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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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SM: This is an interesting climate study however, is it missing basic hydrological elements and issues.

DVA: Thank you for the thorough review. Below I will reply to all your comments. The manuscript has improved by taking your comments into consideration, for which we are grateful.

SM: Major issues: The authors have not included evaporation in their surface mass budget, but only melt and sublimation. This should be fixed through out the calculations, since evaporation plays a significant role in the ice sheet surface budget, approx-

imately the same level as sublimation.

DVA: The reviewer is right in that we did not mention evaporation. I apologize for the omission. Evaporation and condensation have always been part of the model calculations, so no recalculations are required. I made the necessary addition to the text in the methods section.

SM: The authors compare their simulated "runoff " (surface generated melt available for flow) against unpublished (in review) observed runoff from the Kangerlussuaq drainage basin outlet. Observed runoff is based on a catchment size of 9743 km2 (an unpublished study, a study in review by Hasholt et al. 2011 - normally studies in review are not used as references), and the simulated runoff is based on a catchment size of 12574 km2 – the estimated catchment area in this study is around 25% greater than the Hasholt et al. estimated area of 9743 km2. Based on two significant different catchments areas the authors get a simulated runoff value equal (within a small difference) to the observed runoff amount from the Kangerlussuaq catchment. Something seems wrong, because two different catchment sizes give the same amount of runoff from the Kangerlussuaq drainage area. Either the model is underestimating the physical processer, including runoff, by around 25%, or the observed runoff is unrealistic too high. It might highly be the last issue, since the model also has been tested against surface processes on the ice sheet surface with reasonable results.

DVA: We have spent quite a few words on the differences in catchment size in various studies in the manuscript because it is indeed a contributor to uncertainty in modeled surface meltwater runoff. We mentioned the Hasholt et al (2011) catchment size to indicate that there can be large differences. Differences arise from the different datasets that are used for basin delineation, and from the methods that are used. We have attempted to delineate the basins using several surface digital elevation models: J. Bamber's DEM, the IceSat DEM, the 30 m Aster (which fails at higher elevations with snow surfaces), and a DEM in which Aster and J. Bamber data were merged. However, the results from these DEMs were very different from each other. Although we have faith in the DEMs (though differences exist especially near the ice margin), we do not have faith in the basins we delineated from them. This is due to the ArcGIS flow direction tool that we used for delineation. This tool, like other tools as far as we now, calculates the flow direction from a certain grid point to be towards one of the surrounding grid points, thus only having 8 directions to choose from. This tool fails over a nearly flat terrain with a low aspect ratio as a flow line can be 360/8/2 = 22.5 degrees off consistently over a distance of hundreds of kilometers. We have tested this by reprojecting the DEMS and recalculating the basins. The basins before and after reprojection were very different, telling me that this tool should not be used for this purposes, and that studies that calculate basins in a similar fashion over Greenland may very well be very wrong. To make this clear in the manuscript, we have added a few sentences on this in the methods section. Unfortunately, we do not have other tools at our disposal for basin delineation. Besides, if other studies use the potentially inaccurate method mentioned above, we can argue that a careful delineation by hand provides results of similar accuracy or better. On top of this, since the meltwater runs over the ice sheet surface for only a relative short distance before is penetrates the ice and continues in or underneath the ice sheet, we can argue that bottom topography s also important for basin delineation. So even well functional tools will not provide the actual catchment area. A good way to delineate a basin based on both surface and bottom topography, we believe to be using I. Joughin's surface velocity fields from 2006, which will tell you into which outlet glacier the ice sheet converges. The basins delineated this way will thus hold information on surface and bottom topography - since ice flow is dependent on both. We chose not to use this method since velocities at higher elevations are too low for accurate flow direction determination. Also, Joughin 2006 data does not have full coverage of the Kangerlussuaq catchment. But you mention above that our results are not very sensitive to the size of the catchment because two different catchment sizes give comparable runoff totals. The cause of the insensitivity is that where the inaccuracy in the location of the watershed increases with elevation on the ice sheet, the surface melt decreases with altitude. So, the 25% different surface

area in drainage catchment that you mention is located virtually entirely in the accumulation zone, where little to no meltwater runs off. As a sensitivity test I reduced our catchment size, and investigated the impact on the amount of runoff. I assumed that below 1000 m our catchment is accurate, since this is relatively close to the ice margin (at S6, see Fig. 1). Above the 1000 m elevation bin, I reduced the surface area of the catchment; by 10% in the 1100 m bin, by 20% in the 1200 m bin, etc, representing the increasing inaccuracy with elevation. As a result, the catchment surface area was reduced by 80% - much more than the 25% you mention. However, total surface meltwater runoff for the entire catchment was reduced by a mere 4% for 2009, and 13% for 2010 (which had more melt at high elevations). I hope that this convinces you of the insensitivity of our results to catchment size. To convince the reader of the manuscript, I have included these test results. I have also added a new figure (Fig. 7) in the new version of the manuscript, which illustrates the runoff contribution per elevation bin, and that nearly all of the meltwater originates from the lower 20% of the catchment.

SM: Also, the Kangerlussuaq runoff time series by Hasholt et al. (in review) were only compared against few (four) ADCP point observations, and not against the full range of independent observations required for a statistically rigorous analysis. Since the Hasholt et al. 2011 reference still is in review, this study should probably not use the Hasholt et al. (in review) reference – the Hasholt et al. (in revierw) should at least be accepted or published (at the moment the paper is in review for Journal of Glaciology) before the reference is used for model validation.

DVA: Referring to papers in publication is common. I will update the reference if it gets accepted or published before potential publication of this manuscript, or follow the advice of the editor if he states otherwise. Since we only use part of their data series, and since we focus on the surface ice sheet processes, I will leave the discussion of the uncertainties in the Hasholt et al river discharge data series to Hasholt et al. Naturally I will list the uncertainties they report – a value without uncertainty is useless.

SM: The difference in specific runoff (l/s/km2) should be calculated and compared, so it is easy to compare surface generated melt with observed runoff. The observed runoff values seems unrealistic and way to high due to missing quality control against independent observations (for further see Hasholt et al, in review), and also >250% higher compared to previous published runoff values.

DVA: We already give a comparison of calculated ice sheet run off and river flux in Fig. 7 (old version). We also state the difference in annual totals between the two records and (in the new manuscript version) give correlation values. Subtracting the grey line from the black line in Fig 7 (i.e. "change in en- and proglacial meltwater storage") does not add to the manuscript and complicates the interpretation of the figure. I believe the first sentence of this comment may stem from you misinterpreting what the black line illustrates. It is the 'surface generated runoff' summed for the entire Kangerlussuaq catchment (in km3/day). This should be clear from the text. Also, the observed runoff at the Watson River may 'seem' unrealistic to you, but I can only state that Hasholt et al. used more observations than ever before for this location to calculate their values. I understand that their calculation methods have changed because until recently they lacked control points in the high-discharge end of their measurement spectrum. Now they have 4 control points, indeed increasing the estimates compared to previous studies, especially in high melt years such as 2010. Our results show that two third of those 250% you mention could be due to the extreme conditions in 2010. I also understand that the changed calculation methods will impact results presented previously by you, so I understand any skepticism you may have. However, I will not get involved in this discussion since I am a mere data user. All I can do is use Hasholt et al's best estimates, report the uncertainties, and compare to our runoff results – two completely independent methods. The agreement is good.

SM: A bedrock map should as minimum be used to estimate the watershed divide, and the routing of water from the surface and to the catchment outlet. Such a map can be found in Lewis and Smith (2009). Therefore the authors should re-calculate their catchment area based on this bedrock map by Lewis and Smith (2009). The map

can be downloaded online. This will require some work, but it will make the paper stronger, and provide the scientific society with more detailed information about estimating watershed divides and catchment areas for the Greenland Ice Sheet, instead of just hand-drawing the catchment area based on surface contours.

DVA: As we already mention in the manuscript, there are no detailed maps of subglacial topography. Current maps are very valuable to investigate large-scale bottom topography features, but are not based on enough measurements of ice thickness, nor have a fine enough grid spacing to be able to look into drainage basins that are fairly small/narrow, such as the Kangerlussuaq basin. Lewis and Smith do present drainage basins for the Greenland ice sheet, which is a great result. However, the study suffers from a few flaws in my opinion. Most importantly, they use the ArcGIS flow direction tool to delineate the surface meltwater drainage basins, which is not the right software to use as I described above and will clarify in the manuscript. I do not believe that the Lewis and Smith basins are accurate, and will therefore not use their results. I'd like to emphasize that the ArcGIS flow direction does not perform well in the interior of ice sheets and should not be used in these regions. You personally do not use bedrocks maps for meltwater runoff calculations either, so I am sure you can understand our reluctance to do so.

SM: The findings in this study should be added in a broader perspective and related to overall conditions for the ice sheet and Greenland in general. This will make the paper attractive, and useful.

DVA: The strength of the manuscript is that we take a detailed look into the forcings behind melt in a certain region of Greenland, based on observations, and validated by observations. There has never been a study calculating surface meltwater runoff for an area of an ice sheet that was this much reliant on observations, with the exception of the Morten Langer Andersen et al (2011) paper, in which they calculated melt for Helheim glacier using MODIS albedo and data from one weather station. I think it is worth it looking into the details as opposed to the larger picture, especially since the studies by Tedesco et al (2011) and Box et al (2011) (Arctic report card) have looked into the large scale perspective already. However, in the new manuscript version I will include in the introduction what this study adds to the mentioned papers, thereby providing some perspective.

SM: Overall chapter 3.5 is weak in is content. The authors are mainly not talking about runoff, but surface generated melt water at the snow and ice surface, available for the internal glacier flow system. The authors should put some effort in discussing and describing flow processes, lag time, and flow properties between surface generated melt and observed catchment runoff. It is important that the authors clearly state, that they are talking about ice sheet surface melt and catchment runoff. Basic elements about runoff is missing from this paper, therefore, this chapter should be re-written, and include basic hydrology and flow conditions, and a description of different storage properties and lag time properties.

DVA: These statements are somewhat similar to the comments posted by I. Bartholomew. We agree with both reviewers that the section was in need of improvement. Bartholomew was very specific in his comments, and we followed his advice in improving the section. Essentially, we removed any text with speculative statements, and in the new manuscript version we refrain from discussing anything except the basics of hydrology, since our results may not have the accuracy to justify and in-depth study. Our study is aimed at the surface energy and mass budget and surface meltwater runoff, not meltwater routing through the ice sheet. Even though the subject is related, justifying a to-the-point discussion, we feel (and so does Bartholomew) that an in-depth discussion should not be included in the paper. So as you suggested, in the new manuscript version we have rewritten the section, rephrasing our text to eliminate speculation and for improved clarity.

SM: A comprehensive motivation for this paper is missing – the motivation could be linked to the perspective, relating Kangerlussuaq to the overall Greenland ice Sheet conditions.

DVA: Agreed, the revised manuscript will include a clearer motivation, which involves the Greenland Analogue Project which looks into meltwater penetration into the bedrock, amongst other things. We won't make statements on how the Kangerlussuaq sector could be representable for the entire ice sheet, as this would be speculative at best. The last paragraph of the introduction will be changed into: "In this paper we investigate surface melt near Kangerlussuaq, southwest Greenland, using data of a dense network of automatic weather stations (AWS) that is operational in the area. In light of the extraordinary atmospheric conditions in Greenland in 2010, we quantify the 2010 temperature and MODIS-derived albedo anomalies for the Kangerlussuaq region. Subsequently, we calculate surface ablation and meltwater runoff, validate results using ablation and meltwater discharge measurements, and investigate the causes for the 2010 melt anomaly. This study provides freshwater availability for penetration into the bedrock, which is one of the main research aims of the Greenland Analogue Project (GAP)."

SM: Minor issues: 2320, L5: Use instead normal periods 1980-2009.

DVA: The more data that are used for statistical studies, the more significant results are, especially when dealing with standard deviations. I know that it is common to use standard climatological periods of 30 years, but this is mostly necessary when comparing different data series, such as measurements from different locations. I would like to argue that using standard climatological periods is not necessary for single data series. Besides, why not use the climatological period that was commonly used until recently, i.e. 1970-2000? Box et al. (2011) (Arctic report card) use 1971-2000.

SM: 2320, L11: Can you estimate 166% precisely, when you take into account the uncertainty. More appropriate would be 160%, or 150%.

DVA: Agreed, I changed 166% into ∼170% throughout the manuscript. Similarly, I changed the 145(.3)% runoff increase values into ∼150% throughout the manuscript.

SM: 2320, L11: How do you know that 2009 is a "normal" year, if you only have observed runoff values since 2007? Please, clarify.

DVA: We added here that we use 2009 as a reference year based on atmospheric temperature measurements.

SM: 2320, L15: What is good agreement - please explain with values. This is more appropriate, that just words.

DVA: I agree, and added: "(r=0.79)". I also added correlation coefficients at similar statements in the manuscript.

SM: 2320, L15: Reference is missing.

DVA: We aim not to have references in the abstract unless the editor indicates differently.

SM: 2321, L9: Use a never reference, e.g., Hanna references. This is a 10-yr old reference, and stuff has changed since then.

DVA: When referring to the observed temperatures in the 1980s, a reference to a 2002 paper dealing with Greenland instrumental records will never get outdated. I don't know what you mean with 'stuff has changed since then' - the temperature records from the 1980s have not changed since 2002.

SM: 2321, L13: Instead of pers comm. use DMI technical reports from Cappelen instead. These can be found at www.dmi.dk

DVA: Good point, thanks. When we started writing the paper the report including 2010 data had not been published yet - I have updated the manuscript with the suggested reference.

SM: 2321, L16: Also, by Box et al. 2010.

DVA: Correct, Box et al: Greenland, state of the climate 2010, does mention this as well as they summarize results found dealing with the climate in 2010. I believe that 3

references here are sufficient though.

SM: 2321, L19: Causes have already bee giving in Box et al. (2010), ARC report. Therefore, this point is not of interest for the reader any more.

DVA: Causes of what and which point do you mean? Box et al (2010) does list observations of the extreme year 2010, but does not go into detail. No other publication looks with this much detail in the causes of extreme melt in the entire Kangerlussuaq catchment area. This paper goes into more detail than that of Tedesco et al (2011), and adds to the Van den Broeke et al (2011) paper by using more station data, using MODIS data, looking into SEB and SMB changes with elevation (not just at 3 locations), and presenting validated catchment-wide runoff totals. I don't know what is not of interest to the reader, especially when compared to the interesting but limited information given by Box et al (2011).

SM: 2322, L6: Not only surface melt, but also surface accumulation, therefore, use the word 'surface processes', instead.

DVA: We have changed it into 'surface mass budget', following your suggestion, but sticking to the terminology used throughout the manuscript.

SM: 2322, L12: Please add reference due to the location of ELA, e.g., see van de Wal papers.

DVA: Agreed; I have added a reference to Van de Wal et al. (2005).

SM: 2322, L16: How low, add numbers?

DVA: Since there are 8 AWS and 6 measured variables that we can provide correlation coefficients for, I will limit myself to giving examples and add: "(e.g. r = 0.75 and 0.06 between KAN_U and Saddle for daily means of wind speed and relative humidity, respectively)".

SM: 2323, L2: Which are these, illustrate and explain why the other methods not were appropriate.

DVA: We tested interpolation by linear fit to the 6 AWS (which is the method that was used), by linear fit with temporally constant slope to the 6 AWS, and by linear interpolation between the AWS. The first of these options was found to be most suitable. If the editor wishes we can include a description of the interpolation methods, but this may make the methods section more tedious, in the words of one of the other reviewers. Instead, I chose to shorten the paragraph, not mentioning the other interpolation methods to avoid confusion: “For model input data we spatially interpolate daily-mean AWS observations into 100 m elevation bins to be able to determine the distributed melt patterns in the region. The horizontal distances between the six AWS are 8 to 54 km, increasing with decreasing surface slope. A linear least-squares fit to all AWS measurements for each time step was utilized since this method allows reliable extrapolation outside the vertical domain with weather station observations.”

SM: 2323, L18: An uncertainty of 20% seems unrealistic due to uncertainties in measuring e.g., the cross section profile. For example a study by Rennermalm et al. (2011) confirms that changing bed elevations over time, a phenomenon observed at several Watson River tributaries upstream of Kangerlussuaq, are associated with discharge uncertainties of up to 47%. Therefore, there is absolutely no reason to expect that 20% uncertainty from the Hasholt et al. study are realistic, simply because the outlet cross section from the Kangerlussuaq drainage basin is highly influenced by depositing and erosion of sediment at the river bottom. Further, observed runoff by Hasholt et al. are within 125% uncertain due to independent Acoustic Doppler Current Profiler (ADCP) observations (Figure 4 in their submitted paper for Journal of Glaciology).

DVA: This discussion fits better in the review of Hasholt et al. (in press), which I believe you may be involved in given the amount of detail you provide. By referring to their paper in our manuscript, the reader can look into how Hasholt et al. found the 20% uncertainty. I will update our manuscript as soon as Hasholt et al. gets published or if they provide me with new numbers. Concerning the sediment erosion/deposition in the

channel the discussion seems interesting, and I am eager to hear the outcome. You report a high influence, but Mernild et al (2009) state: “This outlet is one of the best locations for observing GrIS runoff because of the well-defined, stable bedrock cross sections.”

SM: 2323, L19: The authors can not use a reference which has not been published. This paper is still in review.

DVA: See my reply to your earlier comment on this topic.

SM: 2323, L29: What are the expected uncertainties, due to this method.

DVA: In the original manuscript we had already included a discussion of uncertainties, also including catchment size. This is more than is done in some studies that simply state what “is” the catchment size. For instance (I’m not saying this because it refers to one of your papers, but because it cover the same topic as our manuscript), Mernild et al. (2010) state “The Kangerlussuaq drainage area (6130 km2) is located on [. . .]” and never discussed inaccuracies or the method that was used to determine the area. We believe that some discussion of the catchment area is justified, and therefore had already included it in the original manuscript. But at your request, and as discussed above, I have added to this section by discussing the reasons why the ArcGIS flow direction tool should not be used. I also state in the new manuscript version that: “we determined the drainage basin boundaries by hand from our digital elevation model of the ice sheet, which can be done within 10° of the surface slope direction”.

SM: 2324, L13: Please, explain the model you use, so the reader right away can get an impression of weaknesses and forces of the model.

DVA: The model was described in this section of the original manuscript. The description was fairly short, and a reference was given to Van As (2001), who describes the model in more detail. In the new manuscript version the description will reveal more detail so the reader can better determine the strengths of the model without having to read Van As (2011).

SM: 2324, L19: From the surface mass budget evaporation is missing, which is approximately in the same order as sublimation. Please include evaporation in your calculations so you are able to close the surface mass budget.

DVA: Evaporation is in the model calculations, as mentioned above. Sublimation/deposition changes into evaporation/condensation when surface temperatures are calculated to be 0 C. We have added this to the text and apologize for the initial omission.

SM: 2324, L24: What are these requirements? Explain. Does the snow model include retention?

DVA: The original manuscript lists these requirements: "i.e. when sub-surface grid cells are at sub-freezing temperatures and not at ice density". Retention is not included.

SM: 2325, L13: Also, should be mentioned, that simulations were done by mean daily values, and not by hourly values. This creates probably a higher degree of uncertainty than the ones which are listed here. Please provide the reader with uncertainty estimates due to the use of daily mean and hourly input values.

DVA: Ok, this will be added to the new version of the manuscript: "Using a daily time step in our model calculations instead of a temporal resolution resolving the daily cycle is justified given the ablation validation for the AWS positions shown below. It also ensures a more accurate spatial interpolation since local variability on short time scales is averaged out." and: "The causes of uncertainty in this study are not exceptional and allow for relatively accurate surface energy and mass budget calculations."

SM: 2325, L18: How close is this agreement.

DVA: This was/is mentioned in L20: "We found root mean square difference (RMSD) values of 1.0-1.7 °C for the six stations and their corresponding elevation bins, which is 4-6 times smaller than the uncertainty derived from the 10% uncertainty statement

by the radiometer manufacturer. This testifies for accurately modeled surface temperatures, as well as for more accurate radiometer readings than specified by the manufacturer."

SM: 2325, L27: Use instead normal period 1980-2009.

DVA: Please see my reply to your first remark on this.

SM: 2326, L8: Add a reference.

DVA: I added a reference to Box et al. (2011). This section will be further rewritten though, so this sentence may be modified as well.

SM: 2326, L8: Again, use the normal period 1980-2009.

DVA: Please see my reply to your first remark on this.

SM: 2326, L14: This has already been described and discussed in Box et al. 2010, ARC report. Add reference to Box.

DVA: The arctic report card does not give temperatures or statistics for Kangerlussuaq, and does not gives values for the second part of 2010 (Sep-Dec). However, there is sufficient overlap to justify the inclusion of a reference, so I added: "as also determined for other west and south Greenland locations (Box et al., 2011)". This section will be further rewritten though, so this sentence may be modified as well.

SM: 2326, L6: Add references for snow and ice albedo.

DVA: This is textbook material.

SM: 2328, L12: Add a figure showing the spatial distribution of MODIS satellite derived albedo. That will help the reader to understand the distribution.

DVA: Figure 4 already gives the spatial and temporal variability to MODIS-derived surface albedo. A map-like figure showing either 2D spatial variability on one specific date, or averaged over a number of dates, would not add much and would not show interesting results (“albedo increases with elevation”). I hope you agree with this assessment.

SM: 2328, L26: Add reference to this function.

DVA: In an effort to shorten the first sections of the results section, as suggested by Anonymous Referee 2, I have removed this paragraph.

SM: 2329, L5: This has already been discussed in Box et al. (2010), ARC report. There should be a reference to Bow et al (2010) here.

DVA: There is already a reference to Tedesco et al. (2011), which is equally suitable.

SM: 2329, L22: Add a reference due to the spatial distribution of snow accumulation, e.g., Ettema et al. 2009.

DVA: I added a reference to Burgess et al. (2010).

SM: 2330, L11: How much larger is MODIS estimated ablation? Please provide the reader with values, so the reader can judge how go/bad the results are.

DVA: You mean ablation calculated with uncalibrated MODIS albedo values? The readers can judge that from the figure. In addition, we now state: “and exceeds the result with calibrated MODIS albedo by 14% at the lowest station and increasingly so with elevation”.

SM: 2330, L24: It’s not reliable, to compare 2009 values with values from 2004-2007. Values from 2009 and 2010 should be compared to observed values from 2009 and 2010, due to the annual variability from year to year.

DVA: In the text we state that the ablation that we measured and calculated is a common value as documented by Van den Broeke et al. (2008). We need to make reference to previous results in the area to offer perspective. We are not using results from other ablation studies to validate our results.

SM: 2331, L3: The paper by Mernild et al. (2010), used not only data from DMI, but

also data from S5, S6, and S9. The statement by the author is incorrect.

DVA: We state that the study is largely based on DMI data, not fully. The Mernild et al study covers a 30-year period. The K-transect weather stations are operational since 2003, of which Mernild et al used three years, and only three variables (temperature, humidity and wind speed). I must argue that the statement is true.

SM: 2331, L26: What is the impact on surface runoff generated below the snowpack, after melt water has penetrated through the snow? Are there any significant difference between 2009 and 2010?

DVA: I don’t understand you comment regarding surface runoff generated below the snowpack. There is no meltwater generation at the snow/ice interface. Melt is produced at the surface, penetrates through a snow layer (if present) where it may refreeze if there is sufficient cold content and volume. This is clear from the methods section. Or would you like to know the roll of snow in the difference between 2009 and 2010? I can guarantee you that the impact on basin-wide runoff or the SEB is minimal, since snow is not abundant in the ablation zone.

SM: 2332, L28: Since, the difference in catchment area between this study (app. 13000 km2) and the Hasholt et al (2011) study (app 9700 km2), and that 2010 melt was most extreme in the higher regions, it is in even more obvious that either the model is predicting to low runoff values, or the observed runoff observed at the catchment outlet is way to big. Simply because the catchment divide is difficult to estimate where the ice sheet surface is less sloped, which is in the higher regions.

DVA: I have replied to this in the beginning of this list of comments. In short: nearly all surface meltwater is produced in the lower 20% of the catchment, which we now show in the extra figure. As a test, I linearly reduced the area of the elevation bins in our catchment from 100% to 0% of their original size between 1000 and 2000 m altitude. The resulting catchment was 80% smaller, but still generated 96 and 87% of the meltwater in 2009 and 2010, respectively. In conclusion: the result is insensitive even to extreme errors in (upper) catchment size. If you are still convinced that both our model results, and the measurements at the bridge are giving 'way too' large values, then I can at least eliminate the possibility of having the wrong catchment size as a cause of the prior.

SM: 2333, L5: Are the authors talking about surface runoff, or outlet runoff? This is unclear. It should be clearly stated.

DVA: I'm not sure what you mean by "outlet runoff". I'm guessing that what we call "surface meltwater runoff" is what you call "surface runoff". To clarify, I changed the sentence into: "The meltwater runoff from the ice sheet surface equals cumulative ablation reduced by losses from sublimation and evaporation and the meltwater that refreezes in snow and firn."

SM: 2333, L7: What kind of routing scheme are the authors using to simulate the outlet runoff hydrograph. This is unclear. Also if they use any lag time between surface melt, and runoff at the catchment outlet.

DVA: In this paragraph we only discuss the surface meltwater runoff; we don't compare to measured river discharge. Maybe the confusion originates from your previous remark? In this case I hope that by changing the first sentence I avoided confusion in the rest of the paragraph. Also, please note that we never discussed a routing scheme as this is not part of the study. We only present surface meltwater on the ice sheet, and measured discharge in the proglacial river. Any time lag between the two data series is because the meltwater has to pass through the ice sheet and a section of the river. This is clear from the discussion in this section of the manuscript.

SM: 2333, L14: Is this a general issue, that albedo can be unrealistic. Why this day? And why not others days?

DVA: The MODIS albedo value during any day will be associated with a measurement error. Most days this error is small, and some days it is large, such as during the day we

highlight in the text. The careful validation of our results earlier in the manuscript gives confidence that the model performs well over the course of the entire melt season. There will always be spikes in observation-based studies, and we should discuss them (as we did).

SM: 2333, L21: Reference is mission.

DVA: This sentence has been removed following a comment by I. Bartholomew.

SM: 2333, L24-25: It is unclear, if the observed discharge only is estimated based on water level measurements. Actually it should be based on Q/h-relations. It is unclear how observed runoff was measured. There should be a more detailed description of the runoff observations, since observed runoff is used to validate the surface generated melt. It is important to state that no internal glacier flow and lag time processes have been added to the simulations, but the "runoff " only is influenced by the direct impact from the climate, and not changes in internal drainage system, or internal storage.

DVA: Our manuscript does not present new measurements of discharge in the Watson River, but uses values from Hasholt et al., so we leave the detailed description to the Hasholt et al. paper. In the methods section, it is mentioned that "River depth and flow velocity data were gathered at Watson River bridge in Kangerlussuaq and were converted into freshwater flux" (old manuscript version: "River depth and flow velocity data gathered at the bridge in the town of Kangerlussuaq were converted into freshwater flux"). The last sentence of the paragraph (cause of the confusion) is changed into: "The freshwater discharge as measured at the bridge over Watson River in Kangerlussuaq is also illustrated in Fig. 8." Concerning the lag time: It is clear from the manuscript that we compare runoff from the ice sheet surface with discharge in the river. Our terminology is consistent throughout the manuscript. We introduced no lag time, which should also be clear from the discussion of the lag time and how it changes during the course of the melt season (discussion in new manuscript version is shortened based on I. Bartholomew's comments).

SM: 2334, L7: One of the main problems are that observed runoff is generated based on a catchment area of app. 9700 km, and simulated runoff of app. 12600 km2. Based on two significant different catchment areas, the authors get simulated surface melt water available for runoff, which is in the same order as observed outlet runoff. One would expect the same amount of runoff if the catchment areas had the same size. Not, when they have different sizes. The authors need to explain this difference, since observed runoff values were used for model validation. If the authors compare specific simulated and observed runoff values, they would see that the model is underestimating the Kangerlussuaq runoff by 25-35%.

DVA: It is unclear to me what you mean by 'specific simulated runoff values'. My best guess is that this is the surface meltwater produced at the ice sheet surface, which runs off and collects in the proglacial Watson River. This is exactly what we calculate, which is clear from the text (also based on the other reviewers' comments, or the lack thereof). If not, please give specific suggestions. As you can read in the manuscript, we don't conclude that the model underestimates runoff. There is a slight overestimation when compared to the river discharge measurements, which is the opposite of what you expect, apparently. The unexpected match cannot be undermined by statements of inaccurate catchment size, as I showed you in reply to earlier comments that the total amount of surface meltwater runoff in insensitive to catchment size. So - even if there is a difference in catchment sizes between 2 calculations, the runoff will be nearly identical.

SM: 2334, L11: Add a reference, due to the expected low values for sinks and sources. I agree with the authors, that these minor uncertainties are not the reasons, for the model to underestimating runoff.

DVA: Note that the model produces larger runoff values than those measured at the bridge, and thus not underestimate runoff. I am not aware of a paper that discusses all sinks and sources of water in the proglacial tundra, especially those linked to groundwater. However, in the revised version of the manuscript this sentence is removed, so

no reference is required.

SM: 2334, L16: Add a reference.

DVA: I changed the sentence into: "Most importantly though, basal topography is a first order control on subglacial meltwater routing, as a dense network of moulins across the ablation zone captures local melt water (Bartholomew et al., 2011)."

SM: 2334, L17-18: This statement incorrect. The authors should have a closer look at Lewis and Smith (2009).

DVA: Agreed. I have refined the statement: "In this and similar studies, the surface topography alone is used for catchment delineation given the lack of a detailed knowledge of basal topography."

SM: 2334, L20: The authors need to expand Figure 1, and include the entire Kangerlussuaq watershed and watershed divides. They should also include the estimated catchment area by Hasholt et al (2011). This will probably clarify why the simulated specific runoff is lower that observed specific runoff, and vise versa.

DVA: Agreed, I have included the catchment outline in the figure. I don't see why I should include the Hasholt et al. (2011) basin. We refer to the paper so the reader can get the details of the discharge calculation, and to give an example of how much catchment sizes can differ. Since we don't base conclusions on the Hasholt et al. catchment, it is not necessary to include.

SM: 2334, L20: What are the expected uncertainties due to the hand-drawn watershed divide?

DVA: We have discussed this in the methods section, thus included: "(see methods section)".

SM: 2334, L23: A reference is missing due to the poorly performed software, stated by the authors.

DVA: In the revised manuscript we argue in more detail why standard flow direction tools are not suitable. It is a conclusion we came to ourselves and becomes apparent if one compares the same basin delineated from the same DEM in different projections. I am not aware of any references dealing with the inaccuracy of the ArcGIS flow direction tool applied to the Greenland ice sheet that I can include. Please contact me if you know one.

SM: 2334, L28: Bedrock maps can be found in Lewis and Smith (2009). They used bedrock map to water routing through the ice sheet. The authors should as minimum use their maps as well.

DVA: Above I argued against using the Lewis and Smith (2009) catchment delineations since their input data and the delineation tools are unsuitable for this (and other) relatively small region(s). Most other studies (such as your own) use surface DEMs for catchment delineation, and so do we.

SM: 2335, L4: It is unclear what the authors are talking about here. Clarify, if it is observed runoff and simulated discharge?

DVA: We call these terms "modeled surface meltwater runoff" (ice sheet) and "observed discharge" (river). You are right that the sentence was in need of rewriting. In the new manuscript version it will read: "Close examination of the timing of the bulk surface meltwater runoff and Watson River discharge peaks reveals [. . .]"

SM: 2335, L11: Add a reference.

DVA: I added: "(Fig. 8)", which shows that meltwater availability falls in August.

SM: 2335, L13: Add a reference.

DVA: There is already a reference to Harper et al.

SM: 2335, L13: If the authors are trying to explain runoff from Kangerlussuaq, this is not out of the scope of this paper. Therefore, the authors should at least include a

discussion about these processes, and their uncertainties.

DVA: Here you request that we discuss "The link between meltwater production, basal pressure, and ice velocity". We do show meltwater production in the manuscript, but not basal pressure or ice velocity as these have no impact on our results and conclusions. Subglacial hydrology is a topic that is very well discussed in a number of recent high-impact papers / in-depth studies. When just comparing/validating surface meltwater runoff with proglacial river discharge a detailed discussion of basal water pressure is out of place, let alone its impact on ice velocity (or their uncertainties?).

SM: 2335, L14: What do you mean by agree – to what degree?

DVA: You mean page 2336. I added that the time series correlate by r=0.79, as I also mention in the revised abstract.

SM: 2337, L27: Papers not accepted for publication should not be used. In this study the model validation relay heavily on a data not accepted for publication.

DVA: Similar comments were listed and replied to above.

SM: 2339, L10: References like this should not be used. This is gray not peer-reviewed literature.

DVA: The Bulletin of GEUS is peer-reviewed and has an impact factor. You have been a reviewer for the bulletin yourself (Dawes and Van As, 2010). Besides, the paper holds relevant information, and references do not have to be limited to peer-reviewed papers.

SM: Table 1: What parameters are used? Add these to the table.

DVA: The used variables are identical for all AWS, and already listed in the methods section.

SM: Figure 1: Add the hand-drawn drainage basin and the drainage divide to the map. Also, the divide estimated by Hasholt et al 2011.

DVA: See my reply to your earlier comment on this.

SM: Figure 4: It would be great to have a figure showing abledo before and after calibration. This will helps the reader to understand how much wrong MODIS is estimating albedo before calibration.

DVA: We use MODIS albedo as a tool to obtain accurate meltwater values and we do not want the manuscript to focus on MODIS validation, and thus include a figure showing only this. However, I do fully agree that we need to quantify to what extent our albedo calibration improved MODIS accuracy. In the new manuscript version we will state (in the methods section) that MODIS vs AWS albedo has a RMSE of 0.114 without calibration, and 0.079 with calibration. This is appropriately mentioned right after we report RMSE values of 0.067 by Stroeve et al. (2006), who compared MODIS to GC-Net data.

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