

Interactive comment on “Hydrology and runoff routing of glacierized drainage basins in the Kongsfjord area, northwest Svalbard” by Ankit Pramanik et al.

Anonymous Referee #1

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1. General comments:

This paper presents numerical experiments on meltwater discharge from tidewater and land-terminating glaciers in the Kongsfjord basin in Svalbard. Meltwater runoff was computed by an energy balance and snow process coupled model, which was previously published by the authors (Pramanik et al., 2018). The results of the previous paper are used in this study to investigate water flow through the glaciers and discharge into the fjord. Two different runoff routing models were applied for the glaciers in the basin to obtain time series of discharge from glacier front. Experimental results are presented in terms of flow paths and drainage basin, as well as time series of glacial

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discharge (hydrograph) from 2013 to 2016. I enjoyed the readable text and carefully prepared plots. Discharge from tidewater glaciers is drawing attention because of its importance in glacier/ice sheet mass loss and for the interaction of glaciers and the ocean. The authors tackle this problem by applying runoff routing models for a relatively well studied glacier basins in Svalbard, where a long-term proglacial discharge and plume observations are available. Results are interesting and potentially important to understand hydrology of glaciers under a similar setting. A weakness of the study is poorly constrained model parameters. This is critical because validation of the model output is only possible for the land-terminating glacier, where proglacial discharge data are available. Modeled discharge from a tidewater glacier is compared with plume area, but it is insufficient to optimize the model. Because of this shortcoming, it is difficult to assess how realistic the presented results are. Accordingly, discussion of the experimental results is pretty weak and the authors failed to draw important conclusions. In my opinion, more rigorous conclusions are required for a paper published in The Cryosphere. I see the value of the experiment and potential importance of the result, thus encourage the author to perform more careful experiment and writing. In my opinion, the paper will be substantially improved by setting clear objectives of the study, designing experiments to overcome the parameter uncertainties, and analyze experimental results to demonstrate the importance and implication of the study. I list my major concerns below, which are followed by specific comments.

2. Major concerns:

(1) Drainage basin analysis It is interesting to see the drastic change in the drainage basin boundaries, depending on the choice of the parameter "k". I wonder if you can enhance this finding by more detailed presentation and analysis. For example, area of each drainage basin can be plotted against "k", so that quantitative analysis is possible for the impact of "k" on the drainage from each glacier. I am also interested in the mechanism of such migration of the drainage boundary. If you focus the region of "subglacial water piracy" and explain the process in terms of bed/ice geometry, you may

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be able to generalize such finding for future research. Please also discuss this finding with an attention on surface water production and water transfer from the surface to the bed. Even if a large area in the upper reach switches to another drainage basin, the influence on the glacier discharge is small in case the area is above the percolation zone. because melt is small and do not penetrate to the bed.

(2) Discharge hydrograph I understand that obtaining a hydrograph is an important goal of this study. The reconstruction of hydrograph is successful for the land terminating glacier (Fig. 5). In contrast, results for tidewater glaciers are not reliable. It is not clear how parameters were tuned for the tidewater glaciers and the validation of the results is not convincing (Fig. 4). Further, parameter settings are very simple, as represented by "k" assumed as uniform in time and space. Therefore, hydrographs for tidewater glaciers are questionable, and uncertainty is unclear. My suggestion is to perform sensitivity tests and evaluate the uncertainties in the results. By taking various values of k , α , and water speed, uncertainty can be evaluated for the discharge and presented as a band in Figs 4 and 5. Please also discuss Fig. 4 in terms of agreement between the discharge and plume area. Frankly speaking, I do not see "agreement" in the plot.

(3) Model parameters Parameters uniformly distributed in space and time are very crude assumptions. Significant spatial variations are expected for "k", and it changes over a year particularly in the ablation area. Water flow through a glacier consists of complex processes, thus speed of water movement varies in time and space. Moreover, processes involved in water movement after runoff is given by the melt-snow model is not very clear. Do you assume water drains straight down to the bed? I believe the time required for such process is highly uncertain and variable. Taking all these uncertainties into consideration is not possible, thus some degree of simplification and assumptions are necessary for this study. Nevertheless, I think the treatment of the parameters is too simplistic. In fact, a large portion of Discussion is allocated to describe such short comings. I encourage the authors to perform sensitivity test and provide more rigorous discussion on the model uncertainty.

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(4) Objective of the study My question that forms the background of the comments listed above is "what is the objective of the study?". The abstract suggests "delay in discharge" is the main point of the study (line 2). In the end of Introduction, "drainage delineations" and "subglacial network" are raised as the purpose of the modeling (Line 49). Judging from the presented result, delineation of the drainage basin is worth to highlight. However, I am not sure if the study achieved accurate quantification of the delay (Fig. 3b) and what is new about the subglacial network. My suggestion is to define clear study goals. Experimental design, data analysis and presentation should be optimized to achieve them. It is not bad idea to place the focus on the subglacial drainage basin (Fig. 2) and hydrograph (Figs 4 and 5). Setting clear goals of the experiment should guide you to design numerical experiments and data analysis necessary to draw conclusions.

3. Specific comments:

Line 23: "minimal effect on the local circulation" » Freshwater discharge to the fjord surface has an effect to enhance stratification, isn't it?

Line 47: "The first order, Water..." » water

Line 47: "determined by bed topography and basal water pressure" » What about ice thickness?

Line 59–64: Please refer to Fig. 1 to explain the study site.

Line 62: "Warm water inflow ..." » This sentence is not clear. Please be more specific in time rather than "in recent years" and "until recently". Also not clear what happens "since 2006".

"Line 69": "terminus depth" » Not clear if this refers to the ice thickness or fjord depth.

Line 71–75: Because this region is relatively well studied, can you describe more about previous works in this region?

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Line 85: "Runoff from a Coupled . . ." » coupled

Line 94: I wonder why you write "interested in discharge over the period 2010–2016" because Figs 4–6 cover only 2013–2016.

Line 120–121: I understand that you assume supraglacial discharge (no basal hydrology) at land-terminating glacier. Please clearly state so if this is correct.

Line 128: Something is wrong with the unit (hyphen and italic). Please check throughout the paper.

Line 134: ". . . water moves according to the bed topography alone." » And ice thickness?

Line 152: What kind of "distance"? Distance from the point on the meltwater production to the glacier front along the drainage path? Can you reword "water wave speed" by "water movement speed" (or water flow speed) as in Line 155? "Wave speed" can be something different, I think.

Line 166: Please consider writing "peak" instead of "maximum".

Line 189: Liston and Mernild (2012) applied the model to a land-terminating glacier. Can you validate the use of the same parameters for tidewater glaciers? For example, the parameter $k(t)$ in Equation (4) in Liston and Mernild (2012) is critically important and dependent on glacier conditions.

Line 206: "Hotedahlfonna and Isachsfnonna" » I understand these are names given on the upper regions of Kronebreen and Kongsbreen. It is confusing to use four names for two glaciers. Can you reword them with something like "accumulation areas of Kronebreen and Kongsbreen"? It helps readers.

Line 210: "(Fig. A4, A5)" » Can you refer to Fig. 2a instead of the supplementary figures?

Line 215: "For k values between 0.5 and 1, . . ." » Can you refer to Fig. 2d for this

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sentence?

Line 226–232: I find the described procedure of the parameter tuning is highly uncertain. The choice of $\alpha=0.15$ sounds subjective. Please consider sensitivity tests instead of fixing the parameters.

Line 235–236: I do not understand the basis to use $k=0.8$. Why "therefore"?

Line 242: What is the definition of "discharge days"?

Line 243: "late discharge events in 2016" » Are you referring to the peaks in September? Please specify.

Line 251: "We also compare peak discharge between . . ." » Do you compare "timing" of the peaks?

Line 265–270: Please refer to Fig. 2 for the text, i.e. Fig. 2d for the first sentence and Figs 2b and c for the 3rd sentence.

Line 265: Once again, it is confusing to use "Holtedahlfonna and Isachsfnonna".

Line 308–310: What kind of process do you have in your mind about "runoff from basal melting"? Do you assume basal melt events in early summer and in late autumn? Any literature about it?

Line 324–326: I encourage the authors to discuss more about the implication of your study for circulation and biology in the fjord.

Line 380: Space is missing in the unit " ms^{-1} ".

Figure 6: As far as I understand, this kind of data can be obtained basically with the melt-snow model, but the drainage from tidewater glacier basins is influenced by the water piracy. Can you discuss more about this point to highlight the importance of your study? On Fig. 6b, I would compare this result with that expected by supraglacial drainage as well as those obtained by other values of the parameter " k ".

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