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 moulins. 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. This new version of the manuscript presents some improvement with respect to the first iteration but it feels that some work still needs to be done to convey the full potential of the study. Improvements in the text increased the readability and make it more accessible for the glaciological community. The figures could be further improved to help with the comparison and present a more in depth intercomparison, in particular, the figures for all the IDCs should be included in supplementary if they are not provided into the text. The improvement to the subglacial hydrology set-up is definitely yielding better results but I still think that the impact of the boundary conditions (BC) on the system is quite prevalent, the set-up should be refined further or the impact of the BC should be clearly pointed in the text. Below are some more details and other points that should be considered.

1.1 Abstract

The abstract is now far easier to read and the work on that has paid off.

- **Page 1**
- Line 15: I would replace “and impact” by “where it impacts”.
- Line 25: Shouldn’t it be “indicate that”?
- Line 33. I have a hard time understanding the sentence starting here.

1.2 Introduction

The Introduction now gives a better understanding of the problem at hand and of the state of the art. One point that should still be improved is the description of the SRLF model, it should be stated that this model is able to treat both bare ice and snow covered regions but that only the bare ice component has been used in this study.

- **Page 2**
- Line 4: Greenland Ice Sheet acronym (GrIS) should be introduced here.
- Line 13: RCM can be omitted as it is already defined in the abstract.
- Line 13: the Sentence starting on this line “In these instances ...” is very long. It could beneficiate from rewording. The citation used to defined IDCs could be replaced by an original sentence here.
- Line 18: Is “very long term” accurate here considering the fact that the conclusion are pointing toward the fact that the routing scheme does not have a huge influence on timescales longer than a day.
- Line 28: As for the IDC, having the definition of the UH as a citation makes the sentence very long and heavy. Please consider to have those definitions as stand alone sentences.
- **Page 3**
- Line 16: “and have different data requirements”.
- Line 29: I am not sure of the meaning of intra-compared here.
- Line 32: “demonstrate” seems to be an overstatement.
- **Page 4**
- Line 1: You mean, best model, approach?

1.3 Methods and Data

This section as been greatly improved but there are still some imprecision.

Page 4, Line 14, it is stated that there are no large lakes in the IDCs, I suspect that the reason is that the IDCs have been chosen to avoid the presence of lake which would hinder the results of some routing models. If that is so this should be explicited in the text.

Page 4, Line 27 and following, it seems that MAR is referenced twice. The second description starting line 30 should be kept but it should probably be introduced in the section above rather than have its own section.

Page 4, Line 30 and other places in the manuscript. I feel that the simplification of "surface runoff" to "(R)" is not needed and that the former should be kept throughout the text.

Page 7, Line 14, here it is suggested that SRLF routing scheme does not evolve throughout the melt season which is somewhat misleading. I agree on the statement that the drainage on the bare ice component does not evolve but if one uses the full model then the evolution from snow covered to bare ice terrain will lead to modifications in the seasonal draining pattern.

- **Page 5**
- Line 15: SUH have been introduced before and should not appear here.
- Line 15: "assumes" should be replaced by "assumes that".
- Line 18: I am not confident with the Snyder SUH but it seems strange to have a discharge expressed as inverse of time.
- Line 18: Equations should not be written in line to help clarity.
- Line 20: "from the 20 to the 23..."
- **Page 6**
- Line 2 and Table 1:SRLF was not used in de Fleurian et al. (2016), the basic of the routing is the same but the scheme use in this study way simplified. Hence I don't think that de Fleurian et al. (2016) has its place here.
- Equation 2: This equation would read better in its fraction form.
- Line 12: "synthetic unit hydrograph" and "rescaled width function" could be abbreviated here.
- Line 16: The acronym has been introduced before and is not necessary here.

- Line 30: “hydologic” should be replaced by “hydrological”.
- **Page 7**
- Line 12: Is it really the transport distance that change or the type of drainage. This could use some clarification.
- Line 26: This sentence is repeated in the next one and could be omitted.
- Line 31: I am not sure that “conducted” is the good term here.

1.4 Results

The new presentation of the results in the manuscript improved the readability. However, the figures are still on the small side to be really able to compare the results of the different models which would be the main focus of an intercomparison paper. My concern on the effect of the boundary conditions on the effective pressure has not been settled by the new set-up, I would recommend to use a far larger domain to avoid these boundary effect even if only a small region of this domain is analysed later on. For example using the SHMIP geometry from de Fleurian et al. (2018) with a single moulin located at a convenient position could be an option. If the author decide that the present set-up is enough for their use I would expect a more in depth analyses of the results in light of the potential impact that the BC have on those. The impact of the BC is clearly seen on the animation. The water head is lower at the right end of the domain when the water input is low and the gradient is very high when the influx is high. I suspect that this impacts the amplitude of the effective pressure variations.

For the figure that are presenting results on the IDCs, all the IDCs should be presented. I agree that only IDC 1 should be discussed and presented in the paper itself but the other IDCs should be shown in supplementary. Also take car when submitting your supplementary figures that they have the same layout as the one of the main manuscript.

Regarding the layout of the figures I would suggest to make a clearer separation between the figures related to the intercomparison (Figure 2 a-f, Figure 5 a-c) and the one more related to the temporal evolution of moulin discharge. That would allow to focus on shorter time scales (a few days) on the intercomparison figures and allow an easier comparison for the reader. Bellow are my suggestion figure by figure.

- Figure 2: I suggest to remove panels g and h of this figure and have them as a separate figure. With those panels removed, the temporal evolution on the discharge is not as relevant as it seems to be quite similar throughout the season (at least the difference between model does not seem to evolve). Those discharge plot can then be focused to span just a few days which would ease the comparison between models. The supplementary figures for the other IDCs should be the same as the figure presented in the paper (again to ease comparison)

- Figure 3: I feel that this figure is only relevant for the dynamic A_c simulations. For the other simulation the seasonal evolution is not as interesting and the intercomparison is easier to perform on Figure 5. I would Use Figure 5 (with the modifications bellow) here and move this one to supplementary.
- Figure 4: This figure should be shown for the other IDCs in the supplementary along with animations for all IDCs.
- Figure 5: To my eyes this figure is the one that really allow to compare the models. The comparison would be even easier if there was one panel used for the RCM results and then one for each model (presented in a single column). The results from RWF should probably be omitted here as the Moulin influx at this date is quite different form the other and can not really be compared. I also note that on this figure the panel are not subscripted and that it is not reproduced for the other IDCs in the supplementary.
- **Page 8**
- Line 30: "study" should be replaced by "studied"
- **Page 9**
- Line 10: UH is already introduced at this point and should be used here.
- Line 11: "cm" should be spelled out.
- Line 12: The sentence starting here is confusing. Perhaps state first that the results of the IDCs are similar before pointing to the figures.
- Line 18: I still think that smoother is not the correct term to use here, I would prefer "UHs with lower amplitude" for example.
- Line 20: Are the peak really higher than 25% higher or just around 25% higher, in the second case you should use $\sim 25\%$.
- Line 21: The comment above applies to the discharge range too.
- Line 23: See comments above with respect to inequality notations.
- **Page 10**
- Line 10: A reference to the figure is missing here.
- Line 12: "...surrounding bed perpendicular to flow." I don't see the meaning of that.
- **Page 11**

- Line 5: I don't see how the A_c threshold with unit of area is a proxy for time.
- Line 24: It does not seem that it is Figure 6 that should be referenced here.
- Line 29: It should be said here that RWF can not use DEM of coarser resolution. As it is now this comparison seems to be a bit unfair towards SRLF.
- Line 30: As stated before I don't think that smoother is a good term here. It seems that "lower amplitude UH" would be more descriptive of what is seen.

1.5 Discussion and Conclusion

If the author decide to keep the current set-up for the subglacial hydrology model (which I do not recommend) a careful assessment of the effect of the Boundary conditions on the effective pressure variations should be given here. In particular when describing the amplitude of the effective pressure variations.

- **Page 12**
- Line 11: This title seems strange.
- **Page 13**
- Line 1: It should be "empirical"
- Line 4: It should be "empirical"
- Line 13: This sentence should be written the other way around, it is the variation in subglacial pressure that trigger variations in ice velocity.
- line 17: This is an important and interesting statement that could be investigated further. The Dynamic A_c simulation gives a good test case to look into this hypothesis.
- Line 28: I don't think that the storage capacity of the supraglacial system is demonstrated, it is more presented.
- Line 31: This could be seen the other way around too and routing models could help to parameterise the storage term built into subglacial hydrology models. It would be interested to test that to see if they have exactly the same effect on subglacial water pressure.
- **Page 14**
- Line 5: It should also be stated here that RWF can only operate on High resolution DEMs.

References

de Fleurian, B., Morlighem, M., Seroussi, H., Rignot, E., van den Broeke, M. R., Munneke, P. K., ... Tedstone, A. J. (2016, OCT). A modeling study of the effect of runoff variability on the effective pressure beneath Russell Glacier, West Greenland. *J. Geophys. Res*, *121*(10). doi: 10.1002/2016JF003842

de Fleurian, B., Werder, M. A., Beyer, S., Brinkerhoff, D. J., Delaney, I., Dow, C. F., ... et al. (2018). Shmip the subglacial hydrology model intercomparison project. *J. Glaciol.*, 1–20. doi: 10.1017/jog.2018.78