Response to reviews: “Submesoscale streamers exchange water on the north wall of the Gulf Stream”,

Thank you to both reviewers for your time spent on the manuscript and your helpful comments. The largest change is to break Fig 3 into two and to add panel c to the new Figure 4. This is a more formal calculation based on reviewer 2’s suggestion that we integrate the velocities, and we think makes for a nice addition to the data presented. This does decrease the total transport reported, but it still brackets the large-scale estimates. There are other small edits in response to your suggestions and our own re-read of the text.

Reviewer #1 Evaluations:
Science Category: Science Category 2
Presentation Category: Presentation Category B
Key Points: Yes

Reviewer #1 (Comments to Author (shown to authors):

Science Category 2

The authors present results from a unique set of measurements that provides insight in submesoscale dynamics at front.

The authors need to explain and expand their discussion of the streamers move up through the water columns and evident in Figure 2. I have a hard time seeing this in the figures 2d. Also, how do the authors know that it is not a spatial effect since the waters in the region have differential velocities compared to the float's velocity?

This was poorly explained in the first version. The new paragraph 151 is hopefully more clear. Essentially, this water has clear origins at the north wall as evidenced by its intermediate T/S characteristics. The water in this T/S class is found to the north is continuously connected to the wall, allowing us to infer that it originates there. It is however, true that we cannot determine how the water moved off the north wall: it either flakes off along the wall, or it converges at the separation point and is “squirted” off the wall. We somewhat expect the latter is what happens, but we cannot say for sure. In either case, the water must move up in the water column as it moves north.

Presentation Category B

I found the figures very complicated, items that are not required and hard to see the different coloured lines in the figures and if the figures are reduced in size, they will be illegible. Possible changes for the different figures are provided below.

Admittedly, the figures are somewhat complicated, but the three-dimensional feature is

complicated. Many different presentation techniques were tried, and we feel this is relatively effective. Note that Fig 3 has been split (though a new panel has been added to Fig 4).

The figure captions are too long and repeat a lot of the material in the text of the manuscript.

The reviewers suggested edits below were largely taken, and we made some cuts ourselves.

Figure 1. I am note sure you really need the pictures and they clutter the pictures. If you remove them, then the SST insert and colour bar could be inside the main figure and reduce the amount of wasted space. Make the R/V Atlantis cross-section labels in white as I cannot not see them easily.

We tried white - it is definitely worse. The Atlantis track is repeated in Fig 3, so hopefully this isn't an impediment in following the paper.

Figure 2. Remove the sentence about the water's temperature and salinity along an isopycnal since it stated in the text. The green contour are sometimes hard to see especially in the darker colours.

Agreed, thanks, we cut this.

Figure 3. The authors need to explain what variable is used in their histograms. The sentences, 'The warm salty GS...', 'The water near the surface...', 'Deeper, there is', 'This used data ...','A water parcel...', and 'This is meant...' should be removed.

- Fixed caption to better describe histogram
- Caption edited with most of these suggestions.

Figure 4. The green contours should be done in white. The figure caption is too long but I don't see obvious sentences, except the last one, that could be deleted. Also, I am not sure what is shaded in f. What are the coordinates in d and e but I don't think it is along- and cross-stream directions.

Again, disagree about white, at least on my monitor.

We modified the caption of f) to clarify: "f) shows the speeds (min/max is shaded, and mean is the line) of the particle clouds in time, and shows that the intrusion water (magenta) accelerates relative to the streamer water (green)."

The caption has been modified to indicate that x,y in d/e are lateral dimensions in the model.

Minor Comments:

l. 46: What is the symbol before '5 km'?

Typo; fixed

l. 134: '(figure 2d)' should be place after the '40 m deep'.

40 m is the depth of the streamer in Figs 2a and b.

l. 139. Why is the feature is called an intrusion whereas the other features are streamers. I think the dynamics for both features

It seems the comments were cut off here? If the reviewer was trying to say that the dynamics of the "streamer" and the intrusion are one and the same, I agree. But there does appear to be some asymmetry at play.

Hopefully there were not further more extensive comments that were given that we did not incorporate. Thank you very much for your suggested changes, and we hope they have improved the paper.

Reviewer #2 Evaluations:
Science Category: Science Category 2
Presentation Category: Presentation Category A
Key Points: Yes

Reviewer #2 (Comments to Author (shown to authors):

Summary:
The manuscript presents the analysis of a series of high-resolution vertical sections collected in a Lagrangian reference frame across the northern boundary of the Gulf stream. These provided an in-situ quantification of the northward flux of salt associated with the streamers detaching from the main current. Some of the observations are further tested by analyzing analogous structures from high-resolution numerical simulations.

Overall, I find the structure of the paper clear, the in-situ dataset used in the analysis innovative and the scientific results relevant. Moreover, I have particularly appreciated the link to the dataset and code used for the analysis which the authors provide in their acknowledgements.

I think that some aspects of the manuscript still need to be addressed to improve its clarity, but they should represent only small changes to its overall structure. Therefore, I recommend it for publication in GRL with minor modifications.

Specific comments:

- Salt flux computation:
*Although four sections are discussed and presented in figure 2, the induced fluxes are derived on lines 130-131 only from indicative values of velocity, depth and width of the streamer, obtaining an indicative flux of 0.5 *10^6 m^3 s^-1.*
Why not providing the values computed by integrating the observed velocities over the area section occupied by the identified streamer-water for the four sections? Are such estimates consistent among the four sections? Do their variability show any trend that might suggest further mixing with the northern waters as the streamer detrain from the Gulf Stream?

*The results from the in-situ observations are then used to derive an estimate of the overall impact of these structures on the cross-stream salt flux (lines 193-202). In doing so the authors introduce a range of values for the transport associated with each streamer (0.2 to 0.5*10^6 m^3 s^-1). However, in the manuscript it is not explained how the lower limit was obtained. More details should be provided.*

Both these points are good. As we are sure the reviewer can appreciate, calculating fluxes from scanty observations is challenging, and we are not sure we believe the flux can be quantified much better than the back of the envelope in the first version (We certainly do not believe we can get pass-by-pass mixing estimates!). However, we have re-arranged the figures a bit, and added a panel c to Figure 4 that calculates the transport relative to the transport in the attached fluid, to arrive at a negative transport anomaly in the streamer (~0.2 to 0.25 Sv). This is less than the upper limit of the previous text, but again, there is no claim made that these numbers are more than rough estimates. These paragraphs have been mildly altered (L131ff and L 200ff) to describe the new calculation. The point doesn't change; the streamers are a mechanism to move salt away from the Gulf Stream at rates consistent with large-scale budgets.

Finally, I think that the author should be more explicit in addressing/commenting the possible limitations of their overall salt flux estimate, since it is derived from in-situ observations of a single streamer. How much should the fluxes and salt content be expected to vary from streamer to streamer? (Maybe the model results could be used to address this question).

That would be a good result to have, but would probably require experiments that were dedicated to arriving at these numbers. Our goal here is to show the process and argue that it is of the right order to close the large scale budgets. Doing this in the model is a big job, and beyond our scope, but is part of a paper by Gula and colleagues.

- In-situ vs remote sensing streamers
In describing figure 1 (lines 95-101), the authors say that streamers are visible in the region of observation from sea-surface temperature. However, from the in-situ sections in figure 2, the streamer water do not seem to extend to the surface. I think a brief discussion/explanation should be included.

We added at the end of the paragraph describing the cross-sections: “The surface signature of the streamers (as seen in \fref{fig:SatOverviewSectD}b) can be also seen in these cross-sections as saltier ``surface'' water (\fref{fig:SalDFirstStreamer}a-d)), but the T/S characteristics are not as distinct because of cooling by the atmosphere.”

- Lack of definitions and details:
In general I find that there are many details and definition that are missing in the manuscript and that if provided would ease its reading and understanding:
1) For instance, providing the TS characteristics of Gulf Stream and northern water masses, as well as the indicative sigma level associated with the front axis would make it easier to identify its location in both figures 1 and 2.

Sorry, not clear on this suggestion. The intermediate water is contoured in Fig 2, and water the the south (negative Y) is Gulf Stream. A T/S plot is shown in Fig 3. One could argue about the order of presentation, but hopefully its all clear when looked at as a whole. Figure 1 is SST, which shows the imprint of the streamers in the surface water, but is a bit ambiguous to link to the deeper water masses given all the cooling and evaporation from the atmospheric forcing.

2) How have the green line in figure 3 defining the TS characteristics of streamers been defined? Given that streamer waters result from the mixing of GS and northern water, I would have expected their TS characteristics to be in between those two (and not for instance to extend down to 14 degrees).

The green box was defined by hand, to surround the “mixed” mode of water and going down to 14 degrees was simply to draw a line. There is no data in the green box down at 14 degrees.

3) Why does the analysis focused on the sigma level 26.25?

This was just chosen as a qualitatively useful isopycnal in the middle of the range 26.125 (or so) and 26.5. This is now noted in the text

4) What was the depth of the Lagrangian buoy deployed at the front axis (this would also clarify the depth at which the passive tracer was released - lines 71-72).

The float was deployed in the mixed layer (i.e. its density set its lateral position) as specified L54.

- Vorticity computation (lines 70-71):
Which type of differencing was used to compute it?
Since the ship tracks are not exactly perpendicular to the float trajectory (proxy for the front axis), how big is the error in the vorticity estimate due to the fact that du/dy it is not exactly the along-track variation of across-track velocity?

The two-d estimates are just a first difference. The error due to the angle error with y will

tend to underestimate the vorticity by maybe 15% (cos(30) or so). Relative to the float it is even less than this (lines are almost perpendicular to the front in the float's frame. Note that the three-d version (Fig 3) don't suffer from this potential bias.

- Virtual Lagrangian particles (lines 87-94):
1 s seems really a small time-step for such type of integration. Were the hourly model velocities interpolated in time for the Lagrangian advection?

Yes, but the calculation is a quick one once we have the model data in hand. The text was modified to indicate that time was also linearly interpolated (L105)

- Potential vorticity in figures 2 and 3:
In the caption of figure 2 and in the manuscript the author refers to potential vorticity. However the figure label sates potential vorticity anomaly instead. That should be clarified.
On lines 108-109, the authors say that the front is characterized by high potential vorticity. However from the sections in figure 2, high values of PV vorticity occur only at depth (below the mixed layer) and are not associated with the front axis at the surface. That sentence should be rephrased.

Captions fixed.

The other point is perhaps us not being clear again about what feature we are looking at in the discussion? We changed the key sentence to include the depth range: "some cross sections show lateral interleaving of salinity north of the front deeper than 70 m (\fref{fig:SalDFirstStreamer}a)"

- Caption figure 3:
In the caption, it seems that the term "front" is used to describe the width of the high PV water defining the streamer.
This should be modified for consistency with the rest of the manuscript, in which "front" refers to the boundary between GS and Northern waters, instead.

The word "front" was misleading here, and has been replaced by "streamer T/S class".

- Lines 156-158:
Is there any evidence for a different acceleration of the two water masses from the in-situ observations in figure 2? I think a brief comment should be added.

This is the detrainment calculation ~L142ff

Repetition:
Lines 180-181 and 185-186 from the same paragraph are almost identical. I suggest to remove one of the two.

Thanks, this paragraph mild reworded to remove the repetition.

Thank you again for your helpful comments.