what actually happens in your model to the meltwater which refreezes. At S6 for example, "refreezing in snow consumes about 10% of the melt energy" (P712, 13 and P722). I presume this refrozen meltwater is melted again leading to its subsequent removal? At S9, you state that "refreezing consumes about 1/3 of the total melt energy" (P722, 23-24). Again, I presume that the meltwater is generated, refrozen and melted a second time thereby resulting in removal of the whole snowpack with no annual accumulation from refreezing. A couple of lines (or extra words) just to clarify exactly what is happening here would help in section 4.4 and in the caption for Fig. 8.

A: First, we moved the sentence 'How much meltwater in total refreezes in the firn on the GrIS is still a matter of debate, as it depends strongly on the melt model used (Pfeffer and others, 1991; Janssens and Huybrechts, 2001; Bougamont and others, 2007)' to the introduction. Second we clarified how the model treats meltwater by partly rewriting the section.

Q: P722, section 4.4. 2) I would like clarification on the internal ice melt procedure in the model. At S5, about 23% of all ice melt occurs below the surface. Down to what sort of depths does the model suggest this melt is occurring? The volume of melt is equivalent to an integrated ˜1000 kg m-2 a-1. If real, this amount of ice melt has the potential to considerably reduce the density of the subsurface ice with obvious implications for your assumed ice density and thus the runoff volumes as determined from your sonic height ranger measurements. I am no doubt missing something obvious here but clarification in the text would help. The same issue is clearly also relevant to S6.

A: We added a new section 4.3 ’Effect of radiation penetration’ and new figure 8 to clarify the effect of radiation penetration, which typically melts the ice down to a depth of 0.5 m. In this new section we also explain why a reduced ice density below the surface does not accelerate the surface lowering.

Q: Introduction. Given the importance of refreezing on the mass balance of the GrIS and the relevance to this paper, it would seem appropriate to reference the seminal paper by Pfeffer in 1991 that highlighted this issue.

A: Has been added.

Q: P713, 12-25. When were the AWS established? - worth including in the summary.

A: Done.

Q: P714, 3. "field area ON 23 August;." A: Changed.

Q: P718, 9. "all value OF Ts;." A: Changed.

Q: P718, 17. delete extra "the" A: Done.

Q: Fig. 4. Can colours be reversed to keep consistent with Figs 3 and 5. Q: Done.

Answers to comments by Jason Box

Jason Box sent an annotated pdf. Most of his recommendations were followed, some clarifications are listed below.

Q: Equation 1: How important is the change in stored heat? How important is heat from precipitation? Are these negligible?

A: For the skin layer approach stored heat is zero. Heat from rain/snow cannot be determined with any certainty, but is usually neglected as precipitation has nearly the same temperature as the surface under cloudy conditions.

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Interactive Comment

Q: 715, l. 19: please use ’interface’ instead of ’skin’

A: We prefer to retain the terminology ’skin ’layer’ as this is the commonly used phrase in literature.

Q: 718, l. 11: measured upward longwave has a reflected component, that is, L up = L emitted + 1-emissivity * L down. Do you not consider this reflected infrared energy?

A: Yes this is taken into account in the model but as we do not have information about the emissivity of the surface, we assume it to be unity so that the reflected component becomes zero.

Q: 723, l. 17: but isn’t that energy released into surroundings on re-freeze, thus the energy is conserved, right?

A: Yes, that is right.

Interactive comment on The Cryosphere Discuss., 2, 711, 2008.