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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: Many thanks for the time and effort invested in the review of the manuscript. Below I will reply to all your comments.

AR2: I am omitting any summary of the paper as this was done already by previous comments and referees. I fully agree with other comments that the paper hides interesting results that have not been fully explored or properly explained.

DVA: I will try to reply to the comments by the other 4 reviewers in detail, and to modify the manuscript based on their advice. I hope this will take away the concerns you have with regard to the manuscript as well.

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AR2: The runoff section is what makes this study interesting with respect to previous studies focusing on the 2010 melting record. The 2010 Arctic report card and other papers (mentioned in the study) already reported the analysis of the strong surface temperature anomalies in Greenland, extreme melting and the albedo feedback mechanisms. Those studies also use data from the K-transect and more sophisticated SEB models to address the causes of the 2010 melting record. Just focusing on a specific site can be potentially interesting if this really adds something new. In my opinion, half of the paper (as it is written now) does not add this information as it repeats what was previously mentioned in other studies. In this sense, the paper is more a report on a specific site and could be shortened to highlight local conditions at Kangerlussuaq.

DVA: Indeed, this is a study of the local conditions in the Kangerlussuaq catchment, in which we do not try to upscale results to be valid for the entire ice sheet, but to be accurate for just this basin. We do not make statements for the entire ice sheet. In presenting the SEB and SMB for this catchment we come to a few conclusions that others have come to for all of Greenland, or for single AWS sites. But we use different methods, such as a well-tested and sophisticated SEB model using only observations for input, such as MODIS albedo for all elevations in the catchment. We quantify how much meltwater ran off the ice sheet in the Kangerlussuaq region, how melt energy varied per elevation bin, and which energy fluxes contributed how much to the 2010 melt excess. This has not been done by other studies. But I do agree that certain sections can be shortened if they touch upon conclusions in previous studies. I will attempt to do so in the next manuscript version, focusing my efforts on the temperature section (although no one else quantified the measured 2010 temperature anomaly in this region, on ice or land) and the albedo section (although no one else quantified the 2010 albedo anomaly in the Kangerlussuaq region except for AWS sites). Also, we will add a figure that illustrates the meltwater contribution per elevation bin to add to the section of the manuscript that you find most interesting. In the introduction I will state more clearly that we investigate the Kangerlussuaq catchment only, and to which extent this study adds to previous studies. The model description needs to be extended

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to inform the reader of the level of sophistication of the model. You may also notice in the revised version that we rephrased a lot of the text for better reading. In addition, I will here include the replies to the comments by another reviewer (and more), as they are relevant to your comments as well: 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). We shall also put more emphasis on the surface energy budget by including these words in the title. In the introduction we shall point out that our study:

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

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

AR2: I would have liked to see a more detailed description of the model. In particular, justifying how and why some parameters were tuned in one way or another.

DVA: I can certainly add to the model description in the methods section. Even though the model is described in detail in Van As (2011), it is important to convince the reader of the accuracy of the model calculations by adding detail to the model description in this manuscript as well. This also feeds back to your earlier comment regarding our model; how it is less sophisticated than other models. This is not the case since our micrometeorological approach of SEB and SMB calculations, equal to those done by Van den Broeke et al but with added dimensions, allows for more accurate results than can be obtained by regional climate models that are not constrained by local observations, but by e.g. ERA interim fields. So indeed, it would be beneficial to add to the model description. I will try not to make the section too lengthy though, since J. Box already mentioned the model description to be "somewhat tedious". But in any case a more detailed description of the only tuned parameter (accumulation) will be included.

AR2: In general, however, the best and most interesting part is the runoff part. There are several issues that have been raised by other comments that need to be addressed before the paper can be accepted for publication.

DVA: Agreed, I will address all comments/issues.

AR2: I tend to disagree with the comment form one of the reviewers that a data set should be used only once a paper is published: there are many data sets that are based on technical documentations without going through a full peer reviewed process. However, it is important to provide a robust and scientific support to the quality of the data reported and the associated methods.

DVA: Ok, thanks. I will take this into consideration in my reply to this reviewer. Feel

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free to follow / add to the (upcoming) discussion with that reviewer.

AR2: My recommendation is to shorten some sections that are repeating from previous papers and focus more on trying to provide new insights on the 2010 melting record or on the combination between modeled and measured runoff.

DVA: I agree. I will try to shorten the beginning of the results section, and will add a figure on meltwater runoff. See also my reply to your earlier comment above.

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