We thank the editor and reviewers for their constructive critiques of this manuscript.

Original reviewer comments are in blue, sans serif, 10 pt. Helvetica font.

Responses to reviews are in black, serif, 12 pt. Times New Roman font.

Reviewer #1 (Comments for the Author (Required)):

Summary:

The authors combine a suite of glacier and fjord observations to investigate seasonal to inter-annual differences in fjord circulation and glacier behavior for three glaciers in the Uummannaq region of West Greenland. The integrated approach allows the authors to tease-out the effects of differences in the volume and entrainment depth of subglacial discharge on fjord circulation. The authors also use terminus position time series, calving behavior, and force balance maps to explain differences in glacier behavior. Overall, the study is well executed and does a good job weaving together a wide variety of datasets, including in situ and remotely sensed observations and model results.

Major Comments:

1) For the force balance analysis, please include a description of the spatial scales over which gradients in velocity, surface slope, and resistive stresses are calculated and/or the area over which the results are smoothed (if applicable). I suspect that the 100 m-resolution velocity data are fairly noisy and could produce large spikes and dips in the resistive stress terms; however, the force balance maps shown in Fig. 6 look to be smoothed over ~1 km length scales. Obviously the area over which the force balance is calculated will also influence the interpretation of the results so it will be helpful for the reader to know that the calculations are performed over the same spatial scales for all the glaciers. The data also look to be truncated within a few kilometers of the terminus. Is this due to smoothing?

The reviewer raises a good point here, and we’ve added some additional text in the force balance section to clarify it. The force balance results are smoothed over 1 km. This causes slight truncation of the results at the termini in Figure 6. Since the original submission of this manuscript we’ve experimented with different smoothing algorithms, and now the size of the truncated region is much reduced.

Although you say that the modeled and calculated basal drag maps compare well, it looks like they have different spatial resolutions, which makes it difficult for the reader to determine whether they are in agreement based on looking at Fig. 6. Are the force balance calculations performed over the same spatial scales? As a follow-up, does the model calculate basal resistance for Umiamako? If so, the results do not appear to be included in the figure.

This is another good suggestion. Per the reviewer’s recommendation, we have plotted the two basal drag results on the same scale so that comparisons can be made more easily. We have also added the ISSM results for Umiamako and included those. For an easier comparison of the results, figure 7 shows the centerline values of basal drag from both techniques.

2) In the description of plume dynamics at the glaciers, you say that plume observations are consistent with model predictions but that it not obvious in Fig. 2. Based on the number of filled circles in Fig 2, it looks like only 2-5 plumes were observed in 2013. In line 279 you say that plumes at RI were "much less frequent" than the other glaciers in that year even though Fig. 2 suggests 2 plumes were visible at RI and only 3 were visible at KS. Similarly, it seems reasonable to infer that plumes persisted between observation dates denoted by the closely spaced colored circles in Fig. 2 but that it becomes increasingly less likely that a plume was present between observation dates as the time period between observations increases. Therefore, based on the large increase in the identified plumes in 2014, it's hard to believe that the data support the modest increase in plume occurrence suggested by the model. Please clarify this discussion and possibly modify Fig. 2 so that it is easier for the reader to follow your observed-modeled comparison.

We have expanded our discussion of the model results (now Fig. 4), including more in-depth discussion of the limitations of the model and potential explanations for the models deviation from our observations.

Minor Comments:

&#x2022;line 35: What separate document are you referring to here?

This was a copy-editing typo. It has been removed.

&#x2022;Fig. 1: It is really difficult to get a good idea of the spatial variations in velocity from the black contour lines in this figure. It would be helpful to label or color code the contours so that the differences in velocity between glaciers are more pronounced.

We have added first and last contour labels. The addition of color to the contour lines made other elements of the figure too difficult to discern.

&#x2022;lines 149-157: How did you delineate the plumes? You hint at using an automated method (since you manually inspect to differentiate terrestrial and subglacial plumes) but it is not obvious here. If you use an automated procedure, do you apply a spectral reflectance threshold for a specific band or combination of bands?

We now explicitly explain that we manually identified the presence of turbid, surface plumes in the satellite observations.

&#x2022;lines 160-162: Do you apply a lag time to derive estimates of subglacial discharge from surface runoff? The lag time likely varies seasonally due to changes in supraglacial and subglacial storage and the distance between the surface meltwater origin and the terminus. This level of detail is probably outside the scope of the manuscript but it is worth noting here for clarity.

We do not apply a time lag to the RACMO-derived surface runoff. We do not feel that a lag is necessary since we do not consider temporal correspondence between datasets with a resolution finer than ~1 week, nor does a clear basis for selecting a time lag exist. This point is now more clear higher in the section, where we describe integrating the RACMO runoff.

&#x2022;line 168: I recommend changing "Uummannaq fjords region" to "Uummannaq Bay" and "glacier fjords system" to "glacier fjord systems".

We have now clarified the language and use consistent reference to Uummannaq Bay and the different fjords throughout the manuscript.

&#x2022;line 188-192: You say that the TerraSAR-X and Landsat velocity data compare well but I am curious whether you find any systematic biases for the individual glaciers. TerraSAR-X velocities are processed using the GIMP DEM to estimate vertical displacements. For glaciers with little elevation change with respect to the GIMP DEM, the geolocation error for point measurements should be negligible. However, for Umiamako, which you say has thinned significantly within the last decade, you may have a fairly large but systematic geolocation error. For your point measurements the geolocation error could lead to a systematic bias between your TerraSAR-X and Landsat velocities.

This is a great comment. The GIMP DEM is indeed used to process the TerraSAR-X images (courtesy of Ian Joughin). Given the thinning associated with UI (less than 40 m between 2007 and 2014), a horizontal shift of 50 m is possible. However, since all of our results are from 2013-2014, during which time the surface was constant, the relative velocity difference during this time period is likely small.

&#x2022;line 220: Please be more specific than "slightly concentrated towards the bed". Do you run the model separately for each glacier or for the entire region? Presumably the layer thickness would be quite different between glaciers either way.

We now specify the extent to which the model resolution is focused towards the bed, and also that we run the model for the entire domain at once.

&#x2022;line 226-228: You say that some of the TerraSAR-X images were poorly spatially referenced. Did this influence your interpretation of temporal variations in velocity as well?

This was imprecise language on our parts. We now clarify that only the raw TSX scenes needed to be re-referenced, and that the velocity products were all georeferenced as part of the velocity processing workflow.

&#x2022;line 246: Does KI have a sill?

We assume here that the reviewer intends to ask about UI, since we don’t use a KI abbreviation. We now describe the sills within the UI fjord.

&#x2022;line 261: Are you referring to the subglacial cavity that would form as the result of basal crevassing (in-line with the inferred cavity at the Helheim terminus)? Please make it more clear here.

We have reworded this sentence to improve clarity

&#x2022;line 265-266: This sentence is pretty ambiguous. How do you define a "major" melt event? What do you mean by "generally coincident"?

We have clarifies this sentence.

&#x2022;line 330: Do you mean the fjord water is warmer and fresher than the bay water? Please be more clear.

We have clarified this section.

&#x2022;Fig. 3: According to the legend, the bay waters are shown in green but there do not appear to be green symbols or lines in the plot.

Thank you. We have corrected this.

&#x2022;Fig. 6: Label the panels.

Done

&#x2022;lines 510 & 513: Where you describe coupling and correlation between variables, it would be helpful to provide correlation coefficients so that the reader can assess the strength of these correlations.

Good comment. We did produce correlation plots for each combination of variables. However, the temporal resolution of the datasets is fairly scarce (particularly for ice velocity), and the results are not illustrative. We’ve changed the text to reflect a more general relationship between these variable.

Reviewer #2 (Comments for the Author (Required)):

This manuscript provides a synthesis of multiple data types, modeling efforts, and analysis methods for investigating the critically under-observed dynamical differences between neighboring Greenland outlet glaciers. As it stands, the manuscript is lacking in a thorough analysis of the data and a clear communication of the methods and main results. While the authors do present many types of data, the RACMO data product, and model results, the clarity of presentation and comparison is at times poor, and the depth of analysis is at times cursory (I point out these instances below in the major comments). Significant improvements should be made to the rigor of the analysis of the data and data products presented to really dig into the dataset. Significant improvements should be made to clearly and logically present the comparison of the three outlet glacier sites. Additionally, there are minor areas of the manuscript where the clarity of the methods and main results could be improved in both the text and figures (I point out these instances in the minor comments). I urge the authors to remember that glaciologists and oceanographers have different backgrounds. As such, studies that combine the disciplines should make sure that the text and figures are accessible to both parties with writing of high precision and clarity that includes ample variable definitions.

We thank the reviewer for their constructive reviews. We have made significant improvements to the presentation of methods and results, as well as the discussion and implications of our work. We expect that the reviewer will find the new manuscript significantly improved, in part, thanks to these valued comments.

Major Comments:

Figure 1a: The above sea level colormap takes away most relevant subglacial topographic details for this elevation range. I suggest a brown-green color map (suggest demcmap in Matlab) with the color intensity lessened if necessary. Grey contours in Fig. 1a are not labeled with velocity values. Label at least minimum and maximum velocity contour plotted.

We have added labels to select velocity contours, as well as hydrologic catchment boundaries. We have also experimented extensively with alternate colormaps for the topography. However, increasing the contrast between elevation bands serves to obscure some of the other patterns we wish the reader to glean from this figure. The existing coloring allows for the relevant topographic detail to be discerned, especially in concert with panel (b). Thus, we have retained the original colormap.

L122&#x00AC;-126: What is meant by the term "merge". Do you always use the multibeam data over the Dowdeswell et al. (2014) and Morlighem et al. (2014) data? Or do you use an interpolation scheme when datasets overlap? Does "merged" mean the same for both the Dowdeswell et al. (2014) and Morlighem et al. (2014) data? Clarity of methods needed here.

We have clarified what we mean by “merging” in the text in this section.

L144: Why do you not plot the catchment areas on Fig. 1a if glacier catchment geometry is the "first-order constraint on seasonal dynamics" (L575)? Lindb&#x00E4;ck et al. (2015) most recently among many other studies back to Shreve (1972) have shown the glacier hydraulic catchments to be dependent on the hydraulic potential at the bed, which is a function of bed and surface slopes and a flotation fraction. Why was the bed slope not considered in this calculation when bedmap data for this region are available? The above sea level colormap of Fig. 1a does not allow the reader to see how surface and subglacial catchments align with surface and bed topography. Clarity of methods needed here. Argument for not including the hydraulic potential at the bed in the catchment delineation necessary if you chose to present only surface catchments.

We agree with the reviewer and have revised the catchment areas to those defined by the hydropotential of Shreve (1972). Catchment boundaries were similar to those defined by the surface gradients, as expected, and thus our results changed only slightly. We have added catchment boundaries to Fig. 1.

Glacier Velocities and Force Balance Method section: The multiple, at times overlapping, velocity calculation methodologies are difficult to tease apart as written. Suggest adding a supplementary table or figure detailing the images and time periods used for the force balance, ISSM inversion, Fig. 1a contours, and Fig. 5 velocities.

This was a good suggestion. In addition to producing the suggested supplemental table, we have improved the clarity of our methods section.

L279: If RI surface plumes are not visible, how do you know where Qsg enters the fjord along the terminus? Chauch&#x00E9; et al. (2015) provide evidence not mentioned here of transient subsurface plumes with changing locations at RI. Additionally, at some periods of the summer, Chauch&#x00E9; et al. (2015) provide data (observations of turbid jets) for arguing the presence of 2 subsurface plumes at RI coincident in time. Handfuls of papers have noted the dependence on subglacial discharge distribution at the terminus on plume dynamics, melt rates, and fjord circulation (most recently Slater et al., (2015)). Yet, the authors provide no discussion on the error sources inherent in using numerical models with their assumptions (mainly all Qsg routed out of 1 channel) to derive the presence of plumes. See next comment.

We have added additional text to discuss this point.

L295 & Figure 2: Observed presence of surface plumes and model predictions of plume surfacing is not always consistent across the fjords. Differences between the predicted plumes reaching the surface and actual plume observed at surface in satellite imagery is very apparent at RI. Is the plume model over predicting the ability of plumes to surface at RI? Factors of the study that could affect this discrepancy (discharge magnitude, discharge routing through only one outlet at the terminus, model parameterizations and physics not representing the plume system properly) are not discussed. Additionally, the parenthetical (L296 "(open and filled symbols in Figure 2b)") is at odds with Fig. 2b caption as I interpreted the caption (Please see my minor comment on the clarity of this caption below). Model predictions of plume surfacing are the horizontal lines; the rest of the symbols relate to imagery-derived data. Am I reading this figure correctly?

We have improved the discussion of these simulations and the associated figure in accordance with the reviewer suggestions. The text should now be more clear, including an improved discussion of the similarities between the observed and modeled plume occurrence.

Fjord water properties and circulation: Suggest moving this section to before "Subglacial Discharge and its Impact at the terminus" to allow for further discussion (and greater reader understanding) of AW and stratification impacts on the modeled plume scenarios.

We concur with the reviewers suggestion and have rearranged the sections accordingly.

L326: Are the TS properties measures outside the fjords all measurements taken at the mooring in Fig. 1a? Or an average of multiple profiles? If they are multiple profiles, mark where the profiles are located on Fig. 1a. Proximity to glacier front is critical for interpreting T/S profiles of glacially modified waters in glacial fjords (among many papers, Straneo et al. (2011) provides good evidence for Sermilik). Why is this information not presented here? It is a standard in the oceanography field to identify profile locations.

Yes, the “Outside” profiles on the TS diagram all come from near mooring site location. We did not indicate CTD positions on the map figure (Fig 1a), as adding >100 dots would have rendered everything else impossible to see. However, we have added new text to the methods better specifying the locations of the CTD casts. The presentation of the casts together, as averages rather than as profiles, meets our goals of providing a broad brush comparison of how subglacial discharge affects the dynamics of these glaciers and fjords differently.

L353: Fig. 3-Are there no data for the UI fjord? How do you model the plume for this system without any T/S measurements of stratification in the fjord?

T/S measurements were taken in UI fjord during July and Sep 2013. We lack hydrographic data in UI during summer 2014, and use the 2014 RI T/S to initialize the UI model over that time period. We have added a statement to the Fig. 2 caption (now Fig. 4) describing this.

L439-445: Fig. 6- How can readers compare the ISSM inversion and force balance results when the panels have different colormaps, different colorbar ranges, and it is not evident whether both panels show the same area or are even plotted at the same map scale? Fig. 6 needs panel labels (a-e) and consistent x- and y-axis labels in Easting and Northing. Suggest label UI, RI, and KS in one force balance panel and the ISSM panel to guide readers.

We acknowledge that this figure was poorly assembled. We have since fixed it. Figure 6 now includes panel labels, and consistent color and map scales so that comparison is more feasible.

L466: Fig. 7-Fig. 7 needs panel labels (a&#x00AC;-d). Fig. 7 is not cited in the text; inline references to the force balance components should be made from L446-466. The x-axis "distance along flow" does not align with the "Distance along profile" of Fig. 1 in units or magnitude. Suggest change to km in Fig. 7, and mark the terminus positions and every 5 km up-flowline from the terminus for UI, RI, and KS on Fig. 1a and/or plot the centerline with 5 km markings on Fig. 6 panels. Suggest "basal drag" be changes to "basal shear stress" for consistency to how this panel is referred to in the text (ex: L539 among others).

We have now brought Fig. 7 more completely inline with the rest of the manuscript. The panels are labeled and called out in the text. The x axis plots distance from the terminus in km, so that glaciers can be directly compared with each other. We are in fact inverting for basal drag in both our force balance and ISSM inversions and we therefore refer to it consistently thus. The caption specifies that we use the centerlines from figure 1.

L513: "positively correlate with melt production." Careful when using melt and runoff interchangeably, as they have different definitions. Runoff is melt able to move laterally away from the RACMO grid point. You are using the runoff output from RACMO2.3, and not the melt, correct? You have not plotted velocity against runoff (only time series), so how do you observe the positive correlation? If you want to determine "the cause and effect of these interconnected processes" why don't you plot the variables against one another and assess their covariance? Just comparing a time series plot of the variables and giving vague inferences to the relationship between velocity, runoff, and retreat is a weak analysis and barely scratches at the surface of the data and data products you have painstakingly obtained.

We now consistently and accurately refer to melt, runoff and subglacial discharge all distinctly. Regarding correlation, see our response in the main text. In essence, correlation between subglacial discharge and ice velocity is not straightforward, therefore, we have changed our language to be more precise and emphasize that we are not correlating the two timeseries.

L522: If you have the terminus position data for 2013 and 2014, can you calculate a timeseries of tau\_b? If not, can you calculate a relative difference in tau\_b from year to year? When presented without the magnitude of delta tau\_b, the claim that delta tau\_b is driving the velocity response between 2013 and 2014 at RI is a fairly weak argument.

In order to properly assess the impact of changing ice geometry on tau\_b, we would need to make some assumptions about the nature of the thickness change during this time period, because thickness changes can almost entirely offset however changes in length to maintain a steady tau\_b (as described in Howat, I. M., I. Joughin, S. Tulaczyk, and S. Gogineni (2005), Rapid retreat and acceleration of Helheim Glacier, east Greenland, Geophys. Res. Lett., 32, L22502, doi:10.1029/2005GL024737.) However, we have added the citation to this reference as well as Thomas 2004, simply to indicate that our hypothesis is plausible, even though we are unable to confirm it.

L550: Suggest providing the hydropotential gradient calculations for the entire study area if you are to use hydropotential gradients as a result in the discussion sections. See above comment for L144.

The hydropotential gradients are a relatively small part of our study and we present the key data in Fig. 1b, i.e., the glacier geometries. The method is well established at this point and don’t feel that the results are sufficiently provocative that they warrant the space in a journal page.

Overall comment 1: At this iteration of the manuscript, the main conclusions of the study are not well presented. From the conclusion, it appears that you are arguing that geometry (glacier and fjord) is the most important difference between the sites: glacier geometry driving runoff and force balance, and fjord geometry driving exchange with the bay and impact of plume-driven circulation. Thus, I suggest drawing more attention to these differences in the results and discussion sections. Glacier geometry (and its affect on runoff) is touched on in the final paragraph of "Impacts of subglacial discharge on glacier terminus dynamics". Suggest moving this portion of the section up to the beginning. You make the claim that "glacier and fjord geometry provide first-order constraints on seasonal dynamics" (L575); thus, this claim should be highlighted first, with arguments for effects of the additional variables following after.

We have re-organized this section to improve the accessibility of our key findings.

Overall comment 2: Strongly suggest adding a table of the main comparisons you make between UI, RI, and KS in the two results/discussion "Impact of subglacial discharge..." sections to bring attention to the major differences and results of the comparison. Suggested variables for the comparison table for both fjord and terminus dynamics: influence of plume dynamics, T/S, circulation mode, driving stresses, basal shear stresses, hydropotential gradient, summer terminus velocity response, calving behavior, geometry, runoff, etc. This could be a qualitative table (i.e. relative influence of plume dynamics), but it also may be beneficial to list variable magnitudes alongside qualitative effects where possible (i.e. driving stresses).

This is a valuable idea, thank you. We have added this table of results to help the reader through the next sections.

Overall comment 3: For the first read through, it is oftentimes difficult to keep the differences in RI, UI, and KS distinct. Table 1 gives a good way to compare the environmental settings of the sites, and an additional table would be very helpful in the results section. Perhaps it would also be beneficial to consider restructuring of sections and sentences to focus on one site at a time. For example, present the arguments for each site individually first (separate sections) and then compare the sites. Again, for the first (and second!) read through, it is very difficult to keep the sites and their differences/similarities in mind for all the fjord and glacier variables when switching every other sentence between the sites.

We appreciate this challenge and hope that the new results table will ameliorate this difficulty. Unfortunately, we fear that if we split the discussion along glacier lines, we would make it more difficult to make the comparisons among the different glaciers, and it is this comparison that is really at the heart of this paper. We draw on diverse methods to identify differences in the response of neighboring fjords and glaciers to surface melt. Through improvements to the language and the new table of results, we hope an appreciation for these similarities and differences will now be more clear.

Overall comment 4: As you present many data types and methods, suggest providing the clearest communication possible for readers between methods and results/figures. In the methods sections, strongly suggest citing the figures that follow from the methods presented. Example: L180 Glacier velocities. Cite Fig. 5b here so readers connect TerraSAR-X method to near terminus velocities.

Good suggestion. We now weave appropriate figure references throughout the methods, as well as the results.

Minor Comments:

L22: "Surface ice" on glacier, fjord, or both? Clarify for oceanographers. A close read throughout the manuscript could be warranted to identify potential misinterpretations of the text buy the joint oceanographic and glaciologic audience.

Clarified. Thank you.

L35: Typo. Remove "See separate document"

Done

L65-L71. Suggest sentence is too long and could be tightened by just listing the outlets from north to south. Suggest remove "its neighbor to the north/south" clauses.

Clarified and shortened.

L71: Make "These glaciers exhibit..." sentence the first sentence for the paragraph that follows (L74-84). Or add an additional sentence to introduce this paragraph.

We have reorganized this section and transition for clarity and flow.

L78: "...as summarized in Table 1." Incorporate some of the more pertinent numbers from the table into the text. What should the reader take away from Table 1?

Done

L83: Can you add the Moon et al. (2014) differing responses to summer melt and terminus retreat to Table 1 for a sense of how these outlets behave dynamically on a seasonal scale?

We now explicitly describe the seasonal velocities of RI and KS in the text. No information is available for UI in Moon et al (2014).

L126: Parentheses around Morlighem citation not needed.

Removed

L135: What is the scale in the force balance of the UI retreat and thinning?

UI retreated roughly 5 km between 2008-2013, and thinned >100 m between 1985-2013. Since the scope of this paper is on seasonal variations, we aren’t calculating time-evolving force balance components. Our force balance results are calculated using surface elevation and ice velocity measurements of UI that are roughly in the middle of its retreat. There is likely a change in driving stress, lateral drag and basal drag due to the retreat, but the spatial patterns (which are the focus of our figure 6) are not likely to change.

L142: I should think a citation of No&#x00EB;l et al. (2015) needed for downscaled RACMO2.3 product.

Corrected

L153: Are these summertime images? Or is it 86 images over the entire year? How many images do you have from the months where catchment runoff is >0?

We have now clarified this point in the text, emphasizing the summer images.

L155: How do subaerially- and subglacially-derived sediments differ in their appearance in a Landsat image? Suggest supplementary figure of Landsat images with examples of each type.

We now clarify this point in the text.

L176: If the hydrography data are available for 2013, may still be useful for readers to see and be able to assess plots of both years. Perhaps this data could go in the supplement.

We now completely present both the 2013 and 2014 CTD casts, and discuss some of their similarities and differences at greater length.

L179: State up front that you are calculating "near-terminus" or "terminus" velocities with TerraSAR-X not full regional velocity maps (I think that is what you are doing). These first two paragraphs of this section a bit confusing going between the different velocity calculations. This section could be improved with perhaps a figure of the velocity calculations in plan view for the RADARSAT, Landsat, and TerraSAR-X methods. How extensive is the TerraSAR-X imagery coverage for the three sites?

We now more explicitly report in the methods section that we are using these satellite products to identify time series of terminus velocities. Discussion of the entire velocity fields is beyond the scope of this paper.

L180: Typo. Space needed "{copyright, serif}

DLR 2014"

We do not see the typo identified here. No change made.

L182: Comma not needed. "11-day or 22-day repeat interferograms gridded to 100-m horizontal resolution."

Corrected

L204: Parentheses around Morlighem citation not needed.

Corrected

L205: Parentheses around Rignot and Mouginot citation not needed.

Corrected

L249: Commas needed around "respectively".

Corrected

L265: Direct reader to Fig. 2a.

Done

L271-273: The different ratios of low- to high-elevation catchment area would be better visualized with catchment boundaries on Fig. 1a.

We now explicitly direct the reader to panel b of Fig. 1. We do not show the surface elevations in Fig 1a. Instead we focus on ice surface speeds and bed elevations in that figure. We do not believe that adding surface elevations to Fig. 1a is necessary to communicate our paper’s findings.

L300: Change dash to a comma.

Corrected

L302: Change dash to a comma. Or make into a second sentence.

Done

L305: "For shallow fjords such as KS" and UI? What controls the subsurface nature of UI plume depth: stratification or discharge?

We have rerun the UI plume model with revised estimates of subglacial discharge and corrected an issue where the plume model was using an older estimate of the UI grounding line depth. Both KS and UI now produce near-surface plumes for the majority of the summer. Discharge and grounding line depth are the key variables for the equilibrium depth of the outflowing plumes.

L312: "integrated melt or runoff" - Don't you just use the runoff variable from the RACMO2.3 output?

We have clarified this.

L316: Description of Fig. 2b is confusing. Directly state horizontal line is plume presence at surface as predicted by the plume model. Directly state the squares and circles are from imagery at beginning of sentence "Open circles represent...".

Clarified

L326: "(labeled Outside)" suggest change to "(labeled Outside in Fig. 3)"

Done

L331: Give definition for density (&#x03C3; - 1000 kg m-3). Is this potential density?

Clarified

L345: Remote antecedent and a comma splice. Suggest sentence is restructured.

Restructured

L352: "for each system". The UI system is missing on this figure.

Caption corrected

L352: Units need on the T/S plot. Is this absolute salinity? Is this potential temperature?

Corrected

L353: "outside the fjords" is plotted in black.

Corrected

L359: Reference Fig. 4 at end of sentence.

Done

L374: Typo: "up-ford flow"

Corrected

L379: Summer 2013 is not plotted. Time series begins in the fall. Suggest change to "...glaciers from October 2013 to August 2014").

Corrected

L380: Why denote Qsg > 2.5 m3 s-1? On L264, significant discharge is defined as Qsg > 5 m3 s-1. This choice is not explained or justified in the text.

We decided to take the vertical lines off the runoff plot in Fig. 4a, as they did not add much information. There is no strong rationale for “significant discharge” other than identifying when the melt season begins and ends.

L381: Suggest plotting depth of KS fjord at location of mooring on Fig. 4c. What is the depth of RI fjord at the position of the RI mooring?

We now specify these depths, and make them clear on the figure.

L383: Are the minimum and maximum along-fjord velocities plotted at daily resolution?

Also suggest putting units on the Fig. 4 b and c color bars.

Yes, the circles are daily estimates. Units added to colorbar.

L393: Parentheses around citations not needed.

Corrected

L400: Joughin et al. (2008) finds <15% seasonal velocity variability similar to RI.

In Figure 3b of Joughin (2008, “Seasonal Speedup Along the Western Flank of the Greenland Ice Sheet”), it looks like close to the glacier terminus, the seasonal speedup is on the order of 2000 m/y, beyond a minimum speed of 8000 m/yr, thus these 2008 results are inline with the 2012 citation. The reviewer is correct in that the extent of the speedup varies with the distance behind the glacier terminus; we think Joughin 2012 makes this case well, and we retain its citation.

L402: Replace dash with a comma.

Done

L426: Fig. 5 needs a, b, and c panel labels.

Added

L426: These values are plotted in Figs. 2a and 4a as "Qsg". They are plotted here as "runoff". Choose consistent variable name across figures for clarity.

Done. We now refer to all quantities as subglacial discharge.

L427: Typo on left y-axis: "Umi ice velocity"

Corrected

L429: Suggest this interesting change in KS terminus velocity is shown. Suggest broken left y-axis and/or increasing thickness of Fig. 5b panel.

This is a good point, which we hope to expand in a further paper. Since it’s not the focus of the present paper, we have decided to leave the scale bar as is. Unfortunately, a broken axis made the seasonal comparison harder to see, but we have added additional text to better bound the occurrence of this extreme slowdown.

L433: Fig. 6 does not have panel labels.

We have added panel labels.

L436: Fig. 6 does not have panel labels.

We have added panel labels.

L447: Do you mean Fig. 7?

We have clarified that we intend the flowline cross-sections shown in Fig 1.

L475: "warm and salt water mass shared by all sites (Fig. 3)" You have not shown T/S measurements for UI.

Corrected to acknowledge that only data from RI and KS are presented.

L504: Suggest rewording: "...being shallower than 50 m and above the upper depth range of the instrument."

Thank you. Reworded.

L595: Suggest rewording: "Fjord geometry and grounding line depth are likely responsible..."

The wording has been modified for clarity.

L603: "Surface mass balance model-derived runoff estimates". Suggest naming RACMO here or citing Ettema et al. (2009) and/or N&#x00F6;el et al. (2015).

Done

L606: Cite N&#x00F6;el et al. (2015).

Done

L609: Change citation to the published version: Vernon et al. (2013).

Done

L614: Do you also develop a better understanding of the components jointly? Suggest "... components individually and as a system".

Yes, thank you. Amended.

L617: Suggest: "Studies evaluating the impact of sea ice/m&#x00E9;lange on tidewater glaciers are also likely ..."

Corrected

L618: The final sentence feels a bit thrown in at the end. Suggest removing and incorporating this thought into the results section. Relatedly, can you track the presence of sea ice through the winter and summer from the satellite imagery? This data could be plotted in Fig. 4 alongside the fjord velocities similar to as you have plotted the presence of plumes in Fig. 2 alongside the discharge curves.

We have relocated this sentence to the results section.

L847: Update reference.

Done

General comment: Variable unit style (negative exponent vs. dash denominator) is inconsistent across figures and text.

We have consistently adopted negative exponent unit style throughout the manuscript.