Point-by-point response to editor and reviewer concerns by corresponding author: E. De Andrés

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tc-2019-264:

Surface emergence of glacial plumes determined by fjord stratification

Eva De Andrés, Donald A. Slater, Fiamma Straneo, Jaime Otero, Sarah Das, and Francisco Navarro

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Dear anonymous Reviewer #1,

On behalf of all authors, I would like to thank you for your detailed and constructive comments. In the following you can find a point-by-point response to your comments. We feel that your insights helped improve the manuscript and we hope that all your concerns have been answered to your satisfaction. We would also like to refer you to the responses to the other reviewer for more improvements and changes to the manuscript.

SPECIFIC COMMENTS / AUTHOR'S ANSWERS

1. I missed a discussion of the effect of the choice of entrainment coefficient. Quite a large range of values are used in literature, and I suspect that this parameter might have a strong impact on NBD/MHD. The value used by the authors is not unreasonable, and I do not suggest that an extensive sensitivity analysis is necessary. However, their choice of 0.09 should be justified, and it should at least be discussed how another choice of entrainment parameter might influence the results.

Thank you for raising this essential point. We in fact find that the NBD and MHD are relatively insensitive to the value of the entrainment coefficient (Fig. R1). Allowing the entrainment coefficient to take values from $\alpha = 0.07$ to 0.12, modeled NBD ranged from $z = -21$ to -29 m in 2012, and from $z = -13$ to -17 m in 2013. For any given value of the entrainment coefficient, NBD is deeper in 2012 than in 2013. For the same range of entrainment coefficient values,

modeled MHD ranged from z = -4 to 0 m in 2012, while it remained at z = 0 m in 2013. In general, higher values of the entrainment coefficient leads to a reduced plume vertical extent because the greater entrainment of deep ambient waters makes the plume denser. We chose α = 0.09 for modeling the plume in the two years, because: 1) that value is within the range empirically obtained for geophysical fluid processes (Carazzo et al., 2008), 2) it is the mean of the two values (0.08 and 0.1) used in previous studies of Saqqarleq Fjord (Stevens et al., 2016; Mankoff et al., 2016), and 3) it provided results that fairly accurately predicted the observed jet depths and plume properties. According to your suggestion, we have added these points and this discussion to section 2.3.1 and L329-334.

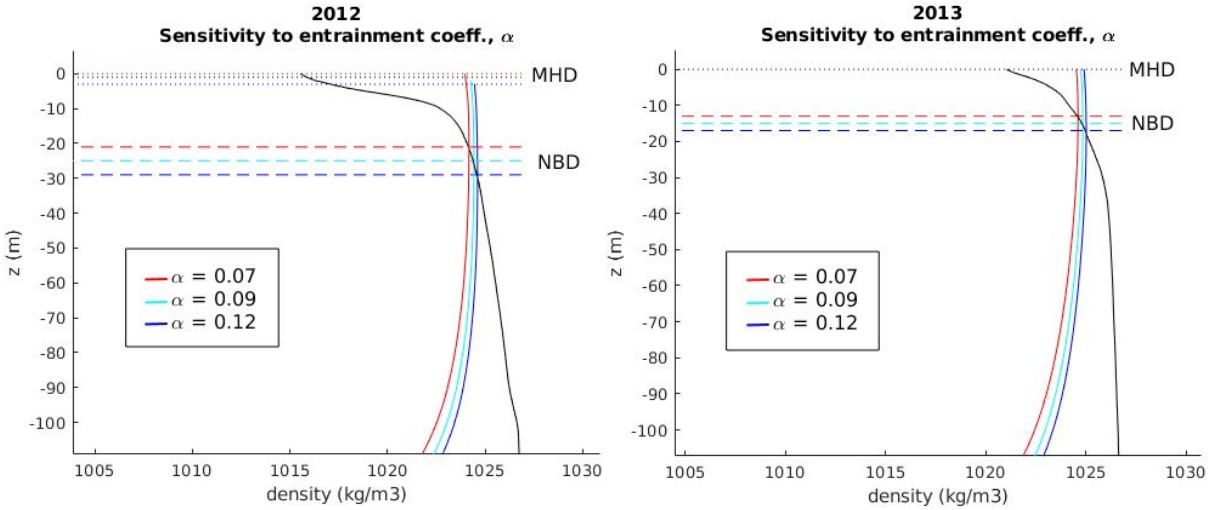

Figure R1. Sensitivity of modeled characteristic plume heights (neutral buoyancy depth, NBD, and maximum height depth, MHD) to the value of the entrainment coefficient (α), in 2012 (left) and 2013 (right). The black continuous line is the observed density profile averaged from all CTD data taken in Saqqarleq Fjord (except those dropped inside the plume). Coloured continuous lines are modeled plume density while coloured dashed and dotted lines represent modeled NBD and MHD, respectively. The subglacial runoff is held constant at the values used in the main paper (Q_{sg} = 101.7 m^3/s in 2012 and Q_{sg} = 101.9 m^3/s in 2013).

2. Section 3.1.1. is very short despite the fact that it provides the key observation motivating the study. It should be extended to provide some additional information, directly or through references: Is there more evidence beside these three photographs for the presence/absence of a plume?

Unfortunately, we cannot further classify the presence or absence of a plume during the field campaigns from satellite imagery. The available images during or immediately around the field campaigns are Landsat 7 or 8 images on July 14 & August 6, 2012 and July 24 & August 2,

2013. None of these images allow us to say anything about the plume, either because of clouds, the small scale of the plume relative to the resolution of the imagery, or stripes in the images (e.g. Fig. R2). We could look at other time periods, but we would rather keep the focus of the paper on the field campaigns when we have concurrent oceanographic observations.

We have other serendipitous photographs of the plume presence/absence taken during fieldwork, but these are no better than those shown in Fig. 4 of the paper. However, we have rearranged and included new photographs in Fig. 4 to facilitate the plume observation. We know from the field surveys that there was no plume for the duration of the 2012 campaign and that there was a continuous surfacing plume in 2013. We hope the reviewer will find the new existing photos and these statements sufficient evidence for the presence/absence of a plume.

Even though we have not been able to add significant further evidence on the presence/absence of a plume, we have included more explicit and descriptive statements on the presence/absence of the plume, and better described the appearance of the plume in 2013.

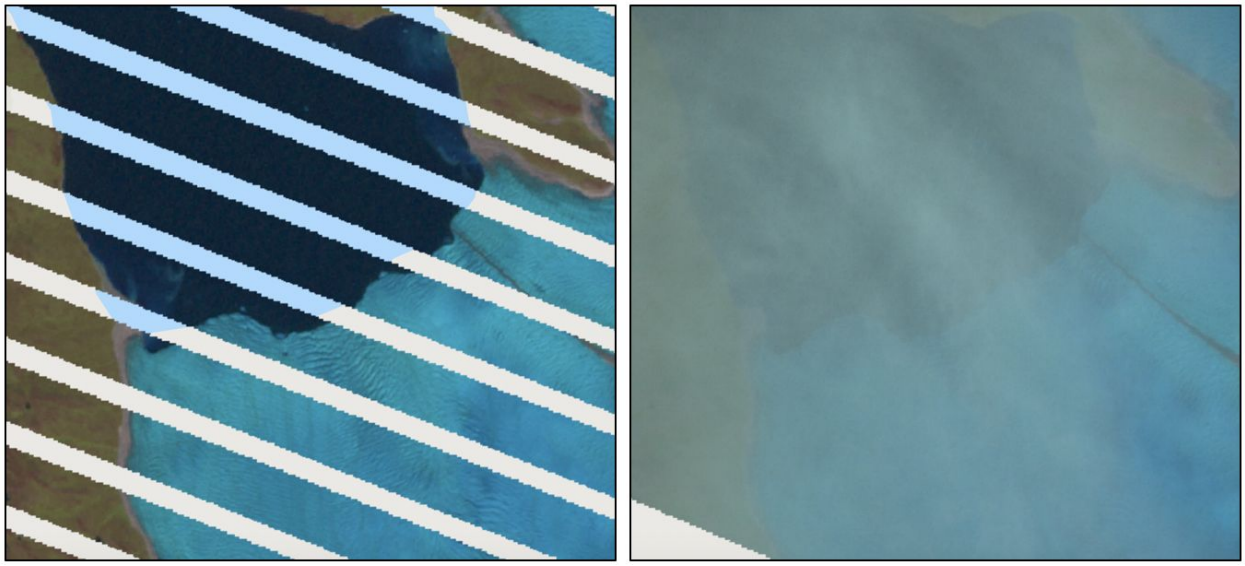

Figure R2. Satellite imagery of SF/SS on July 14, 2012 (left) and August 6, 2012 (right).

Approximately how far did the plume extend along the glacier and into the fjord when it was observed?

In 2013, the plume extended approximately 200 m parallel to the glacier front and 300 m into the fjord (Mankoff et al, 2016). This has been inserted into the text, L171-172.

Was the glacier terminus located in approximately the same position during 2012 and 2013?

Yes. Multiple terminus position traces for 2012 and 2013 may be found in Fig. 2b of Stevens et al. (2016). This statement has been added to the text, L67-69.

Without a spatial scale in Figure 4 it is also somewhat difficult for a reader to compare the three photographs - please add some sort of reference to the extent of the plume, and ideally to the scale of the images.

We agree that it was difficult to compare the three photographs in Fig.4. Therefore, we have modified Fig. 4 (also included are new photographs) to facilitate the plume comparison between the two years, 2012 and 2013. We have also described the approximate size of the plume surface extent in lines 171-172 and in the caption of the figure.

3. Line 275: Please elaborate on how agreement between model and observation is improved in this study compared to these previous studies.

Thank you for this suggestion. In our study, the modeled plume properties at NBD fall within the range of the observed water properties. In both Stevens et al. (2016) and Mankoff et al. (2016), modeled plume properties were consistently too fresh and therefore too light. We attribute our improved model to observation agreement to our use of a line plume model of appropriate width (Jackson et al., 2017), which leads to greater entrainment of denser deep fjord waters than would be achieved with the half-cone plume models used by Stevens et al. (2016) and Mankoff et al. (2016). These points have been added to the manuscript, lines 311-317.

4. Section 2.3, which describes the methodology wrt. the plume model, should be made clearer. Please provide appropriate references to easily direct the reader to the exact set of equations and parameter values used here, and state explicitly what exactly is meant by "running the plume model" in Section 2.3.2. Are the equations numerically integrated using the observed T/S profiles as boundary conditions? On Lines 99, 107 and 111 the authors refer to Slater (2016) for a description of the model: however, this paper only explicitly contains the plume equations for a half-conical plume, and furthermore discusses both numerical and analytical solutions. Please also specify the plume water properties with which their plume model was initialized.

We have not described the plume model in great detail in our paper because it has become a very standard tool in the related literature (e.g. Slater et al., 2015, 2016, 2017; Jenkins, 2011; Stevens et al., 2016; Mankoff et al., 2016; Jackson et al., 2017; Carroll et al., 2016), but we have now improved the description by following all of the reviewer's suggestions. The equations are indeed numerically integrated from the grounding line to the MHD or fjord surface, whichever comes first, using the observed T/S profiles as boundary conditions (now stated on lines 118-120). Slater et al. (2016) do use the numerical line plume model also used in our paper, but you are right that the equations are not explicitly stated; these can instead be found in Jenkins

(2011) as now stated on lines 103 and 110. The plume model is initialised with the flux of subglacial discharge (the magnitude of which is described later in the paper), with this discharge assumed to have zero salinity and to be at the pressure melting point. These details have been added on line 119-120.

5. It is stated on Lines 123-125 that Qsg and W are both varied at set intervals. Is it not it the combined value Qsg/W that impacts the model, or do these quantities also come into play individually in some other manner? Please clarify.

Yes, the reviewer is correct that the only dynamically-relevant quantity is Qsg/W - these quantities do not appear individually anywhere in the line plume model. This is why we are able to plot characteristic heights versus Qsg/W in Fig. 9 of the article. Our reason for describing Qsg and W separately in some places in the paper is to make contact with the real system (in which clearly Qsg has a value in m^3/s defined by surface melting of the glacier and W is set by the dynamics of subglacial channels). In doing so we hope to make the paper more accessible. Therefore we would rather leave these lines as they are, but we have added a clarification that the only quantity that enters the model is Qsg/W (lines 135-136).

6. As far as I understand, it is an assumption of the line plume model that the discharge is distributed over a wide enough area that the side interfaces of the plume can be neglected. Using the line plume model with a 10-m width (Line 124) seems likely to violate this assumption. Please justify the use of a line plume model with W as low as10 m, or acknowledge this as a limitation of the study.

We thank the reviewer for raising this important point. We agree and have increased the minimum channel width we consider from 10 to 50 m (line 135). In fact this doesn't change any of our plots because the new range of Qsg/W ratios still covers the ranges that were previously plotted (e.g. on Fig. 9).

7. Section 1: This is an excellent introduction section!

Thank you.

8. The first paragraph of Section 4.2. should be moved to Results, e.g. merged into Section 3.1.2.

Following your recommendation, the first paragraph of section 4.2 has been merged into section 3.1.2 (L178-186).

9. Section 2.3.3. should also briefly state how the integrated melt rate is calculated.

The submarine melt rate, m, at a point on the calving front within the plume is calculated using the plume model. This does not vary within the width (W) of the plume, and therefore the integrated melt rate is defined as

$$M = W \int_{z=-150}^{z=-NBD} m(z)dz$$

We have added this information on L159-161 and in the new Eq. (3).

10. Line 284: Please elaborate on, or provide a reference for, why there is a characteristic "plume distance" that might be approximately equal to the grounding line depth.

This section describes how significant mixing occurs as waters from the plume flow horizontally away from the glacier close to the fjord surface, but including reference to a specific 'plume distance' here is unnecessary and so we have changed this to 'a few hundred metres' (line 324).

11. Line 360-362: This is not new, please remove or modify to reflect that this is in line with previous studies.

Modified as suggested (lines 405-407).

12. Line 371-377: The modelling experiments seem to suggest that the maximum plume height in July 2012 was only a few meters below the surface - I think this should be acknowledged when the reduced nutrient fluxes to the photic zone are discussed.

Agreed - the model is suggesting that while the plume was not observed to surface in July 2012, it must have been very close to surfacing. We have now acknowledged in lines 417-419 and 424-425 that the impact on vertical nutrient fluxes in SF in July 2012 may have been small.

13. Line 406: Please explain here or around Line 235 why the jet might be more diffuse in 2012 (reduced stratification?).

On L211-214, we have included the potential explanation and the observed values of N^2 at the jet depth in each year as support for the hypothesis.

14. The map figure (Figure 1) should be made visually clearer and perhaps used to clarify the description of the study area (see the comment below). It is a little difficult to differentiate between ocean, lakes and land, as well as between sea-ice covered water and glaciers, in the current figure. If possible, I suggest superimposing coastlines in a distinct color.

Great advice. We have changed Figure 1 according to the reviewer's suggestions.

15. It was not entirely clear to me from the figure and the text how far the area referred to as SF in fact extends. The text can be read as meaning that SF extends all the way to the sill near the opening to JI, but the placement of the SF and TF labels in Figure 1 makes it a little unclear e.g. whether the area between the two sills belongs to SF, TF or to the unlabelled fjord to the right. There also seems to be a discrepancy between the length of SF between line 57 (35 km) and line 61 (15 km). Please clarify.

Apologies that the text was rather unclear here. We consider SF to extend up to the 70 m deep sill ~15 km from SS; beyond this is TF extending up to the junction with JI. We have reworded the first paragraph of section 2 and revised Fig. 1 in line with the reviewer's comment above to clarify these points.

TECHNICAL CORRECTIONS

There are many inconsistencies in the use of past vs present tense throughout the manuscript - I recommend sticking to one or the other through each section.

Thank you for this suggestion - we have reviewed the tenses and now consistently use the past tense in sections describing the observations and the present tense when describing the plume model and model results (e.g. see changes on line 77, 96, 187-192, 271-272, 301, 304, 321, etc).

Abstract: "Ice Sheet" should be "ice sheet" or "Greenland Ice Sheet".

Changed as suggested.

Line 61: Missing space after "(2019)."

Added.

Line 106: "tidewater face" should be "glacier face"?

Yes, changed as suggested.

Section 2.3.3: Please specify that the N2 used in the scaling is the mean stratification of the upper layer (not the entire water column) if that is the case.

Yes, N^2 in the scalings is the stratification of the top layer. This has been clarified in the text (lines 153-154).

Line 170: Should 0.11 s-2 be 0.011 s-2? It should also be clarified that this refers to the *maximum* of the mean N2 profile if that is the case.

Changed and clarified as suggested.

Line 192: This sentence should be revised for clarity.

Revised.

Line 204: please specify: "while it did in July 2013" if that is the case.

Done.

Line 217-218: Exponentials should be in superscript.

Now corrected.

Line 219: Unclear what is meant by this sentence ("Our goal is to identify the model parameters.."). What model parameters are you referring to exactly? Please clarify.

Essentially we are considering whether the plume model can reproduce the observations, and have changed the sentence accordingly.

Line 231: "sigma_theta" should be "theta"?

Corrected.

Line 235: "Properties" should be replaced with e.g. "waters".

Replaced.

Line 247: For clarity, please replace "stratification" with "N2" or " B-V frequency squared", etc..

Replaced with N^2.

Line 283: Missing "the" before "plume model".

Added.

Apparent discrepancy between Line 310 on one hand and Line 248/Figure 11/Table 1 on the other. The latter say the exponent is 0.24, the former says it is 0.26. Please correct or elaborate.

The correct value is 0.24. We have corrected the mistake on line 365 (old L310) - thank you for spotting this.

Line 329: There already seems to be strong variability. Do you mean "increased" variability?

In this paragraph we mean to highlight variability within a year (i.e. seasonal variability), whereas previously in the paper we have mainly contrasted 2012 and 2013 (i.e. interannual variability, now explicitly included in L376)

Line 368: The meaning of "reaching the fjord surface less" is not clear. Please revise this sentence and clarify.

Revised: “plumes may reach the fjord surface less **often** over the coming century”

Line 380: Should be plural: "act as atmospheric CO2 sinks".

Corrected.

Figure 2: Please revise the colour scheme used in this figure. It is currently difficult to distinguish the black points from the background in the figure on the right. I suspect the figure on the left would be challenging to colorblind readers. I also recommend labelling the subfigures a and b. A scale bar should also be added to this figure.

Figure 2 has been changed following all of your suggestions.

Figure 5 caption, first line: should "density" be "potential density"?

Yes. Corrected.

Figure 5: I cannot see the horizontal dashed lines referred to in the label.

We have now added the horizontal dashed lines.

Figure 7ab: I assume the contours are isolines of sigma theta? They should be explained in the figure caption.

Explanation added to the caption.

Figure 8: I would suggest replacing the x-axis units (DOY) with dates, as it would make for easier comparison with the rest of the manuscript. Ctot and C1 in the figure caption should be formatted with subscripts.

We would prefer to keep the DOY, as it makes our study easily comparable to the two previous studies in SF (Mankoff et al., 2016; Stevens et al., 2016), but we have added a conversion of DOY to the field survey dates in the figure caption. We have also formatted the subscripts.

Figure 9: Please specify exactly what the vertical shaded regions represent (one and two standard deviations? standard deviations for the two different years?).

The regions represent one standard deviation of the subglacial runoff during the 5 day period preceding the velocity measurements in the fjord. This has been clarified in the figure caption.

Figure 11a: Please add some information indicating the location of the surface and the top layer here - e.g. as horizontal lines in the plot or as a second y-axis showing "depth below surface".

The interface between the top and bottom layer is 100 m below the surface and is therefore outside of the y-axis limits shown in Fig. 11a (all tested plumes reach significantly higher than this interface). We have added the fjord surface level to Fig. 11a.