Response to anonymous referee #3

General comments:

Supraglacial stream/river networks route large volumes of surface meltwater on the Greenland Ice Sheet but their discharge remains poorly estimated. This paper presents very valuable discharge measurements and continuous water level measurements. These measurements, to my knowledge, are the first long-term (almost entire summer 2016) observations of supraglacial streams and will significantly improve our understanding in Greenland surface hydrology. Therefore, I suggest this paper will be a very good contribution to TC and recommend it to be published. I have the following comments. Some of these comments are proposed for further discussion so it is fine if the authors cannot address all the questions. But I suggest the authors consider including some of these comments in the revised paper to make their paper more broadly interesting.

This paper compares stream discharge with surface energy fluxes and reveals shortwave radiation as the primary driver of melting (contributing 50-78 % energy). Besides surface energy fluxes, it will be useful to compare stream discharge with modeled surface runoff because these two variables can be directly compared. The catchment boundary is delineated with very high accuracy so this comparison is possible. I assume the comparison is not conducted because the studied catchment is too small to compare with coarse-resolution RCM simulations. If so, I suggest the authors briefly explain it in the revised manuscript.
Additionally, it is necessary to better explain how proportions of melt energy supplies are obtained from discharge-energy correlation analysis in the Abstract and Results.

Author reply:
We appreciate the suggestion about comparing with modeled runoff from Regional Climate Models. However, the objective of this manuscript is to highlight the novel data set of stream discharge and its drivers over the span of 62-days of the melt season. We are going to revise the manuscript to make our objectives and science questions more clear (see list below, these comments are also included in response to reviewer 1). We think this paper stands on its own, and believe that a model comparison study would require a separate manuscript to do a rigorous model and observation comparison that would provide new insights. Indeed, as the reviewer points out, the RCM model resolution is large compared to this catchment, which requires a more involved study than we think should be added to this manuscript.

Novelty of our study

a) While the conclusion about surface energy balance is not new, our study is unique by using direct measurements of runoff. Previous studies used weather stations and model simulations to estimate melting to compare with surface energy components. We will rewrite the manuscript to make these points clearer.

b) While timing of daily maximum discharge has been estimated using in-situ discharge in previous studies, those studies only span a few days. In our study, using 62-days long in-situ stream discharge, we show that this timing of daily maximum discharge changes over the melt season due to varying catchment conditions. This long term record allows us to make assessments of supraglacial stream dynamics that are more substantiated than other studies that may be subject to random variability within the system. More explanation about this change and the processes affecting it will be given in the manuscript to make this clear.

c) The goal of performing the hydraulic geometry analysis was to see how the 660 catchment parameters compare with the previous studies and if they fall into a range of parameters from similarly sized supraglacial streams. Our parameters compare well with the parameters from Knighton (1981) and Marston (1983), but do not match with streams from Gleason et al. (2016). Indeed, we reach the same conclusion as Gleason et al. (2016), i.e that hydrologic parameters cannot be generalized over the Greenland ice sheet. However, we find that our study is a contribution given the extremely small number of similar studies. In other words, part of the novelty is that Greenland supraglacial stream flow observations are extremely rare, and long observations like ours are even more rare. Additionally, hydraulic parameters of supraglacial streams have shown to be highly spatially variable and this study furthers Gleason's conclusions by analyzing streams closer to the ice edge.

Research questions behind the collection of these unique measurements. We will clarify the research questions driving our work in the introduction. These questions include:

a) How does the supraglacial stream discharge vary over the entire melt season?
b) What are the drivers of this discharge throughout the season?
c) Given the findings of a model study by Yang et al. (2018) on how timing and amplitude of daily discharge change with stream network size and evolution, what does the timing and amplitude of discharge suggest about network evolution in our study catchment?
d) How do the hydraulic geometry parameters of the streams in 660 catchment compare with the previous studies?

We actually did not use the discharge-energy correlation analysis. While the proportion of energy components are shown as a ratio to total melt energy. However, their contribution is shown as a percentage by normalizing the proportions from 0-1 and estimating their change after normalization. We will explain this better in the revised manuscript.

Specific comments:

1. line 11, it is not clear to me what "which" means in this sentence, "surface runoff" or "supraglacial stream networks"?

Author reply:
Here, 'which' meant the evacuation of meltwater. We will rewrite the sentence to avoid misunderstanding.

2. lines 15-17, I think this is not the main finding of the paper and should be moved backwards.

Author reply:
We agree that this is not the main finding of the manuscript, and we will de-emphasize it in the abstract.

3. line 29, three papers are cited so it is not clear where 286+-20 Gt comes from.

Author reply:
We will revise the manuscript and only cite the paper from which this number is from (i.e. Mouginot et al, 2019)

4. line 35, it is not straightforward to understand the importance of supraglacial streams from this sentence. I suggest adding "extensive supraglacial stream networks route large volumes of surface meltwater runoff each summer" or similar descriptions to make the logic easier to follow.

Author reply:
We appreciate the suggestion and will edit the sentence as suggested in the revised manuscript.

5. line 39, delete "drainage".

Author reply:
We accept the suggestion and will edit the text in the revised manuscript.

6. line 43, "or" the ocean?

Author reply:
It should be "and the ocean" since most water will actually end up there.

7. line 54, compared to smaller catchments with similar surface melt intensity, large catchments imply a longer stream network.

Author reply:
We will consider this sentence instead of the one we currently have.

8. line 58, besides weathering crust, meltwater stored in firn or snow can also influence discharge magnitude and timing.

Author reply:
We appreciate the suggestion and will edit the text as suggested in the revised manuscript.

9. line 100, what is the purpose to introduce "cryoconite"? it is not included in the following analysis.

Author reply:
We plan to add a new time series of water level and temperature in cryoconite holes in the revised manuscript to explain the shift in time to peak discharge. This introduction to cryoconite holes will be necessary to understand the analysis.

10. Figure 1, use a dot to indicate the study area; will it be useful to show the high resolution satellite image of the study area? Any crevasses or small moulins identified in the catchment? use different line widths to show streams of order 1, 2, and 3.

Author reply:
As requested, we will add a dot to indicate the study area. We experimented with different background images when we made the study area figure. We will add a third panel that shows an intermediate map where the reader can see where the study area is relative to land, and most likely we will use a satellite image as a background for that map. However, we found that any background images below the stream network did not add any important information for the study, and also made that panel unnecessarily busy and difficult to interpret.

11. line 69, why regional climate models (RCMs) are not used in this study?

Author reply:
We agree that RCMs comparison would be very interesting. However, as explained earlier, the objective of this manuscript is to highlight the novel data set of stream discharge and its drivers over the span of 62-days of the melt season. We think this paper stands on its own, and believe that a model comparison study would require a separate manuscript to do a rigorous model and observation comparison that would provide new insights.

12. Figure 2, is it possible to add scale bars in Figure 2a and 2b?

Author reply:
We appreciate the request and will add scale bars in 2a and 2b in the revised manuscript.

13. Figure 3, Thermal erosion cannot be identified because the deepest point changes over time. Is there a stable reference point during the field-work period? It is fine if there is not because I understand the main point here is to quantify the uncertainties of channel cross section rather than analyzing thermal erosion.

Author reply:
There was a stable reference point while collecting hourly measurements, with which the cross-sections were aligned to in Figure 3b. We will mention this in the text of the revised manuscript. However, we do not have the same throughout the season.

14. Figure 4, put legend in the figure to save space.

Author reply:
We accept the suggestion and will move the legend into the figure in the revised manuscript.

15. lines 162, "depth measurement errors can be isolated from incision errors", not clear what this sentence means.

Author reply:
The discrete discharge measurements over a cross-section are susceptible to both measurement and incision errors. However, with an assumption that the incision is small during a 26-hour period, the uncertainty in those hourly measurements are caused due to measurement errors alone. This is what we mean by isolating measurement errors from incision errors. We will rewrite and clarify this in the revised manuscript to avoid confusion.

16. lines 177-179, delete this sentence. Instead, add ack = 1, b + f + m = 1 after equations 2-4.

Author reply:
We will not add the equation because these equations only hold for ideal situations. In theory the sum of exponents and the product of coefficients must equal 1, but in practice due to measurement error and when $R^2<1.00$, i.e., the power law does not perfectly describe the data, and we can expect deviations from 1. However, we will add these sentences to explain the same.

17. line 180, although the HOBO weather station is not used, it will be useful to add several sentences to explain why it failed to provide continuous meteorological observations.

Author reply:
We have decided to completely remove and discard the HOBO weather station data from this manuscript. We have done additional quality control of this data, and found that we are unable to produce an accurate dataset from the HOBO station. The reasons for the poor data are most likely due to poor installation, for example the station was not installed at a fixed elevation above the surface, the station tilt was at times substantial so that the radiation sensors were far from level. Fortunately, a better quality dataset with the variables we got from the HOBO station could be replaced with data from another AWS station operated by the PROMICE project situated at a similar elevation ~7-8 km away. This is the same station that we used for the energy balance calculations.

18. line 197, the spatial resolution of panchromatic WorldView-1 image is 0.5 m rather than 1 m. Considering showing this image in Figure 1.

Author reply:
We appreciate your suggestion and will correct the spatial resolution to 0.5 m in the revised manuscript.

19. line 198, very impressive to identify the catchment boundary in such a convincing way. Which date was this work conducted? It will be useful to add the date. Also, it will be helpful to add a DEM-derived catchment boundary for comparison (optional though).

Author reply:
We appreciate the comment. We will include the date of boundary GPS measurements in the revised manuscript. However, we will not include a DEM derived catchment boundary. We actually have tried this, but it is not trivial and does not produce a good catchment due to issues such as small snow bridges and other factors explained in a paper by Kang Yang (Yang et al., 2015).

20. lines 200-201, "We estimate the catchment area to be accurate within 5% given that it was manually identified in the field. However, the precise delineation of the catchment is not relevant to the outcome of the study", what is the purpose of this sentence?

Author reply:

We added this sentence because we want to communicate that there is some uncertainty regarding the catchment area if people want to use this delineation for other purposes.

21. line 206, it is surprising that supraglacial streams remain stable at such low-elevation, fast-flowing areas. Any reasons? It will be useful to add ice flow velocities.

Author reply:
As requested, we will add a figure in the supplementary material that shows the ice flow velocities (e.g. https://nsidc.org/data/NSIDC-0725) and discuss any implications on the results.

22. Figure 6a, I think it is not necessary to calculate uncertainty of daily mean discharge by averaging hourly discharge. Actually, this is just unit transform rather than uncertainty. Also, I think it is more informative to add daily discharge on Figure 5a and move Figure 6b to Figure 5. By doing this, there will be a much illustrative Figure 5.

Author reply:
We are going to keep hourly discharge data in figure 5, and daily discharge data in figure 6. We plan to change panel 5b and 5c so that they are stacked and have the same x-axis length as panel 5a. We want to show the hourly discharge data. Also, we think adding panel 6b to Figure 5 would overload this figure with information. We want to separate the figure that makes a point about discharge variation (Figure 5) and energy balance variation (Figure 6). Furthermore, the gray shaded regions in both Figure 5 and 6 helps intercomparison of these two figures. However, we can remove the uncertainty envelope in Figure 6a if the reviewers and the editors think it is not important to the analysis.

23. Figure 8 is not easy to follow. Particularly, it is not clear what "proportion of melt energy" means.

Author reply:
Our intention was to show the contribution of individual energy components towards total total melt energy. However, showing percentage is just confusing with negative net longwave radiation and greater than 100% net shortwave radiation contribution (that balances out the negative net longwave radiation). Therefore, we decided to explain the contribution as a ratio to total melt energy (shows the same result as percentage except this will be in fraction) as the individual component's contribution to total melt energy. We will clarify this in the revised manuscript.

24. lines 273, what does "network storage" mean?

Author reply:

We will rewrite this sentence to explain the following points: The time to daily maximum discharge is influenced by the catchement's ability to evacuate water. The catchment capacity to evacuate water depends on how much water is transported in the stream channels versus overland on the ice surface. How long and wide are the channels, what are the resistance to flow in the channel and ice surface, how much water is trapped in the weathering crust and so on.

25. lines 293-295, delete this sentence.

Author reply:
We disagree that this sentence should be deleted. But we will explain better why our exponents do not add up to one as explained earlier.

26. lines 379-380, this sentence is not easy to follow.

Author reply:
We appreciate the comment and will rewrite this sentence so that it is easier to understand.