Interactive comment on “Linking glacially modified waters to catchment-scale subglacial discharge using autonomous underwater vehicle observations” by L. A. Stevens et al.

Anonymous Referee #1
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General comments
This novel paper presents hydrographic measurements from a fjord in west Greenland, collected partly using an autonomous underwater vehicle. These measurements are unique in their detail and proximity to a tidewater glacier. By comparing to inferred subglacial hydrological pathways, the authors are able to qualitatively match glacially modified water in the near-ice fjord with estimated subglacial discharge channel locations. The paper offers constraints on the pattern of subglacial discharge emerging at the grounding line of the glacier, constraints, which are difficult to obtain but much needed for understanding of submarine melt rates and glacier dynamics. Given the unique nature of the data presented, the clarity of the paper, and the interesting implications for ice-ocean interactions, I believe this paper is suitable for publication in The Cryosphere provided that the (mostly minor) concerns below can be addressed.

Thank you for your detailed and helpful comments on the manuscript. We give a full point-by-point response to each concern and comment below.

Specific comments

Main concerns
One concern lies with the geometry of the plume model used, which I believe is inappropriate here. The authors use a line plume model (Jenkins, 2011), which assumes a uniform distribution of subglacial discharge across the grounding line. In particular, if this line plume has parallel-to-grounding line width w and perpendicular-to-grounding line width h, use of the equations presented in Jenkins 2011 requires h w, as entrainment at the plume sides is neglected. In this paper the subglacial discharge is assumed to emerge through Röthlisberger channels, which have h ≈ w at source. Therefore a point source plume model (e.g. Morton et al., 1956; Carroll et al., 2015; Cowton et al., 2015) would be more appropriate. Indeed use of a point source model will result in greater entrainment which may improve quantitative agreement between the data and plume model. Use of a point source model would also avoid the need for the rather contrived argument (p4599 lines 20-22 and p4600 lines 3-6) needed to match Röthlisberger channel discharge with line plume initial conditions. I don’t expect that use of a point source rather than line plume will change the qualitative results nor the conclusions of the paper, nevertheless I think it is important that a point source plume should be used when point source discharge is assumed.

We agree with the reviewer, and have revised all plume model results in this manuscript using the point source plume model from Jenkins (2011) modified to a half-conical plume. As the point source plume model uses inputs of subglacial discharge, we no longer need to estimate subglacial conduit surface area and subglacial flux. As such, we have removed the paragraph in section 4.3 on estimating channel mouth size and geometry, and the lines in Table 4 on estimated channel mouth surface area. As the reviewer expected, we found no significant qualitative difference between the results of the point source and line source plume models in relation to the conclusions of our paper. We present the revised model results throughout the text: most changes occur in Section 4.3, Table 4, and Fig. 5 c, d. The main difference between the line and point source plume results is a final plume temperature and salinity for both D1 and D2 scenarios that is closer in T/S space to the GMW1 and GMW2 (Table 4; Fig. 5 c, d).

My second main concern is that a little more discussion could be allocated to the region between D1 and D2, where the authors indicate there is "little to no subglacial discharge". It would, for example, only take 0.02 m2/s of discharge distributed across the grounding line between D1 and D2 (3 km) to account for 50% of the total dis- charge from the glacier. Given that the fjord is quite weakly stratified at depth even this small discharge might lead to significant submarine melt, e.g. Sciascia et al., (2013). I understand that it is probably impossible to say much more on this issue from your data but if this paper wishes to provide constraints on subglacial hydrology for submarine melt modelling then I think a slightly expanded discussion along these lines should be included. The line in the abstract on this point might also be scaled back (maybe remove "only" from line 13?)

Our data do not allow us to directly ascertain whether there is distributed discharge along the grounding line between D1 and D2. Indeed, as pointed out by the reviewer, a small amount of discharge distributed across this ~3km distance could account for a large portion of the total available subglacial discharge we calculated. However, our observations of two main types of glacially modified waters localized in space do support our finding that the subglacial drainage system near the grounding line is channelized over the time of our field campaign, which results in the bulk of the subglacial meltwater discharge occurring as point sources. Of course, this may not be the case outside of peak melt season, when subglacial discharge rates across the terminus may be insufficient to create (or sustain) a channelized drainage system. Indeed we hope that future work will allow us to explore these transitions in more detail. We have removed "only" from the line in the abstract, and have worked to scale back and qualify times in the text when discussing the D1 and D2 as primary locations of subglacial discharge. In accordance with a comment from Reviewer #2, we have qualified the last paragraph of the discussion section 5.3 "Observational constraints for modeling the heterogeneous near-ice environment".

Lastly the discussion relating plume theory to the differing properties of GMW1 and GMW2 (p4601 line 27 – p4602 line 23) appears rather confused and needs correcting. Firstly, in p4602 line 9 the authors state that an increase in subglacial discharge leads to "a decrease in the fraction of subglacial discharge in the plume". I don't think this is true (and indeed this statement appears at odds with similar statements in p4596 line 16 and p4602 line 21). According to Straneo and Cenedese (2015) Eq. 8, for a line source plume volume flux scales with the initial buoyancy flux B raised to the power 1/3. Subglacial discharge itself scales with B. Therefore the fraction of sub- glacial discharge in the plume scales with B/B1/3 = B2/3, meaning that the fraction of subglacial discharge in the plume increases as subglacial discharge is increased. So although it is true that large subglacial discharges drive higher entrainment fluxes, the increase in entrainment (B1/3) is not as large as the increase in subglacial discharge (B). This observation also affects p4602 lines 15-16 and lines 17-18. Furthermore the contrast in properties between plume and ambient also scales with B2/3 (Straneo and Cenedese (2015), Eq. 8, expression for g') therefore plume temperature is decreased as subglacial discharge increases. Finally the authors state that "Greater discharge at D1 . . . results in GMW that is closer in θ and S to IIW". According to Table 2, it is GMW2, which has properties closer to IIW. Therefore in general this section of the discussion is rather contradictory and needs rethinking. Note that each of the scalings referred to above are the same for a point source plume.

We agree with the reviewer that the discussion relating plume theory to the identified glacially modified waters is poorly presented. We have revised the statements on the fraction of subglacial discharge in the plume with citations to Straneo and Cenedese (2015). Yes, GMW2 does have properties closer to IIW. We have rethought our interpretation of GMW1 and GMW2 to produce a less contradictory discussion of the data. Please see third paragraph of section 5.1 for changes.

Minor concerns/comments

P4593 line 24 – I don't quite follow why setting the flotation fraction to 1 gives the maximum catchment area. I don't think this is a general result. If this is a result you have obtained only for this catchment by varying the flotation fraction then perhaps you could insert an additional sentence to clarify this.

Yes, this is not a general result, but rather what we found for the specific region of our study. We have modified this sentence to clarify this is a site-specific result: "The flotation fraction was set to f_w = 1 (basal water pressures are equal to ice overburden pressure), which resulted in the maximum catchment area possible based on basal hydraulic gradients in this region."

P4593 line 28 and other relevant places – Based on Fig 7a, there are only 3 RACMO 2.3 grid cells in the catchment (though I appreciate that cells outside the catchment will have some effect due to the interpolation). This must presumably reduce confidence in the values of catchment runoff that you use in the plume model. I understand that this is a limitation of the RACMO dataset and therefore there is not much that can be done, but I think the rather large RACMO grid spacing vs catchment size should be acknowledged somewhere, either here or for example in p4598 lines 7-10.

Yes, the small areal extent of the Sarqardliup sermia catchment does mean there are a low number of RACMO grid points within and around the catchment. While the RACMO data products are published without error calculations, we can certainly alert the reader and acknowledge the limitations and potential sources of error inherent in using these data products. We have acknowledged this in the first location the reviewer mentioned (Section 3.2) by adding the sentence: "Catchment runoff values obtained from this method are based on a low number of RACMO2.3 grid cells within and bordering the catchment area due to the small size of the catchment and the low resolution of the RACMO2.3 grid."

P4595 line 28 – why is it you can assume the bulk of the entrainment was of waters at $\sigma\theta = 26 - 26.5$ kg/m3? What about waters with $\sigma\theta = 25.5 - 26$ kg/m3?

We assume that subglacial discharge feeding the plumes emerges at grounding line depths of the D1 and D2 discharge locations (Tables 3 and 4), and that the bulk of the entrainment is of waters that are between this grounding line depth and the lower depth range of the two GMW identified (60 m for GMW1, and 70 m for GMW2 (Table 2)). The ambient fjord waters at these depths are at maximum salinity of 32 PSU, thus we will extend the range of densities to $\sigma_0 = 25.5 - 26.5$ kg/m3 as the reviewer suggests.

P4599 line 4 – "depth-integrated" is not appropriate here. It is true that you can integrate the plume equations in Jenkins (2011) to get the solution, but "depth-integrated" here implies some sort of vertical averaging, which is not what has been done.

Have removed "depth-integrated".

P4600 line 19 – At the prescribed subglacial discharges I believe that submarine melting will have a very small effect on the plume properties (try running the plume model without any submarine melting). If instead we changed the submarine melt parameterization to produce more melting, this would make the *plume more buoyant and increase the discrepancy between the plume model depth and the data. Therefore I don't think an incorrect submarine melt parameterization can explain the discrepancy between model and data.*

Have removed the mention of an incorrect submarine melt parameterization. Sentence now reads, "First, the plume model may have an incorrect entrainment parameterization."

P4600 line 20 – if you use a point source plume then subglacial flux will no longer be a function of channel surface area, removing this possible source of error. But I agree that the subglacial discharge flux could be incorrect. One possible reason for this which I don't think came across in the paper is temporal variability in the subglacial discharge flux over the survey period – either diurnally or from day to day – might this help to explain the rather vertically smeared signal in turbidity you see in Fig 5c?

With the use of a point source plume model, we have removed the subglacial flux as a source of error, though the error in the RACMO-derived subglacial discharge estimates remains (Sentence changed to: "Second, the estimated subglacial discharge could be incorrect."). As our observations span a time of the summer melt season that lacks discharge peaks greater than the standard deviation of RACMO discharge estimations (Fig. 7b, Table 3), we do not investigate the day-to-day variability in discharge estimations over the survey period. The vertically smeared signal in turbidity seen in Fig. 5c could be related to subglacial discharge flux variability resulting in changes in neutral buoyancy height of the plume. Alternatively, the vertical extension of high turbidity above and below the GMW may be related to portions of the plume that have lost their temperature signature but maintain a high particulate matter (ex: shedding of neutrally buoyant eddies). Furthermore, the vertical extension of high turbidity below the GMW may be related to sediment rain out (Mugford and Dowdeswell, 2011).

Technical comments

P4586 line 17 – I think this sentence could be better written to make it clear exactly what is "serving as a mechanism. . ."

Added "…and this entrainment driven by plumes serves as a mechanism" to increase clarity of sentence.

P4586 line 20 – I find "higher entrainment" a bit ambiguous. I think it would be better to make a statement to the effect that plumes with larger initial discharges entrain a greater volume of water or set up stronger circulation (Carroll et al., 2015).

First half of the sentence changed to: "Plume theory and models combined with melt rate parameterizations suggest that higher subglacial discharge rates entrain a greater volume of ambient fjord waters that leads to higher submarine melt rates (Jenkins, 1999, 2011; Sciascia et al., 2013; Xu et al., 2013; Carroll et al., 2015)".

P4586 line 28 – suggest rewording to "largely unknown characterization of subglacial discharge" as this then includes hydrology, which is brought up in the following sentence.

Reworded to "largely unknown characterization of subglacial discharge".

P4588 line 24 – Am I mistaken that there are in fact three LBL transponders shown in Fig. 3 rather than two as described here? Might this sentence belong better in the previous paragraph (e.g. lines 15-16)?

Yes, there are three LBL transponders shown in Fig. 3. Have moved this information in this sentence into the previous paragraph, which now reads: "At depth, REMUS navigates by acoustically ranging to a network of three moored Low Frequency (LF 10 kHz) Long BaseLine (LBL) transponders (Fig. 3)."

P4590 line 6 – typo – "and" should be "an"

Typo corrected.

P4590 line 20 – insert comma after "depth sounder"

Comma added after "depth sounder".

P4591 section 3.1 – might this subtitle be changed to something more appropriate? The section appears to discuss fjord bathymetry, subglacial topography, and behaviour of the glacier in recent decades.

Subtitle changed to "Fjord bathymetry, subglacial topography, and historical terminus positions."

P4591 line 26 – I believe this should be Fig. 2b rather than 2a

Figure reference changes to Fig. 2b.

P4593 line 3 – I believe this should be Table 3 rather than 4.

Changed reference to Table 3.

P4593 line 9 – need a space before "g"

Added a space.

P4595 line 28 – "than" should be "then"

Typo corrected.

P4598 line 26 – suggest adding "width" to the list of plume properties for completeness.

Plume width added to the list of plume properties.

P4599 line 25 – I think "cross-sectional area" rather than "surface area" is more correct.

Changed to "cross-sectional area".

P4599 line 29 – I couldn't see where the range in conduit size has come from – does it arise from a range in subglacial discharge?

Yes, the range in conduit size, S, comes from the range in Q_{sq} estimated for each catchment. Have changed sentence to read: "The range in average daily catchment runoff during the field expedition (Table 3), Q_{sg}, results in a conduit size, S, of 37–92 m^2 for D1 and 9–21 m^2 for the D2."

P4599 line 29 – p4600 line 3 – By using a Röthlisberger channel and the results of Slater et al., 2015, you have already assumed a semi-circular channel so I believe this sentence would belong better at the start of the paragraph which begins in p4599 line 23.

Have moved this sentence to the start of the paragraph.

P4600 line 8 – typo – "that" should be "than"

Typo corrected.

P4601 line 25 – it would be interesting, if you have the data, to know what the discharge through D1 was at this time.

The data are forthcoming in a manuscript by Mankoff et al. (submitted, JGR).

P4602 line 13 – suggest inserting "qualitatively" before "consistent" as you have done in the abstract and conclusion.

Inserted "qualitatively" here.

P4602 lines 24-26 – don't need two "additions"

Removed the second "additional."

P4603 line 26 – it's not GMW1 which enters the fjord, rather it is subglacial discharge from D1 which enters the fjord and subsequently becomes GMW1 after mixing, melting etc. So I suggest changing "GMW1" to "discharge from D1".

Changed "GMW1" to "discharge from D1" and in the same sentence, "GMW2" to "discharge from D2".

P4604 line 8 – typo – need "of" after "couple"

Typo corrected.

P4604 line 24 – need to correct the spelling of variability

Spelling corrected.

P4604 line 28 – I think one of the original plume papers (e.g. Morton et al 1956) would be a better reference for variability in plume neutral buoyancy.

Reference changed to Morton et al. (1956).

P4605 line 10 – "amount" should be "amounts"

Typo corrected.

P4605 lines 14-19 – this sentence doesn't read correctly – does it need "While" at the start?

Yes. Added "While" to the beginning.

P4605 line 21 – don't need two citations to the same paper in one sentence.

Removed second citation.

Table 1 – need to correct spelling of "mission".

Spelling corrected.

Table 4 – I presume "Plume θ" and "Plume S" refer to values at the neutral buoyancy depth – could this be clarified in the table? Is volume flux used anywhere in the text? If not it might be worth removing.

Yes, these are the values at the neutral buoyancy depth of the plume. Table variables clarified and volume flux variable removed.

Figure 1 – the red box in the plot is labelled Fig. 2 – I assume this is meant to be Fig. 3.

Figure has been edited. The red box label has been changed to Fig. 3.

Figure 3 – 4th line of caption: "line" should be "lines"

Typo corrected.

Figure 7 – might the outer fjord CTD markers be a different color than green? They are quite hard to see at present.

Figure has been edited. The outer fjord CTD markers have been changed to black.

Literature cited

Carroll, D., D. A. Sutherland, E. L. Shroyer, J. D. Nash, G. A. Catania, and L. A. Stearns, 2015: Modeling turbulent subglacial meltwater plumes: Implications for fjord-scale buoyancy-driven circulation. Journal of Physical Oceanography, 45 (8), 2169–2185.

Cowton, T., D. Slater, A. Sole, D. Goldberg, and P. Nienow, 2015: Modeling the impact of glacial runoff on fjord circulation and submarine melt rate using a new subgrid-scale parameterization for glacial plumes. Journal of Geophysical Research: Oceans, 120 (2), 796–812.

Jenkins, A., 2011: Convection-driven melting near the grounding lines of ice shelves and tidewater glaciers. Journal of Physical Oceanography, 41 (12), 2279–2294.
Morton, B., G. Taylor, and J. Turner, 1956: Turbulent gravitational convection from maintained and instantaneous sources. Proceedings of the Royal Society of London Series a-Mathematical and Physical Sciences, 234 (1196), 1–23.

Slater, D. A., P. W. Nienow, T. R. Cowton, D. N. Goldberg, and A. J. Sole, 2015: Effect of near-terminus subglacial hydrology on tidewater glacier submarine melt rates. Geophysical Research Letters, 42.

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Mugford and Dowdeswell, 2011: Modeling glacial meltwater plume dynamics and sedimentation in high-latitude fjords. Journal of Geophysical Research, (116), F01023, doi:10.1029/2020F001735.