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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.

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Review:

Surface mass budget and melt water discharge from the Kangerlussuaq sector of the Greenland ice sheet during record-warm year 2010, by van As et al. for TC.

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

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, approximately the same level as sublimation. - The authors compare their simulated "runoff" (surface generated melt available for flow) against unpublished (in review) observed runoff from the Kangerlussuaq drainage basin outlet. Observed runoff is based on a catchment size of 9743 km2 (an unpublished study, a study in review by Hasholt et al. 2011 - normally studies in review are not used as references), and the simulated runoff is based on a catchment size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. 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. 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. - 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

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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. - 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. - 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. - 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.

Minor issues: 2320, L5: Use instead normal periods 1980-2009. 2320, L11: Can you estimate 166% precisely, when you take into account the uncertainty. More appropriate would be 160%, or 150%. 2320, L11: How do you know that 2009 is a “normal” year, if you only have observed runoff values since 2007? Please, clarify. 2320, L15: What is good agreement - please explain with values. This is more appropriate, that just words. 2320, L15: Reference is missing. 2321, L9: Use a never reference, e.g., Hanna references. This is a 10-yr old reference, and stuff has changed since then. 2321, L13: Instead of pers comm. use DMI technical reports from Cappelen instead. These can be found at www.dmi.dk 2321, L16: Also, by Box et al. 2010. 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. 2322, L6: Not only surface melt, but also

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surface accumulation, therefore, use the word 'surface processes', instead. 2322, L12: Please add reference due to the location of ELA, e.g., see van de Wal papers. 2322, L16: How low, add numbers? 2323, L2: Which are these, illustrate and explain why the other methods not were appropriate. 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). 2323, L19: The authors can not use a reference which has not been published. This paper is still in review. 2323, L29: What are the expected uncertainties, due to this method. 2324, L13: Please, explain the model you use, so the reader right away can get an impression of weaknesses and forces of the model. 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. 2324, L24: What are these requirements? Explain. Does the snow model include retention? 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. 2325, L18: How close is this agreement. 2325, L27: Use instead normal period 1980-2009. 2326, L8: Add a reference. 2326, L8: Again, use the normal period 1980-2009. 2326, L14: This has already been described and discussed in Box et al. 2010, ARC report. Add reference to Box. 2326, L6: Add references for snow and ice albedo. 2328, L12: Add a figure showing the spatial

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distribution of MODIS satellite derived albedo. That will help the reader to understand the distribution. 2328, L26: Add reference to this function. 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. 2329, L22: Add a reference due to the spatial distribution of snow accumulation, e.g., Ettema et al. 2009. 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. 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. 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. 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? 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. 2333, L5: Are the authors talking about surface runoff, or outlet runoff? This is unclear. It should be clearly stated. 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. 2333, L14: Is this a general issue, that albedo can be unrealistic. Why this day? And why not others days? 2333, L21: Reference is mission. 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

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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. 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%. 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. 2334, L16: Add a reference. 2334, L17-18: This statement incorrect. The authors should have a closer look at Lewis and Smith (2009). 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. 2334, L20: What are the expected uncertainties due to the hand-drawn watershed divide? 2334, L23: A reference is missing due to the poorly performed software, stated by the authors. 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. 2335, L4: It is unclear what the authors are talking about here. Clarify, if it is observed runoff and simulated discharge? 2335, L11: Add a reference. 2335, L13: Add a reference. 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. 2335, L14: What do you mean by agree – to what degree? 2337, L27: Papers not accepted for publication should not be used. In this study the model validation relay

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heavily on a data not accepted for publication. 2339, L10: References like this should not be used. This is gray not peer-reviewed literature.

Table 1: What parameters are used? Add these to the table. Figure 1: Add the hand-drawn drainage basin and the drainage divide to the map. Also, the divide estimated by Hasholt et al 2011. 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.

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