

## ***Interactive comment on “Inter-comparison of surface meltwater routing models for the Greenland Ice Sheet and influence on subglacial effective pressures” by Kang Yang et al.***

### **Anonymous Referee #2**

Received and published: 19 December 2019

Review of: “Inter-comparison of surface meltwater routing models for the Greenland Ice Sheet and influence on subglacial effective pressures” by Yang et al.

General Comments: There has been a significant amount of recent work done on capturing processes influencing moulin hydrographs on sub-diurnal timescales. Going forward with these approaches will require significant investment of resources, particularly if field-derived empirical parameters are needed to calibrate supraglacial hydrology models. Underlying this work is an assumption that moulin discharge variability at sub-diurnal timescales might impact the evolution of inter- and sub-glacial hydrological networks, and thus ice dynamics. If this is the case, then supraglacial hydrological

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processes necessitate further investment to be properly constrained at fine temporal and spatial scales. However, the impact of supraglacial discharge variability on subglacial hydrology has not yet been investigated, and it is therefore not clear if, where, and what specific investments are needed. In this context, this paper makes two major contributions:

1. This paper is a first attempt to investigate the extent to which moulin hydrographs matter for subglacial channel evolution and effective pressure on diurnal timescales.
2. This paper evaluates three different contemporary approaches to estimating daily moulin hydrographs and evaluates the consequences of each with respect to modelled evolution of subglacial channel evolution and effective pressure.

This paper therefore constitutes an original and valuable contribution to ongoing research on supraglacial hydrology. The paper could be improved by clarity and specificity around the methods used and the objectives of the paper. Suggestions in this respect are provided below and in the accompanying annotated PDF. My comments focus primarily on the supraglacial hydrology components of the study.

Specific comments:

Title: Only one of the models is a routing model. Consider saying ‘inter-comparison of moulin hydrograph estimations’ or something similar.

Throughout: The use of ‘routing models’ seems inaccurate. Only one (the SRLF) approach is a flow routing approach. The other two (RWF and SUH) do not route flow. A different word choice would be preferable. A ‘comparison of hourly moulin discharge models’, or something similar. . . .

Introduction: In general, I do not find that the introduction sets up the objectives of the paper very well. It does not provide sufficient information to set up a methods comparison, but also only emphasizes the subglacial channel evolution at the end – like an afterthought. I suggest the following changes:

- You need to be clearer in your introduction that this is not a methodological paper per se – as you say later in the paper, you cannot say which method performs better due to a lack of empirical evidence. You can only speculate on the modelled (not observed) hydrological implications of the three different methods. This needs to come across more strongly.

- The differences between the three approaches as well as the assumptions and limitations of each needs to be made clearer for a glaciology (rather than a hydrology) readership. The limitations of the empirically-derived RWF and SUH also need to be made clear, particularly the temporal limitations of the original field-derived moulin hydrograph measurement. As is, a comparison of these three approaches is only useful for conditions similar to those under which the SUH and RWF approaches are calibrated – this needs to be made clear.

- Because the comparison between the approaches is inherently limited due to the temporally-limited nature of the field moulin discharge measurements in the SUH and RWF, I think it would be useful in the introduction to put more emphasis on the goal of the paper as an exercise in examining (modelled) subglacial effective pressure sensitivity to diurnal hydrographs, rather than explicitly a comparison of moulin discharge estimate approaches. I would consider a bolder introductory statement around Page 2, Line 17 that frames the paper as a (preliminary) investigation of the extent to which moulin hydrograph estimates matter for affecting modelled subglacial hydrology and effective pressures.

- Last paragraph of introduction. I think there needs to be more explanation of what you are hoping to achieve with using the SHAKTI model. I think you need to be explicit that there is no objective way to compare the three moulin hydrograph methods, and the differences between them only matter in a glaciological sense if they significantly impact subglacial hydrology and effective pressure. You therefore run what is loosely a sensitivity test using SHAKTI to assess the modelled impacts of each approach. Although this comes out later in the paper, you need more framing of this consideration

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in the introduction.

Study area and datasets:

- More specific justification of the chosen study IDCs is needed. They are approximately similar sizes to the IDC used in the Smith et al. (2017) measured moulin hydrographs, which should be pointed out, and they also appear to exclude large supraglacial lakes, which is likely to affect the comparison of the SRLF approach with the RWF and SUH approach. This should be noted in the study area description and in the discussion.

Methods: Overall, the methods seem written for hydrologists, not for glaciologists. More information is needed in this section to make it useful to its readers.

- Presumably, July 2015 was chosen for the MAR runoff simulations because that is coincident with the field-collection of the moulin discharge hydrograph. This should be made clear, so that the constraints on the method are obvious to readers.

- If this paper is to be a useful methodological resource for glacier hydrologists, a more complete comparison of the three approaches is needed. Perhaps a table would be useful in comparing the three methods – this table could keep track of the references, acronyms, assumptions, limitations, etc. . .

- Section 3.6 – your explanation of a ‘dynamic’ Ac is not clear, or perhaps I have missed it. In Figure 3, it looks like you tested all five of the different Ac values independently over the whole time span, but then there is also a ‘dynamic Ac’. Is a ‘dynamic’ Ac one that evolves according to your six five-day RWF-UHs? Be sure to call that ‘dynamic’ here.

Results: the figures for this section are confusing. They need to be pulled apart to be more readable, and the legends and captions need better information.

- Figure 3 is too confusing with this many panels, and the result is that the lines are too small to make out the subtle differences due to the different variables. I suggest taking

the third column (effective pressure) out and putting it in its own figure, as it is a distinct part of the results and discussion. You could then make the main figure slightly larger, and show with a title on the legend in (g) and (j) that the different series refer to g) the DEM resolution and j) the channel initiation threshold (proxy for time).

- Figure 4 is not discussed in the results section. Some discussion should be provided in the 'long-term evolution' section, or it should be removed.

- Figure 3k – how is that there is so much smaller discharge for 5000m<sup>2</sup> than there is for 100m<sup>2</sup>? With a higher  $A_c$ , there should be less efficient routing, lower peak  $Q$  and a flatter hydrograph but still, presumably, similar discharge. This distinction is not clear from the figure, perhaps because the lines are so compressed and the 'flashiness' of the hydrographs is not clear. Perhaps an inset figure would be helpful.

Discussion:

- Overall, I think this section is too critical of SRLF and not critical enough of SUH and RWF. Some discussion of the limitations of the latter two is needed, namely: the chosen study catchments do not appear to have lakes, and those methods may not perform adequately in catchments with lakes, at different times of year and in different snow/ice/surface slope conditions than those in which the field-measurements of the moulin hydrograph (Smith 2017) were collected.

- More discussion of the limitations of SHAKTI would be helpful. You should be clear that SHAKTI is used to provide preliminary insight into the possible importance of accurately capturing the details of an hourly moulin hydrograph, and that many complexities are not captured by this model.

Technical corrections: Please see specific in-text comments in the attached annotated PDF.

Please also note the supplement to this comment:

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<https://www.the-cryosphere-discuss.net/tc-2019-255/tc-2019-255-RC2-supplement.pdf>

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