Reply to Reviewers

Ken Mankoff *et al.*

Comments from reviewers are in normal font and differentiated from the replies that use a **bold colored font.**

Contents

1 Summary

We are grateful for the helpful reviews and pleased that the reviewers seem to think this is useful work. We would like to warn and apologize to the reviewers that due to their helpful comments, the document has changed substantially - enough that while the LaTeXdiff program did not crash, it does not produce very useful results. This significant re-write means that some of the specific comments below cannot be directly addressed because some text and figures have been entirely removed, and that a second round of review may be a bit more work than if we had only done the minimum needed to address the comments. We hope you don't mind this extra work and see the value in the changes that we made. Thank you.

2 Review 1

2.1 Summary and General Commments

A high-resolution product of liquid discharge from the Greenland Ice Sheet (GrIS) and the unglaciated area of Greenland is derived for the period 1979 – 2017 and provided with various static hydrological quantities (e.g. basins and outlet locations). Gridded runoff is taken from two regional climate models (RCMs) simulations (MAR and RACMO), whose output is statistically downscaled to a horizontal resolution of 1 km. Hydrological characteristics (e.g. basin delineation) are computed from surface elevation according to ArcticDEM. BedMachine surface and bed elevation data is additionally considered to assess the sensitivity of the routing network to uncertainties in surface topography and to consider subglacial ice sheet drainage.

This study addresses a very relevant topic, namely the quantification of liquid discharge from Greenland in the current climate and specifically the locations where this freshwater will enter the ocean. It closes the link between RCM simulations, which provide gridded runoff at increasingly higher horizontal resolution and the need for (high-resolution) liquid discharge locations, which are not directly provided from RCM simulations. The manuscript is well written but the structure needs some improvements in my opinion. Additionally, certain topics (particularly methods) are not explained with enough details.

We're happy to read that reviewer 1 thinks the topic is relevant, and this work "closes links" and is well written. Reviewer 1 has also provided an extremely detailed review. We respond to all comments below.

2.2 Major Comments

2.2.1 1) Improve structure of manuscript

In my opinion, the manuscript lacks a clear structure, as e.g. introduction of data and applied methods are not restricted to the data and methods sections but also appear e.g. in section 6. Furthermore, the partitioning of subtopics in results and discussion (sections 5 and 6) does not seem logical to me. I would suggest the following structure:

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1. Introduction
2. Input and validation data
2.1. Downscaled gridded RCM data (part of current section 2)
2.2. Time-invariant data (DEMs, ice/ocean masks) (part of current section 2)
2.3. River discharge observations (part of current section 6.2)
3. Methods
3.1. Masks and grid cell alignment (current section 3.1)
3.2. Derivation of hydrological quantities (e.g. basins, outlet locations, etc.)
     (see next major comments for more details about the content of this section)
     (current section 3 and part of current section 6.3.1)
4. Product evaluation and assessment
4.1. Main characteristics (current section 5)
4.2. Comparison with previous similar work (current section 6.1)
4.3. Validation/Comparison of product with observational river discharge
     (current section 6.2)
4.4. Product uncertainties (current section 6.3)
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4.5. Remaining sources of freshwater input in fjords (current section 6.4)
5. Technical product description and data/code availability
5.1. Product description (current section 4)
5.2. Data and code availability (current section 7)
6. Conclusions
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We have re-arranged the document into roughly the order that you suggest.

2.2.2 2) Missing parts in method section

The method section should be extended – particularly the part about the derivation of the hydraulic characteristics. Specifically, I miss information about:

- How were (artificial) depressions in the DEM handled? With a filling algorithm?

Correct re filling algorithm. This was not described in the submitted manuscript because the routing and filling algorithms are standard GIS tools. We've added a sentence explaining that sinks are filled so that all water is routed to leave the domain.

- I'm confused about the applied flow direction algorithm. Was a single flow direction (SFD) or a multiple flow direction (MFD) algorithm used? And how were the basins delineated if a MFD algorithm was used (which has a dispersive character)?

We used SFD-8.

Moreover, the method used for assessing the basin uncertainty (section 6.3.1) should be moved to this section. It should include a more detailed discussion of the equation used to compute the hydraulic head and how this equation is applied to derive the sensitivity experiments in the appendix (with various subglacial pressure).

We now only perform subglacial not supraglacial routing. Discussion about sensitivity experiment is now re-written into methods. Results are now discussed in result section, not appendix.

2.2.3 3) Sensitivity of basins delineation to uncertainties in surface elevation and partitioning of surface/subsurface runoff

The evaluation of the Kangerlussuaq / Watson river catchment with river discharge data reveals that the accurate basin delineation is crucial. The sensitivity experiments with a different DEM for the surface and the consideration of subglacial drainage are thus extremely interesting and useful. I wonder if the uncertainty in the basin delineation, which is illustrated in the appendix, could be translated to runoff uncertainties (and be included in the runoff output product). One could for instance compute discharge at all (coastal) outlets for all sensitivity experiments and check the range in obtained runoff. This work would reveal catchments for which runoff quantifications are more (un)certain. It's probably not necessary to include these runoff uncertainty values in the current product but it would be nice upgrade.

We have moved this sensitivity section into results rather than appendix. Unfortunately even these sensitivity experiments do not capture what we believe to be the "true" Watson basin (see Lindbäck et al. (2014) and Lindbäck et al. (2015)). We now manually select those "southern" basins to show that when included the modeled runoff matches the observed.

We now provide subglacially routed water (for $k = 1.0$) rather than supraglacially routed water, as this better reflects reality. We generate subglacial routing for $k \in [0.8, 0.9, and 1.0]$ and use that for the sensitivity experiment. We also created a revision with $k \in [0.70.8, 0.9, 1.0, 1.09]$ (where $k = 1.09$ is effectively surface routing) but the paper became too complicated covering all those edge cases, 0.7 and 1.09 are extreme values, and releasing even 3 product for $k \in [0.8, 0.9, 1.0]$ seems unhelpful to end-users. We opted instead to briefly use the 3 k scenario for the sensitivity experiment, but only discuss results from $k = 1.0$ and release that data.

As for using this simple sensitivity experiment to quantify runoff uncertainty - we are not sure how to do that. In the expanded Uncertainty section we now discuss the complexity of quantitatively estimating runoff uncertainty and how the basin uncertainty is not directly related, and even defining or comparing ice basins between k scenario is difficult. For land runoff, the outlet is fixed. As you suggest we can and do take different k simulations for upstream ice, route to a fixed land outlet, and look at the range of runoff from that outlet. The difference between $k = 0.8$ and $k = 1.0$ is minor - much less than the differences between RCMs, or an RCM and observations. Doing this for ice outlets is significantly more complicated because the basins and outlets change for each k simulation, so it is not clear what should be compared between simulations.

It is a tractable problem to do manually for one or a few outlets. We provide the streams, outlets, and basins for $k \in [0.8, 0.9, 1.0]$, so that users can see possible changes in basin size, but only runoff for $k = 1.0$ to make the end-product simpler to use.

2.3 Minor Comments

2.3.1 Content-related (text)

Page 1 line 10: "contributes an additional ~35% to the ice runoff " → confusingly stated (because the ~35% are referring to the total runoff I guess) → rephrase

We have removed this text and the discussion between ice and land runoff.

P2L26-28: I don't understand to what "satellite basemap imagery" is referring to. To the ocean mask?

This was referring to the background satellite image in the map graphics of the basins for each observational data set. I have removed this text from this section of the document, but keep the "Basemap from Howat et al. (2014); Howat (2017a)" in each figure where the data is used, as per ESSD policy. I disagree with this policy.

P2L29: Mention somewhere here that RACMO only provides runoff for the glaciate area of Greenland

RACMO now includes land runoff, expands back in time to 1958, and both RACMO and MAR forward in time to 2019.

P3L5-7: The runoff downscaling should be explained in more detail (or a reference for the procedure should be provided)

References added.

P3L8: Is it justified to assume that the firn layers in both simulations (MAR and RACMO) are in approximate equilibrium in 1979 (i.e. was there a spin-up performed or when did the simulations start?)

Yes, spin-up occurred prior to results being provided here.

P3L14-17: How are (artificial) inland depression treated that would lead to erroneous inland outlets. Are such depressions apparent in the DEM? And if so, how are they removed?

All depressions are filled until runoff leaves the domain (ice margin for ice runoff, coast for land runoff). This is now clarified in the text.

P3L15: I'm confused by the part "multi-flow direction from eight neighbors". Does it imply that a multiflow direction algorithm with dispersion was used? Or a D8 algorithm (because this algorithm also allows flow from (maximal) eight neighbors).

Multi-flow can come in from 8 neighbors, but only leaves to one. The text has been clarified.

P3L26-27: I'm not sure if I understand this sentence correctly: so land pixels surrounded by ice are set to ice (but their elevation is left unchanged)?

Correct. These local bumps may impact local streams, but should have no other effect because results are reported at the outlet, and the bumps are internal to basins, or if they define basin boundaries they still do even if their classification has changed. Put differently, inland ice streams often terminate (incorrectly, we presume) at nunatuks. We therefore set nunatuks to type 'ice' for the routing so that everything is routed to the ice margin. We then reclassify as 'land' so that the ice basin is not artificially enlarged for the runoff estimates.

We now explicitly point out that streams in the ice domain are merely "representative" of the model streams, but do not likely reflect actual subglacial streams, unlike the land streams which do appear to follow actual streams when compared against satellite imagery.

P4L18-20: I don't understand this part: Is the downscaled gridded runoff data provided on an EPSG:3413 map projection (because I guess the direct output from the RCMs is on a rotated lon/lat-grid)? And the EPSG:3413 projection is based on WGS 84 (and thus an ellipsoid). But some data is provided in a coordinate system based on a sphere (earth spheroid)?

I'm not entirely sure about the internal model grids - some are on a rotated pole lon/lat grid. They are provided to me on a EPSG:3413 projection. In EPSG:3413 1 m^2 is not equal to 1 m^2 in the real world, because of projection errors. We scale the data to account for this scale effect.

P4L22-23: It should be stated in this section that land quantities (e.g. basin polygons and runoff) also include the same quantity from the glaciated part (I assume). So I guess runoff from land contains both runoff from the unglaciated and the glaciated part?

The land runoff only include runoff that originated on the non-ice-covered land. In the data set you saw with the initial submission, land polygons included the

upstream ice area. In the current version we provide both the polygons with the upstream ice area, and the polygons cropped to only the ice-free land area, which is where the RCM land runoff is partitioned.

Including the land basins with upstream (under-ice) included is useful so that a point placed on the ice can easily determine which land basin contains the land outlet.

P5L8-10: Why are the more larger land basins than ice basins? Do the land basins incorporate the ice basins?

Yes - in the first version the land basins incorporated the ice basins, but land runoff was only calculated from the MAR land cells over the ice-free land portion of those basins. Land basins have to include the ice basins at some point in the processing because otherwise the routing algorithm will treat the ice edge as the edge of the domain, route streams there, and place outlets there. Having the land domain include the ice area forces outlets to the ocean boundary.

In this revised version we now crop the land domains to the ice-free portion after the routing algorithm step.

P5L20-22: It should be more clearly stated in this sentence that the 4380 m3 refers to runoff from a single basin.

Sentence removed.

P5L24: I assume the ±30 km3 represent the RCM runoff uncertainty of 15% (this should be clearly stated here). And shouldn't it rather be ±60 km3? And how is this value of 15% derived (is there a reference)? I think it would be useful to mention this uncertainty value already in section 2 (input data).

This sentence removed at the suggestion of reviewers. Annual runoff is not the point of this work. But we did add a sentence about RCM uncertainty when we introduce the data. You are correct that errors of X % should have been ± X, not ± (X/2)

P6L8-11: This sentence does not belong here but rather in section 6.3.1. Furthermore, I find the sentence a bit hard to understand (particularly the last part) – could it be rephrased? It states that flow-path derived from the ArcticDEM generally agree better with satellite images than flow-path derived from BedMachine data, right?

Correct, clarified, and moved.

P6L17: Could you explain the reason why the increase in spatio-temporal resolution increases the signal-to-noise ratio in more detail? And I would include a reference to section 6.3.4 here (so that the reader knows where this strategy is discussed in the manuscript).

More detail added and reference to Mitigation section where it is discussed even more.

P6L18-26: I would move this part to the data section (2).

Done - and in table form.

P7L33-34: "MAR runoff slightly overestimates the GEM observations early in the year, and slightly underestimates the observation late in the year" → this is an interesting finding and probably related to storage of water in the (un-)glaciated area of the basin on intraannual time scales

It may be interesting, but it is no longer in the revised manuscript. Given the additional observational data sets we now show all data from only the last year of each data set. This feature is no longer apparent and is not discussed in the revision.

MAR has been updated (v3.9 to 3.11) and I don't think this interesting artefact is there but not being show. It would show up in the scatter plots if it exists, and I don't see it there (consistently).

P8L3-4: This step-like change in MAR runoff is rather strange. Are you certain that this is not an artefact (e.g. caused by an issue in MAR, the statistical downscaling procedure of runoff or the alignment of the 1 km and the 100 m masks)?

We no longer see this feature. We are using now MAR 3.11 instead of 3.9, which may also be one reason it has disappeared.

P8L27: "slight lag between models signals and the observations." → could this time lag be related to the neglect of routing travel time?

Perhaps. We now focus more on bulk analysis, and use what we think are more appropriate graphics than a time-series, although the time series is still included.

P8L28-29: What is the reason for the significantly higher temporal variability in RACMO? Could this be linked to the different treatment of liquid water retention on bare ice between the RCMs?

In the revised manuscript MAR has been updated from 3.9 to 3.11, and RACMO now includes land runoff. This artefact is no longer present.

P9L1-5: Why was the existing proxy data not used for further model validation (if it exists)?

We have remove this text. This proxy data is a bit far removed from the models to be useful for validating them - turbulent plumes exist and must be modeled. The more likely scenario is that this product released here is used as inputs to those downstream studies. Indeed, that is what has happened in the past, but each study needed to do the work done here, wasting effort.

P9L12: There is no equation 1

There is, but we only referred to it immediately around it, and therefore did not reference the number (1). That has been changed.

P9L15: "because large volumes of runoff usually come from large areas." → I do not understand this part of the sentence, could it be rephrased?

"areas" should have been "basins". Sentence no longer in revised text.

P10L4-5: What is meant by "hydraulic jumps"? I guess not the physical phenomena in hydraulics. If not, this term should be replaced to avoid ambiguity.

That is the precise meaning. This occurs when masks are misaligned. Whenever the Citterio et al. (2013) ice mask transitions from ice to land, if BedMachine ice thickness is not 0, then the system transitions from subglacial to land surface in an abrupt fashion. There will be something unrealistic - either a waterfall or a sink that needs to be filled and may flow out somewhere else. There are many small basins along the coast and ice margin and many of these are realistic, but some may be due to the mask issues described above. This is discussed in more detail in the revised text.

P10L11: This equation (and the corresponding text) should be moved to the method section.

Now rewritten because we only use subglacial routing. This equation and the description of subglacial routing is moved up to the Methods section.

P10L17-19: I find the transition between the previous and this part a bit strange. The part before explains how routing and basin delineation is derived when bed elevation is considered (this part should anyway be explained in the methods section in my opinion) but this section compares basin delineation based on two different surface topographies.

This text has been removed.

P10L30: Can you provide a reference for this value of 15%? Also, this value should already be mentioned in the data description (section 2).

Additional text and reference added. Value introduced in the Methods section. More text in the RCM Uncertainty section.

P11L4-5: Replace "highlighted above" with reference to relevant section. Additionally, are you certain that the step-like changes in RCM runoff originates from the actual RCM simulation (and is not generated by the subsequent postprocessing steps (e.g. downscaling or grid cell alignment).

Text removed.

P11L15: "current limitation" → future RCM simulation will still only capture features and process of certain spatial scales. But do you think that the most crucial scale will be represented in these simulations with higher resolution?

Text removed, but yes, I think when the RCMs are run at 1 km or 100 m they'll do well enough. I think they do already (depending on your use case) after seeing the agreement between RCM and observations here.

P12L4-5: Can you provide a reference that supports this (net storage is approximately zero) assumption?

I cannot, but have made the statement less certain.

P12L21-23: This sentence should be rephrased or removed. Making a prediction about fjord precipitation from the Greenland-wide fraction of land runoff is not reasonable in my opinion.

Removed.

P13L6-7: "perhaps due to temporal directionality" → I don't understand this part

Removed. Time moves in one direction (?) so we cannot cite papers that have not yet been written, or document bugs we have not yet found. We use the GitHub website for this work to note those papers and issue that arise after publication.

P13L7: Is "version of the dataset" meant here?

Both. The document, code, and dataset from that code are all versioned to the same GitHub hash. Parts of this document write itself, using the data it generated. The NetCDF files also have the git hash. We prefer to leave the sentence as-is. The document has a git hash. The dataset has a git-hash for an earlier version of the document. If you compare changes between the dataset has and the current document hash, changes only occur in the text portions of the document (that are exported for the journal to publish) not the code portions of the document that generated the data set, or the code changes don't impact the data values (for example, only changes to metadata or file location).

P13L20: Again, are the stated uncertainty values correct?

We've doubled the errors - they are now ± 15 %, not ± 7.5 %. Not shown, but discussed, when time series plots show the ± 15 % from all three k scenario, differences in runoff from k are « differences between RCMs or between RCM and observations.

2.3.2 Typos, phrasing and stylistic comments

Page 1 line 10: Change "over the time series" to "over time"

Sentence changed.

P2L4-5: I don't understand the meaning of this sentence ("Immediately upstream from…") – isn't it obvious that no submarine melting occurs upstream of the grounding line?

Sentence changed.

P3L12-13: "Each outlet has one upstream basin and each basin has one outlet" → I don't understand the meaning of this sentence, isn't this fact obvious?

Removed.

P3L22: Change "100 m2 pixel" to "10,000 m2 pixel"

Change to "100 m x 100 m pixel"

P4L12-13: "In the case of a small basin," → this sentence is a bit oddly stated – could it be rephrased?

Rephrased.

P4L30-31: I would remove "four per year" (and optionally change "provided as annual NetCDF files" to "provided as four annual NetCDF files").

Done.

P5L6-7: “Runoff ice products. . . ” → oddly stated sentence → rephrase

Rephrased. Also, we now use "discharge" to refer to our product, to keep it distinct from the source of the water, which is runoff from the RCMs.

P5L21: “2012-08-06” → write date out

Sentence removed.

P5L27: “contributes an additional 35% to the ice runoff” → again, I find this a bit confusingly stated (I guess the 35% refer to total liquid runoff?). Maybe better: “contributes 35% to total runoff”

Sentence removed.

P6L5: Maybe change “and additional data products.” to “and is provided with additional data.”

Done.

P6L23: change “results to all observations that we have been able to find that are publicly accessible” to “results to all publicly accessible observations we could find”

Done.

P7L1: change “with high melt or runoff; Basin” to “with high runoff (and associated melt): Basin”

Done.

P7L7: change “include ice to the south of itself” to “include a glaciated area to the south”

Reprhased.

P7L23: “and a without an ice basin does have RCM ice cells” → odd formulation → rephrase (e.g. change “without an ice basin” to “unglaciated”

Rephrased.

P8L11-12: Rephrase sentence, e.g. to “The MAR relative runoff bias ranges from -20% (last day of time series) to +140% (28 July).”

Text removed, but we now discuss biases much more extensively.

P8L25: change “models than the observations” to “models than in the observations”

Text removed.

P8[edit: 9]L7: change “discussed below” to “still discussed in Sect. 6.3.2.”

Done.

P8[edit: 9]L8: change “source uncertainty – the routing model, which exhibits in two different ways: Spatial (basin delineation) and temporal (runoff delay)” to “source of uncertainty – the routing model, which generates both spatial (basin delineation) and temporal (runoff delay) uncertainty”

Done.

P9L10-11: Rephrase to e.g.: "Temporal uncertainty is not systematically addressed in this work but a method to reduce it is discussed in Sect. 6.3.4."

Rephrased.

P10L19-20: Change sentence to e.g. "Results from additional sensitivity experiments (with different input data and hydraulic head computations) are shown in the Appendix."

This section rewritten and moved.

P10L29: rephrase "they do not precisely nor accurately capture reality" to e.g. "they represent reality discretised and simplified."

Done.

P12L11: change "That ice downstream" to "The downstream ice"

Done.

P12L26: Change "are approximately steady state" to "are approximately in steady state"

Done.

P12L30: Replace "GIS-wide ice sheet surface runoff" with "Greenland-wide ice sheet surface runoff". Otherwise, "GIS" is used both for "Greenland Ice Sheet" and "Geographical Information System"

Sentence removed, but we are more careful with our use of GIS.

P13L2: Replace "This work in its entirety is available" with "Output data of this work and part of the discharge observations are available"

Rephrased.

P13L8-9: This sentence is a bit oddly stated. Could you rephrase it?

Paragraph removed.

P13L12-15: This sentence is rather complicated to read and understand. Could you rephrase it?

Paragraph removed.

P13L15: change "differences in needed" to "differences are needed"

Paragraph removed.

P13L21: change "displaying and overall increase in both magnitude and variability" to "an overall increase in both its magnitude and variability"

Sentence removed.

P13L22: change "scale" to "scales"

Done.

2.3.3 Figures and Tables

Figure 1: Change caption to: “Overview showing ice basins (blue), land basins (green) and locations of following map figures (black).”

Figure modified and caption changed.

Figure 2: Change “Sec.” to “Sect.” in caption (also other occurrences)

Figure removed, but we use "Sect." everywhere now.

Figure 3: What is meant with “(this)” in the caption?

Figure removed, but "this" was referring to this product, not 3rd party products.

Figure 4: Maybe the error bars should be removed from this figure to improve readability. Additionally, “(this product based on ArcticDEM basins in Fig. 5)” should be rephrased.

Figure simplified and combined with following figure.

Figure 5: It’s difficult to distinguish between different basins in this plot. Maybe readability could be improved by only plotting the basin’s boundaries (without hatching). Could you also plot the Lindbäck et al. (2015) basin and the one you used to produce the right panel of figure 4?

Figure simplified and combined with previous figure.

Figure 6: A reference to this figure only appears in section 6.3.1, so its number and position should be changed accordingly. The legend is hard to read (it could be moved outside of the map area). Additionally, I would remove the sentence: “Region is zoomed in near Sermeq Kujalleq (Jakobshavn Isbræ).”

Figure removed.

Figure 7: Change “Fig. 10” to “Fig. 10 and 11” and “visible is basin artefact” to “visible is a basin artefact”. Is the “RCM ice” mask showing the mask from RACMO or from MAR (also in the following figures)?

Figure edited to include runoff time series and scatter plot in other panels. RCM mask is now the same for RACMO and MAR.

Figure 8: Use “Fig.” or “figure” consistently.

Done.

Figure 10: Change “Only 2017 shown” to “Only 2017 is shown”

Text changed.

Figure 11: Again, I would remove error bars to increase readability.

Done.

Figure 13: Change “Uncertainty only shown for total MAR runoff, not ice or land components.” to “Uncertainties are only shown for MAR total runoff and not the individual land/ice components.”

Figure removed. Simplified figures now show total runoff, not separating land and ice.

Figure 15: This plot is very hard to read. Again, I would remove the error bars. It also difficult to distinguish MAR from RACMO. Additionally, "MAR" should be removed from the y-axis labelling.

Figures changed.

2.3.4 Supplementary Material

Figure B1: Remove "not zoomed in". Additionally, I would always provide all necessary information in the figure caption about the comparison (experiment setting, margin/coastal outlet). References to other figures implies constant switching between figures. This also applies for the following figures.

This appendix and these figures have been removed and incorporated into a single figure in the main text.

However, we've gone in the opposite direction regarding figure captions, and they are now even briefer and send the reader elsewhere - not to another figure, but to a section of the text. We know this is not ideal but really don't know how else to handle the current situation. There are 10 figures that are nearly identical, all with six panels. Repeating information for each figure seems unnecessary, and given the height of the figures, there may not even be space. Each figure caption would be 10s of lines. We hope you are OK with the modified figure captions referring readers the Methods section where we introduce the six-panel layout in full detail.

2.4 Review of provided dataset

The presented dataset provides, to my knowledge, a unique and new source for high-resolution discharge data for the GrIS and the unglaciated part of Greenland for the present-day climate (1979 – 2017). The dataset seems very useful for downstream application in various field like e.g. hydrology, ecology and oceanography (particularly for fjord systems). The dataset can be accessed via the provided link, is complete and sufficiently supported with metadata and seems to be of good quality.

However, the description of certain processing steps is insufficient in my opinion and should be improved in the manuscript (see point 2 under "Major comments").

We're happy to hear you think the data set is important and well-produced. The manuscript has been improved with your help and we hope it is now a better document in support of the data.

2.5 Minor Issues

I was not able to found units for the Qaanaaq discharge dataset (https://promice.org/PromiceDataPortal/api/download/0f9dc69b-2e3c-43a2-a928-36fbb88d7433/version_01/mel
)

https://doi.org/10.22008/hokkaido/data/meltwater_discharge/qaanaaq works for me now. I'm not sure what the issue was with the URL you used. Regardless, that DOI remains but GEUS has updated things are we are now using more formal software (the Harvard Dataverse) to server our data.

When checking the static data (basin polygons, outlet locations and streams), I found some inland outlet locations near Kangerlussuaq. What is the reason for this?

This was due to a mask issue and exporting an incorrect variable. It has been fixed.

3 Review 2

This study provides high-resolution datasets of Greenland hydrologic outlets, basins, and streams, and a 1979 through 2017 time series of Greenland liquid water runoff for each outlet. This is a timely and important contribution for the Greenland hydrology community and I'm happy to see the paper and the associated datasets to be published. That said, I think some important issues need to be solved before it can be considered for publishing in ESSD.

Timely and important are nice to hear. Your review was helpful and because of it the paper is now improved.

3.1 General Comments

1/ The result section does not highlight the main contribution of this study very well. It includes numerous numbers of basins, outlets, streams, runoff but their importance is not well demonstrated. Furthermore, this section focuses on the total ice and land runoffs which can be easily derived from RCMs and have been well reported in previous studies. I suggest the result section should focus on what we can learn from runoff partitions in different basins, which is the new contribution of this study.

Following your suggestion we have removed the annual and Greenland-wide comparison (we replace it with a monthly and Disko-only, highlighting the improvements in this work relative to Bamber et al. (2018). We have removed the results section at the suggestion of Reviewer 1 and now have a "Product description" section that highlights this new product.

2/ The discussion section is too long and not easy to follow, particularly "6.2 Validation against observations". Most parts of section 6.2 should be removed to the result section. I suggest the authors only highlight the most important implications of their datasets and shorten this section.

This section has been moved and rewritten, but it is not shorter. The previous Results section was 7 pages of text (3 validation, 4 uncertainty). It is now 11.5 pages (6 validation, 4.5 uncertainty, 1 summary). Importantly, we think the new layout may make it more efficient to read - the contents are more clearly broken down by section and if a reader wants to skip the detailed comparison between modeled discharge and observed discharge (after reading one or two and seeing the pattern), they can more easily do so.

3/ It is important to mention that moulins are not identified so stream networks are delineated to continuously flow from inland to ice edge outlets. Therefore, the stream product may not represent the actual hydrological environments where moulins are widely distributed and fragment drainage networks, such as southwest GrIS. In contrast, the stream product may reasonably predict northwest GrIS drainage pattern since no moulins form there. Moreover, the contributing area threshold should be better illustrated since it determines the extent of streams. It may be useful to state

that the derived stream product aims to represent the general meltwater flow pattern rather than the actual spatial distribution of supraglacial rivers and streams.

We now route subglacially, meaning water is assumed to immediately enter the subglacial system. Neglecting some surface flow (likely just a few km citetp:yang_2016_internally) is not likely to impact results because results are reported at the outlet. We also clarify that subglacial streams are model creations and do not represent real streams, although land streams appear to match streams seen in satellite imagery.

4/ The quality of the main figures should be improved. Currently they are not satisfactory for publishing.

Done.

Also, the main point of each figure should be highlighted.

We now refer readers to the section of the text where each figure is discussed. Please see detailed reply to Reviewer 1 about our figure captions, but briefly, given six panels it is not easy to briefly discuss one main point per figure, and often there isn't a specific point. The goal is to introduce data set users to the data, but given the diverse range of possible downstream users, it is difficult to know exactly what their use-case may be and therefore what their focus will be when trying to understand how the modeled discharge compares with observed. Furthermore, with six panels, figure captions will become lengthy, and with the figure repeating 10x, captions would be repetitive. We hope the current choice to limit figure captions and refer readers to the relevant text sections is acceptable.

5/ More previous similar studies should be included. In the paper, the authors only compare their results with Lewis and Smith (2009). However, at least two important similar studies, Andersen et al (2015) and Pitcher et al (2016), should be added as comparison results.

We spent some time searching for papers by Anderson in year 2015 (and other years), that mention Greenland. If you are referring to Andersen et al. (2015), we disagree that is a related paper. That paper (from a colleague at GEUS) addresses the multi-year mean surface mass balance to correct solid ice flux through "gates" at the PROMICE flight line at ~1700 m elevation. It is not about liquid water runoff, or anything at the daily resolution, or hydrologic basins.

Pitcher et al. (2016) is appropriate to cite and we thank the reviewer for reminding us about this paper. We now compare to it in the manuscript, but we are not sure it is "similar". It does address the uncertainty from the k value, which we find to be less uncertain than the uncertainty introduced by multiple RCMs or observations. Beyond that, we do not see any other similarities - Pitcher et al. (2016) focus on one basin, do not provide any geospatial data, any time series of runoff, nor their code.

3.2 Minor Comments

P1 L17, in this paragraph, I think it is necessary to say surface runoff contributes very importantly to Greenland mass balance (along with ice discharge).

Done.

P2 L4, it is not straightforward to understand "liquid runoff form surface melt, condensation, and rainfall".

I'm not sure how to clarify this. We changed it to "liquid runoff from melted ice, rain, and condensation"

P2 L22, why is 100 m ArcticDEM used to do the analysis?

Elsewhere we clarify that when using the 150 m ArcticDEM, streams flow into the wrong fjord. ArcticDEM 100 m had no such artefacts. We did not see the need to use higher resolution ArcticDEM products.

P3 L5, it may be worthy to mention that weathering crust of bare ice layer can store meltwater. Citation is required for this sentence.

We leave this sentence as is. Many things can store water (tundra, cryoconite holes, crevasses etc.) but here we are not listing where storage could occur in reality, only pointing what level of storage are provided by the MAR and RACMO models.

P3 L7, citation is required for this sentence.

Reference added.

P3 L10, it is not common to use the term "hydrologic head elevation".

Changed to "subglacial pressure head", which we do see used in other literature (e.g. Gulley et al. (2014)).

P3 L11, it is unclear how outlets are determined.

Sentence removed. Outlets are provided by the 3rd party GIS tools we used for the routing.

P3 L13, it is unclear what "major streams" means, some specific channel initiation thresholds (i.e. contributing area thresholds) are used to extract streams? It may be useful to call these "major streams" as rivers.

Text has been clarified - we now add "above an upstream contributing area threshold", but keep the word "stream" throughout the document.

P3 L17, why is 1 km2 used as threshold to merge small basins?

It seemed an appropriate balance between too many micro-basins and generating too many unrealistic basins by combining larger basins that should be hydrologically distinct.

P3 L20, "When this value is negative, it indicates submarine (subglacial) discharge", this sentence is not clear.

Sentence has been removed.

P3 L21, this section is too long. I suggest it should be shorten or some parts can be put into SUPP.

We have shortened the section as per your suggestion.

P4 L24, see my general comment, more explanations should be provided for the stream product.

Done.

P5 L8, it is not easy to understand what these numbers mean and why they are important

Paragraph removed.

P5 L15, what does "adjusts" mean here?

This paragraph has been rewritten.

P5 L21, 4380 m3 rather than 4380 m-3.

Paragraph removed.

P5 L23, which basin? also report the similar value in Lewis and Smith (2009).

Paragraph removed.

P5 L26, Mt. Pinatubo eruption, add a citation to support this result.

Paragraph removed.

P5 L27, the land runoff is considerably large. It is useful to further illustrate its meaning.

Paragraph removed.

P6 L7, "Routing with a 5 km DEM is likely to cause some basins and outlets to drain into an incorrect fjord", what is the reason for this?

Changed to "Routing with a 5 km DEM that does not capture small-scale topography". A lower resolution DEM may miss a hill or small mountain range that changes modeled stream patterns.

Fig 2 is not easy to understand. What is the main point of this figure? Perhaps remove it to the SUPP?

It was a graphical depiction of the coverage issues. Removed.

Fig 4, it is not clear why runoff from the Watson River basin plus the two large basins immediately to the south performs better.

This section has been re-written. The reason is likely that the true basin includes the two large basins to the south - that is what is contributes to the observed runoff, so that is what should be included in the model.

Insets are required to show the location of Fig 5 -9 in Greenland.

These figures have been changed but do not include an inset. We invite the reader to refer to Figure 1 for location.

Fig 6 is not easy to follow. What is the meaning to change outlet locations?

The figure has been changed (and is now Figure 2) and the text describing the methods has been clarified and made more central. It is now in the methods section, not in the supplemental material.

Fig 7 is not easy to follow. What is the main point of this figure?

Changed, but still included, and now repeated for each of the observation locations. These figures (now panel a for each location) provide an overview of the observational field site and environment, the basins (land and ice) and the RCM coverage.

Merge Figure B1-B8 into one figure.

Done (now Figure 2).

4 References

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