

Review of the article entitled "Inter-comparison of surface meltwater routing models for the Greenland Ice Sheet and influence on subglacial effective pressures."

1 General comments

This study focuses on different ways of treating the supraglacial drainage of water at the surface of ice sheets. The region of interest studied here is Russell glacier in South West Greenland. The authors presents an intercomparison of three different surface routing models and compare their results to the output of a Regional Climate Model (RCM). The different inputs are further compared through by using them as the forcing quantity provided to a subglacial drainage model. The conclusion of the study are that the use of a supraglacial drainage system allows to get a better representations of the lag of the water input to the moulin. Some sensitivity among models also allow to quantify the impact of the Digital Elevation Model resolution on the drainage characteristics

This study provides an interesting insight into the differences that arise from the use of different supraglacial drainage model. However, my impression is that this study should be further refined in order to be more understandable and provide a usable tool for the community. I find that the presentation of the different models and their results is lacking detail and clarity. Moreover I am concerned by the choice that were made with regards to the boundary condition that are applied to the subglacial hydrology model. Details of my concerns and potential improvement are given bellow section by section.

1.1 Abstract

The abstract is quite hard to read due to the accumulation of acronyms. Where possible I would urge the authors to refrain from using acronyms in this part of the papers. It might be beneficial to simplify the abstract to make it more accessible to readers which might afterwards gather the details of the study in the rest of the paper. As an example, the author could state that they compare three surface meltwater routing models at this point

without specifying those models, the list of variables line 21 (page 1) could be omitted and replaced by “key variables”.

1.2 Introduction

The introduction gives a succinct outlook on the motivations of the study. This could be developed further to point out the current lack of representation of the supraglacial drainage system and the necessity to have a better representation of this system. The description starting on line 20 (page 2) would fit better in a method section of the paper. Moreover, some terms defined in the introduction (such as Unit Hydrograph or Internally Drained Catchments) might not be familiar to the Cryosphere community and the author should consider defining those in more details.

I don't completely agree with the statement starting on line 10 (page 2) to my knowledge supraglacial meltwater routing is usually simplified in subglacial hydrology models (*e.g.* Banwell et al., 2016; de Fleurian et al., 2016) it would be interesting to have some citation here that present studies directly using an RCM as their water input. I am not sure that the citation to Flowers et al. (2018) is relevant in this context or I missed the point of the author here. Further down, the citation to Bartholomew et al. (2011) seemed to be misplaced here as this specific study treats about observations rather than modelling.

1.3 Study area and data source

This section is missing a major information as the study area is actually never named. The Russell glacier region will be familiar to most of the reader interested of the subject but mention of it should still appear in the paper. My opinion is that this section should be merged into a section 3 (Methods and Data). Regarding the content of the present section it is not clear to me how and why the IDCs that are presented in this section were generated and why those specific IDCs have been chosen.

1.4 Methods

The description of the different models here is quite brief and some more details could be provided. Particularly it would be interesting to have a better overview of the advantages and drawbacks of each models. The paragraph starting line 15 (page 5) would fit better in the introduction of the study rather than here. Subsections 3.5 and 3.6 refer to the sensitivity studies that were performed for some of the model, it could be beneficial to transfer those sections into the descriptions of the relevant models. That would outline the advantages and potential drawbacks of the models and would clarify the overall setup of the experiments.

From the references that were provided in the paper regarding the SRLF model I understand that this model is routing water with different equations if it sits on snow

or on bare ice. From the model description given here it seems that only the bare ice formulation was used. Is that so? If yes the reasons for this choice should be explained.

As stated above my main concern with this study is the way in which the subglacial hydrology model is set-up. In my opinion the boundary condition that is given for the left edge of the domain is not realistic, I do not think that we expect to find water pressure at the atmospheric pressure anywhere under the ice sheet. A more sensible choice would be to set the water pressure at a given fraction of the overburden pressure. A change of boundary condition would need to perform new simulations but I would expect a good argumentation on the choice of the present boundary condition if it is to be kept. I also do not understand why the slopes of the bed and surface, and velocities are not taken from the values of the IDCs as is done for the ice thickness. As it stands now I have a hard time trusting the results from the subglacial hydrology model as it seems that the downstream boundary that is currently set is exerting an important control on the whole domain. I would also note that the Figure 2 related to this section is not very informative and could probably be omitted.

1.5 Results

In general I find the presentation of the results quite hard to follow. This might come from the structure that was chosen by the author, from the presentation of the figures or both. I also wonder why only the results from IDC 1 are presented, it appears from the supplementary figures that the results from the four IDCs are quite similar but this should be stated. I also expect that changing the boundary conditions and parameters of the subglacial hydrology model may alter those results.

Regarding the presentation of the results, it would be clearer to me if the author would describe first the results of the intercomparison itself before delving into the sensitivity studies that were performed on DEM resolution and the value of A_c . Comparing the results to a given reference might also help with the clarity of the text. I generally find the presented figures a bit too busy and so hard to read. Figure 3 is described a lot throughout the manuscript but the size of some panels make it hard to read. As for the text having specific figures for the intercomparison and the sensitivity study might help to lighten the figures. Figure 4, 6 and 7 however are not described in the Results part and should be included there. Lastly I have not seen any information with regard to the sampling of the effective pressure that is discussed, is it an average value or this value is taken at a specific point?

1.6 Discussion and Conclusion

The discussion of the manuscript is clear, it would however take advantage of the alterations suggested above for the Result section. Particularly describing all the figure in more details in the result section would help during the discussion. I also expect that the

changes required above regarding the subglacial hydrology model might have a significant impact on the results and should be taken into account in the discussion. I have noted a few minor concern on this section which are listed in the Specific comments bellow.

2 Specific comments

Bellow is a list of more specific comments throughout the manuscript given with line and page number:

- Page 1
 - Line 16: “ice surface” can be replaced by “ice sheet surface”.
 - Line 17: “climatological melt ” should be replaced by “surface metwater”.
 - line 21: MAR abbreviation is not defined here.
 - Line 23: “input” can be replaced by “used as input”
- Page 2
 - Line 2: Surface melt is not restricted to the ablation zone but occur in the accumulation zone too.
 - Line 3: “Greenland ice surface” should be “Greenland ice sheet”
 - Line 3: “can be” should be “is”
 - line 14: Bartholomew et al. (2011) does not seem to be a fitting citation here as this paper treats of observations.
- Page 3
 - Line 3: “to discern”, to is missing
- Page 4
 - Line 13: The parameters C_p and C_t should be explained.
 - Line 13: “time-to-peak in” reads strangely.
 - Line 15: I am not sure that the citations are needed here an interested reader will find those in Smith et al. (2017)
- Page 5
 - Equation 2: t, t_c and t_h are not described in the text.
 - Line 16: Replace “research” by “study”.

- Page 6
 - Line 6: The contributing area (A_c) should be introduced and discussed in the model description.
 - Line 15: “compute” rather than “derive”.
 - Line 19: “framework” could be omitted.
- Page 7
 - Line 6: “climate model” can be skipped here.
 - Line 7: The times given here do not agree with the one that are present on Figure 3. The author should chose which are the more relevant and keep them throughout.
 - Line 16: I don’t agree with the statement on the smoothness of the UHs. From the figure it seems that the UHs from SUH are actually the smoothest of all.
- Page 7
 - Line 2: Shouldn’t it be “potential dynamism”?
- Page 9
 - Line 16: Figure 7 actually shows the results from the three different models not only SUH. The comparison between the results of SHAKTI with the forcing from the RCM and the various models should be presented here to convince the reader of the advantage to use those models. As stated before, the set-up of the subglacial hydrology model should be corrected to give convincing results. I am also unsure of the location where the effective pressure presented on Figure 7 is sampled from the model.
 - Line 30: The study from Chandler et al. (2013) actually shows subglacial travel time. I don’t see how this reference fits here.
- Page 10
 - Line 18: Should be “bare ice”.
- Figure 2: I don’t think that Figure 2 is necessary and it could be skipped.
- Figure 3: This figure is quite hard to read as it holds a lot of information. I would suggest to plot on this figure only the optimal simulations for SRLF and RWF which would allow an easier and more fair intercomparison of the models. An other solution might be to split the figure to present the intercomparison on a specific figure and the sensitivity studies on others. Finally, a zoom on some relevant period for the

discharge and effective pressure would help the comparison of the different models. I also noticed a discrepancy here between the times given in the first column and the one of the text. It would be advantageous to introduce the RCM instantaneous runoff in the first column for ease of comparison.

- Figure 4: Figure four is barely described in the text, it should either be better described or completely omitted.
- Figure 5: A_c is given here in km^2 , it should be given in m^2 for consistency with the rest of the manuscript. The caption here could be shortened to its descriptive part.
- Figure 7: As for Figure 3 this figure is quite busy and should be simplified. The caption here is not adequate with some description missing and some discussion points that could be stripped.
- References: dois are missing from the references

References

Banwell, A., Hewitt, I., Willis, I., & Arnold, N. (2016). Moulin density controls drainage development beneath the greenland ice sheet. *Journal of Geophysical Research: Earth Surface*, 121(12), 2248–2269. doi: 10.1002/2015JF003801

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