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The Cryosphere Discussions

Interactive comment on "Surface mass budget and meltwater discharge from the Kangerlussuaq sector of the Greenland ice sheet during record-warm year 2010" *by* D. van As et al.

D. van As et al.

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DVA: Thank you very much for the time and effort you invested in the review. Below you will find the replies to all your comments. But first of all, based on your comments and the comments from other reviewers, I believe that it is important that we describe better what the aim of the paper is, how this is done, and how it adds to previous studies by Tedesco et al, Van den Broeke et al, and Box et al (Arctic Report Card 2010). This should be done in an extra paragraph in the introduction, and by adding to the methods section (better model description). In the introduction we shall point out that our study:

- Is the only one that calculates the SEB and SMB for the entire Kangerlussuaq catch-

ment based on on-ice in situ observations. The MAR model has proven to be very valuable in studying the Greenland surface climate, but cannot compete in accuracy with SEB and SMB observed/calculated/validated at AWS. The Van den Broeke et al study uses a SEB model that is as sophisticated as the one we use, but only shows results for the AWS location.

- Is the only one that calculates basin-wide runoff for these years. Because of the large amount of on-ice observations used, the high density of AWS measurements in the region, (importantly) the usage of MODIS albedo instead of assumptions or extrapolations, and the three-fold validation, this study is more accurate than previous similar studies calculating basin-wide runoff in Greenland. There is a discussion about the accuracy of the size of the catchment that we use, but in the new version of the manuscript we will argue that ArcGIS-determined basins cannot be considered accurate, that using surface topography is not a good way to find catchment size, but that sub-glacial topography is not well known, and that results for runoff are not very sensitive to the exact size of the catchment since melt decreases while watershed accuracy decreases with elevation.

- Is the only one to show and discuss temperatures for the town of Kangerlussuaq.

- Is the only one to include the entire year of 2010. The other studies only include results up to August or September.

MP: van As et al. (2011) combine two unique data sets of runoff and energy balance for the Kangerlussuaq sector of the GIS. This offers the potential for a watershed scale comparison of meltwater input versus output. For this reason the data set and the approach of the study is of considerable potential value. This value is not realized in the current draft. I found that the study just did not portray the data in nearly as quantitatively a detailed fashion for albedo, surface energy balance (SEB) or ablation as the van den Broeke et al (2011) paper. I constantly had to refer to the former paper and continually noted the superior documentation of the aforementioned processes for

the same transect, though not the identical period or purpose. We are left with no clear spatial view of the difference in the 2010 SEB versus other years. There is no clear spatial view of the ablation for the 2010 melt season in the watershed.

DVA: The Van den Broeke et al (VDB) paper is indeed a very good paper, I agree. It looks into the details of the surface energy budgets at the three IMAU weather stations in the K-transect. Our study was designed to do even more by making observation-based calculations of the surface energy and mass budgets and surface meltwater runoff for the entire drainage basin, using a model of equal complexity. For this reason we interpolated weather station and MODIS albedo measurement to elevation bins covering the entire basin. Figure 6 shows just that: spatial variability in the surface energy budget. It is my impression that you did not interpret this figure to give a spatial view, based on comments below. I am willing to change the figure to give a better spatial view. You mention that you'd like to see a map. Since our results are calculated per elevation bin, adding the across-slope dimension to figures would not add information though, as also stated below. Feel free to indicate how to improve the spatial view of e.g. Fig. 6. However, in the updated manuscript version we will include a figure that illustrates catchment area, runoff, area cumulative with elevation, and cumulative runoff to add to discussion of spatial variability. Finally, in reply to your comment on the VDB study/figures: As we do not study the SEB and SMB at the sites of the weather stations alone (which has been done very well by VDB), we cannot have figures identical to the VDB study. We focus more on spatial variability, so we have figures showing just this. Below you comment on this again, at which point I will reply in more detail.

MP: Given the connection to the meltwater drainage system, we also need a schematic or map that will better define in a qualitative sense at least this system. The paper should should depict the first two components in a map based format.

DVA: It seems that you would like to see a (schematic?) map of the drainage system. Do you mean of the surface meltwater streams and/or the en-/subglacial conduits? Based on the comments by Ian Bartholomew we have decided to reduce the discussion

of the meltwater routing. Even though it is a topic that is tempting to discuss since we have calculation of meltwater production on the one side of the glacial conduit system, and discharge on the other end, I believe that our data in its current form cannot add to the papers already published on the topic. So instead of going into more detail regarding the drainage system, I chose to reduce the discussion on this topic, somewhat shortening the section dealing with runoff. However, I will add lines representing watersheds to figure 1 in the revised version of the manuscript, which may be what you are after. I would like to emphasize again that we calculate values in elevation bins, i.e. we have one-dimensional results which are the average values valid for the entire width of the basin, i.e. applicable to two dimensions. These one-dimensional results are best plotted in line figures. I could easily produce a contour map with e.g. ablation for the catchment area, but the spatial variability it would show would not add to a line plot. Such line plots are in the current version of the manuscript, giving a spatial view of variability of several parameters.

MP: This paper can be a valuable contribution, but at present it simply is a step backward from the earlier paper by the same research group.

DVA: Again, please notice that we do not show the same results as VDB et al do. We emphasize on spatial variability, which, I am afraid, you may not have fully understood: You ask for which station the results in Fig. 6 are, while it shows results for along the entire elevation domain, as you can tell from the horizontal axis and the text. Concerning the statement about 'the same research group': The VDB paper is written by authors from IMAU in The Netherlands. Our paper is written by authors from two Danish research institutions, a UK university, and - yes - 5th author Michiel van den Broeke who has contributed to this manuscript by providing K-transect data and assisting in the writing. This is not the same research group. Could you please clarify your statement?

MP: 2323-5: Why is albedo derived strictly derived from MODIS not compared to, verified by the stations themselves used as described by van den Broeke et al (2011: 378) and portrayed in Figure 2.

DVA: This remark is a bit unclear to me. I will do my best to interpret and answer it. The albedo is not strictly derived from MODIS, but has been compared with and calibrated to the AWS measurements. This is in the paper, described in the methods section. Fig. 2 in the VDB paper shows an example of albedo change over the 2004 summer for one weather station in the K-transect. The figure is clear and explains in detail how snow pack evolution impacts surface albedo. The figure is placed in the methods section of the paper and intended to provide the reader with background information. In our manuscript there is also a figure (4) illustrating albedo, but measured by MODIS (calibrated by weather station observations) to be able to provide catchment wide albedos, not for points on the ice sheet. The figure shows measurements for four different elevations and during the whole course of the melt season, based on a decade of MODIS imagery. Also, the figure illustrates the 2010 albedo anomaly, its spatial and temporal variability, relative to the mean shown above. I find spatial and temporal variability of albedo clearly illustrated in the figure and I find the figure informative, but I am open to suggestions for improvement.

MP: 2330-3: Figure 5 is not nearly as informative in terms of the surface height changes largely resulting from ablation as Figure 4 in van den Broeke (2011). What is the specific ablation differences at the stakes from 2009 to 2010?

DVA: We think that Figure 5 holds more information than Figure 4 in the VDB paper. Although we only show data from 2 years, which are the years we are focusing on in this paper, we show measurements and model results from 6 AWS/elevations on the ice sheet, as opposed to the 2 sites shown by VDB. For the rest, the figure is virtually identical. Because we show results for a shorter period, and from stations that are positioned closer to each other than S5 and S6 in the VDB figure, the time series are more similar and thus the lines positioned closer to each other in our figure. It is unavoidable that our figure is less clear since it shows more data, but that does not make it less informative or unclear in my opinion. On the topic of the stake measurements: the continuous surface height measurements by sonic rangers and pressure

transducer assembly are in my opinion a better tool to validate model calculations of ablation/accumulation and temporal variability therein. Besides, I have concluded that the stakes at the 3 GEUS stations occasionally experienced melt-in during the summer, so I did not find the stake measurements sufficiently reliable to include these in the figure or validation.

MP: 2330-10: The meltwater drainage network is an important ingredient in this paper. A schematic figure or a map based figure of at least a portion of the watershed is essential.

DVA: This point was also brought up by other reviewers, and I agree. Figure 1 in the revised version of the manuscript will show the watersheds.

MP: 2331-8: The energy balance difference is not as clearly portrayed as is needed. The anomaly versus 2009 and other years at each station is just not clearly conveyed. Table 2 in van den Broeke (2011) is an example of how clearly the annual average energy balance has been quantified at S5, S6 and S9. What are these values for 2010?

DVA: The aims of the VDB paper are not the same as the aims of our paper. We interpolated the variables needed for the energy budget calculations over all elevations of the model domain in order to be able to show the surface energy and mass budgets for all elevation, not just for the weather station sites. So our study presents results for a large area / the whole catchment, while the VDB study presents results for 3 points. There is need for calculating the SEB and SMB at the weather station sites since this was done expertly by VDB. Again, we calculate all results in elevation bins as the meteorological variables are (assumed to be) showing little across-slope variability. This allows us to average the MODIS albedo pixels into the same bins to reduce the uncertainty. So we end up with a SEB and SMB that is valid for the entire catchment, but only varies with surface elevation, as is clearly shown in figure 6. We can add a table that shows the numbers plotted for all elevation bins in figure 6, but that would not

add information to the paper.

MP: 2331-22: The spatial variation of ablation is not well conveyed. A map or figure of the modeled ablation along the K-transect is needed, not just at the stakes, since the whole basin is being modeled. Of course this also suggests that the model results must be better compared to the observed ablation at the stakes themselves.

DVA: (You refer to stakes where you should be mentioning automatic weather stations.) Model results of ablation are validated in figure 5. I could make a plot showing the surface height change due to ablation/accumulation versus elevation (I discussed above why a map would not be better than a line plot in this study), for instance for 2009 and 2010, and then insert the surface height change measured at the AWS (/stakes) over the hydrological year as six points. This would not add to the validation, which is shown in figure 5, but it will give surface height change versus elevation. However, the two lines for 2009 and 2010 will be similar to the lines for melt energy in figure 6, while the absolute values of ablation can already be read from figure 5 for the AWS elevations. I believe that such an ablation map / line plot would not provide sufficient additional information to justify inclusion in the manuscript.

MP: 2332-4: As noted above document the energy balance changes for more than just and for each station, not just a general statement that ablation was due to a more negative long wave radiation balance. Figure 6 attempts to do this but just compares two years and for what location?

DVA: I am afraid you may have misinterpreted the figure. We presented the SEB in 2009 and 2010 for all elevation bins in figure 6. The figure does not show results for one location. This is visible from the horizontal axis label, and can be understood from the text that describes the figure. And I should point out that the statement that you refer to is not 'general', and it does not state that ablation was due to a more negative longwave radiation budget.

MP: 2332-1: The conclusion that albedo was more important than temperature to the

higher melt rate at the upper stakes, can be much better illustrated with a figure like that of Figure 7 in van den Broeke (2011). How did the albedo vary in 2009 versus 2010 at Kan M and S6 and S9?

DVA: This information is in figure 6, the figure that you may have misinterpreted (see above). The figure compares SEB averages for the summer of 2009 and 2010, and it shows the spatial variability well. The figure for instance illustrates the difference in net shortwave radiation, caused by albedo.

MP: 2334-15: It is noted that many moulins transport the water from the surface quite close to the origin of that meltwater. Quantify this statement, the suggested figure may do so.

DVA: You would like us to give the distance between where surface meltwater is formed, and where it enters a moulin. This is very difficult to determine, highly variable over the entire catchment, and can be the topic of studies completely focused on glacial hydrology. Based on comments by I. Bartholomew we chose to remove part of the discussion on meltwater routing.

Interactive comment on The Cryosphere Discuss., 5, 2319, 2011.