Ocean Sci. Discuss., 12, C1385–C1403, 2016
www.ocean-sci-discuss.net/12/C1385/2016/

Ocean Science
Discussions

Interactive comment on "Volume transport and mixing of the Faroe Bank Channel overflow from one year of moored measurements" *by* J. E. Ullgren et al.

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Received and published: 4 January 2016

Dear Editor,

We appreciate the time and effort invested by both reviewers. In particular, we would like to thank Reviewer 1 for his or her kind words and thorough review, and we thank Reviewer 2 for raising valid and important questions about whether both our mooring arrays cover the entire extent of the plume and whether the overflow splits before arriving at section S so that the array might have missed one branch of the overflow. We respond to these concerns in detail in our revised Discussion section (see quotes below), including a new figure which shows the position of mooring array S in relation

to earlier reported locations of the bifurcation. We are of the opinion that the addition of an in-depth discussion of the bifurcation location, and the other changes we have made following the reviewers' suggestions, have significantly improved our manuscript.

As pointed out by Reviewer 2, there was a discrepancy between Figure 3 and the values cited in the text. During the revision process, we found that the figure in question had been made erroneously using an older version of the post-processed data set, and that there was an inconsistency in the 'percent good' threshold used. In the final version of the manuscript, we ensure that all the figures, cited numbers and calculations consistently use the most recent version of post-processing and cut-off limits.

Here follows a point-by-point reply to the reviewers' comments, marked by "C". Our replies are marked by "R".

Response to Reviewer #1.

SPECIFIC COMMENTS

C. 2316-18: "mean diffusivities" - probably "time mean vertical diffusivities"?

R. We changed this to "time mean vertical diffusivities".

C. 2316-22ff: If you want to describe the whole GSR overflow you also need to mention the IFR and WTR overflows.

R. We now mention the IFR and WTR overflows in the first paragraph of the Introduction: "Cold waters that flow from intermediate levels in the Nordic Seas into the North Atlantic must cross the shallow Greenland–Scotland Ridge. Overflows occur both across the Iceland–Faroe Ridge and the Wyville Thomson Ridge, but the densest overflow plumes are those passing through the deepest gaps in the Greenland–Scotland Ridge, namely the Denmark Strait and the Faroese Channels (Hansen and Østerhus, 2000; Saunders, 2001)."

C. 2317-1: Maybe more precise to say "the thermohaline composition of North Atlantic Deep Water"

R. We have changed this as suggested.

C. 2317-22: "similar results" - please specify results

R. We now specify (1) that the temperature thresholds gives similar values of height and width of the overflow plume as the other method, and (2) that the volume transport determined in this way, 1.6 Sv, agrees well with that of Hansen and Østerhus.

C. 2317-23: It would be helpful to mention instruments and averaging time span for the Hansen and Østerhus volume transport.

R. We now mention that the Hansen and Østerhus volume transport estimate is based on 10-year measurements (1995-2005) from moored ADCPs and temperature sensors.

C. 2319-5f: Continued moored measurements in the FBC since 95! This is one of the few global long term moored time series of (part) of the deep MOC, so I wouldn't say most of the work on the FBC has been based on hydrographic sections.

R. We have rewritten this paragraph to highlight the importance of the long-term time series from the FBC sill mooring: "The overflow transport is continuously monitored by moored instruments at the sill section, where at least one upward-looking ADCP mooring in the centre of the channel has been maintained since 1995 (see Hansen and Østerhus, 2007). Other work on the FBC overflow has often been based on hydrographic sections, sometimes combined with current measurements. Some studies have used e.g. short-term moorings (Dooley and Meincke, 1981) or gliders (Beaird et al., 2013). Aside from the FBC sill mooring (Hansen and Østerhus, 2007), which provides one of the world's few long-term moored time series of a deep branch of the meridional overturning circulation, mooring measurements of the FBC overflow of more than a few months' duration are rare (e.g. Saunders, 1990; Darelius et al., 2011). In particular, few long-term observations have been made downstream of the sill, and

none of them cover the whole vertical and lateral extent of the plume."

C. 2323-22: Do uncertainties become too large when going beyond January 17 with less instruments? My earlier understanding was that some ADCPs stopped sampling after about 3 months, so this statement doesn't make sense.

R. Three high-frequency ADCPs (one at C2, one at M1, and one at S3) were deployed to study mixing processes at the plume interface, and stopped sampling after 3 months because they were set to sample at a higher temporal resolution. Their stopping early was planned and did not affect the total vertical range of velocity measurements at those moorings. In our mooring design, we made sure to cover the same depth range using lower frequency ADCPs sampling for the entire duration of the deployment. However, the only ADCP at mooring C3 stopped recording (unplanned) after 234 days, and no vertical velocity profiles are available from that mooring after 17 January. One might attempt to construct full-depth velocities from C3 based on point measurements from the RCM-7 at 25 mab or using a relationship with velocities at C2, but considering that C3 has the highest velocities in the C-array we think that "making up" velocity records here would introduce an undesirable level of uncertainty in the transport estimates. We prefer to report volume transports only for the period during which we have velocity profiles from the ADCP at C3. This is now explained in appendix B. We have also changed these lines (originally 2323-22) to specify that we are considering the period with "complete vertical coverage of velocity of the cold overflow plume at all C-array moorings", with a reference to appendix B.

C. 2323-24: How do you calculate the standard deviation of daily values?

R. As described (and cross-referenced to) in Appendix B, transport time series at each section were calculated using daily gridded fields. The standard deviation for each section is calculated over this daily time series. We have rephrased this to "standard deviation of the daily resolution time series".

C. 2325-5: Any idea what could cause a seasonal cycle in core plume temperatures?

Hansen & Osterhus (2007) show a seasonal cycle in temperature with a minimum in July/August.

R. Hansen and Østerhus 2007 suggest that the seasonality in temperature is linked to the seasonality in volume flux: large outflow would draw the water from deeper depth and that would give lower temperatures. However, our volume transports do not show a clear seasonal cycle, probably because of the limited record length – as discussed in our revised Discussion section: “Saunders (1990) found no seasonality of plume thickness, temperature or velocity in year-long measurements, but Hansen and Østerhus (2007) showed a seasonal variation of kinematic overflow at the sill of about 10% of the mean over a 10-year period. The relatively low amplitude of the seasonal cycle compared to shorter-term variations means it only becomes apparent in data sets covering several years. The seasonal variability in the outflow has been linked to seasonality in the barotropic northward flow of Atlantic Water in the Faroe–Shetland Channel (Lake and Lundberg, 2006). Darelius et al. (2015) showed that variability in volume flux on shorter time scales is linked to the local barotropic forcing.”

C. 2325-14: How do you know about semidiurnal variability in volume transport if daily averages were calculated? Why not calculate hourly transport and show the tidal peaks in the spectra?

R. This phrase about the tidal band variability is a remnant from our earlier calculations when we used hourly transports. We now remove this statement. There is indeed tidal variability, but this is not important for the focus of the present study. In our earlier analysis we used hourly fields, but then decided for using daily averages, mainly to remove the tidal variability. We retain this presentation.

C.2325-15: What is the difference between plume transport and total transport?

R. This initially referred to transport of overflow water and including modified overflow water, respectively, but since the maxima and minima of both occur in the same months we have changed this sentence to refer simply to "volume transport".

C.2325-9: Either swap Figs. 7 and 8 or mention Fig. 7 already when talking about volume transports in Section 4. I’d prefer the latter.

R. We amended the following in Section 4, first paragraph, with cross-reference to Fig 7: “Time series of daily transport across sections C and S are shown in Fig 7.”

C. 2328-18ff: The heat budget section needs a little more scrutiny. For the heat budget to work you need to assume steady state, this needs to be discussed. I do not understand the averaging process, what do you mean by "daily averaged data in 30 day windows to sufficiently average over the 3-5 day mesoscale variability"? Equations 1-4 need to express that quantities are averaged. Shouldn’t the horizontal eddy temperature fluxes enter equations 3 and 4? What are the errors on volume transports Q3 and Q6 through upper isotherms?

R. We now mention that a steady state is assumed and also cite key references (Hogg et al., 1982, Whitehead and Worthington, 1982) which introduced the method and which also discuss various points raised by the reviewer. The budget equations are solved using 30-day averaged parameters (Q, T, etc.) which are daily data. The calculations are done using 30-day width windows which are moved by 1-day. That is at each day we have output from the budget calculation. The 30-day window is long enough to average over several 3-5 day scale oscillations. We clarified these points in the revised version using: “The budget was calculated using averages over 30-day long segments of the daily time series which are moved by 1 day throughout the record (that is, 1-day moving 30-day width windows). The 30-day window sufficiently averages over the 3-5 day mesoscale variability. While the steady-state assumption is questionable, the variability in section volume transports is mainly contained in time scales less than 10 days (see Fig. 10 for the variance-preserving transport spectra). Budget calculations using time average windows from 5 days to 3 months with 5-day increments (not shown) confirm that the results are not sensitive to the choice of the 30-day window after approximately 15-25 days. We thus expect the budget calculations to be fairly representative of the steady state, and to yield a more accurate estimate of entrainment rates compared to analysis of snapshots of hydrography and currents from single cruises."

We now express in Eq 1-4 that we are using averaged quantities. Under Eq 2, we amended: "... (array S), and all quantities are time averaged over a suitable time window...." Here it suffices to mention a suitable window, because in the next paragraph we justify the choice of 30 day.

We repeated the budget calculations using time average windows from 5 days to 3 months with 5-day increments. These calculations (not shown) confirm that the results are not sensitive to the choice of the 30-day window after approximately 15-25 days.

Q3 is solved as the residual of Eq(1) and is bounded by the upper layer. The only uncertainties are thus those associated with the volume flux measurements at Sections C and S, and with the bottom Ekman transport. The drag coefficient used in the Ekman transport is from turbulence measuments and, on average, is well constrained. Q6 is the residual of Eq(2) after Q3 is obtained. Error in Q3 is thus carried over to Q6.

C. 2329-10: The equation relating turbulent vertical heat flux is defined locally, not in an area-averaged sense as implied here. The quantity you get from the heat budget, averaged over the volume/area defined by the mooring arrays, is the heat flux. Calculating an average vertical diffusivity can be misleading as K and dT/dz may not be independent from each other.

R. Agreed. We clarified this point under Eq.(4), by revising as: "..The diffusive heat flux, averaged over volume and time, is approximated as FH = -Kz (dT/dz) L W, where Kz is the average vertical diffusivity, L is the horizontal along-path separation between the arrays, W is the average plume width defined by the given isotherm, and dT/dz is the average vertical temperature gradient. This approximation is typically invoked in the budget calculations, and is valid if the local values of diffusivity and the vertical temperature gradient are not correlated. Our resulting estimates of average vertical diffusivity should thus be interpreted with this caveat in mind."

C. 2329-26: I am not convinced yet that the Ekman transport can be neglected in the heat budget. The plume has lateral temperature gradients as you move from the edge to the center, i.e. the zero degree core of the plume doesn't cover its whole width. Also, at Section S there is no zero degree water left. You mention transverse circulation later in the discussion (2332-23), could this not be at play here?

R: The part of the control volume emanating from the channel is constrained within the channel such that the bottom Ekman transport is retained within the plume. The export only happens on the open slope. Given an estimate of 0.3 Sv for QEK, and T close to nil (say as large as 1°C), the term QEKTbottom is very small compared to the other advective temperature flux terms. The possible effect of transverse circulation should be included in the estimated average entrainment/detrainment results.

C. 2330-7: K is usually termed vertical diffusivity, not eddy diffusivity.

R. Agreed. We replaced eddy diffusivity with vertical diffusivity throughout.

C.2333-1: What could be the important implications?

R. We now mention the implications in the Discussion section: "Simple estimates of the contribution of the FBC overflow to the global thermohaline circulation assume doubling of the volume transport due to entrainment. Our observations suggest that despite substantial entrainment, volume transport does not increase significantly because of detrainment, implying that the impact of FBC overflow on the global thermohaline circulation can be less than what has usually been assumed."

C.2333-18: Again, K should be vertical diffusivity.

R. Changed to "vertical diffusivity".

C.2334-7: Great to see that horizontal eddy fluxes are consistent with baroclinic instabilities generating the eddies! R. We agree. No action taken.

C.2335-12: Repeating the heat budget results here seems a little out of place where one expects a more general closing statement.

R. Agreed. We move the inferred mean vertical diffusivities 3 sentences up, before “The present study provides…”, and improved our final paragraph so that it now mentions the important implications discussed above. Our new closing paragraph reads: “The contribution of the FBC overflow (and the dense Greenland–Scotland Ridge overflows in general) to the North Atlantic Deep Water is sometimes estimated based on an assumed doubling of the volume transport due to a factor of two entrainment. Our data, although limited to a region close to the overflow site, suggest that this is not accurate. As the plume proceeds into the stratified ambient water, there is substantial detrainment from the deeper layer, of comparable magnitude to the entrainment into the interfacial layer. A simplified view of along-path entrainment of a gravity current is thus not valid for the FBC overflow.”

C.2336-4: Table A2 does not list the percentage of data discarded.

R. We now include the percentage of data discarded in the table.

C.2336-10: Does it make a difference if you calculate volume transport from hourly observations of T and v and then low pass-filter? Why do you reduce your resolution in time?

R. As mentioned above, since the higher (i.e. tidal) frequencies are not the focus of this study, we decided to use daily averages. However, we have also calculated volume transports based on hourly fields; the results of low-pass filtering these are not significantly different from those computed using daily fields.

C.2337-1: Would it make a difference if all velocities were included?

R: No. We use this criterion because we define overflow as transport out of the FBC toward the North Atlantic only. This is the dominant mode of transport, and including reverse velocities makes no significant difference.

C. Figure 1: Labels on the depth contours would be helpful. Also, colorbar should be

labeled here and elsewhere.

R. We have labelled depth contours in Fig. 1. We have also labelled the colorbar in this and all other figures with colorbars.

C. Figure 4: Different colormaps for time-mean and standard deviation would make this figure much easier to interpret. Again, colorbars should have labels.

R. We have changed the colour map for standard deviation so that it is different to that of the mean. As mentioned above, we have labelled all the colorbars throughout the manuscript.

TECHNICAL CORRECTIONS

C. 2316-5 arrays were deployed - use past here and elsewhere if applicable

R. We have changed to past tense in the abstract.

C. 2319-22: at section C, at section S

R. Changed.

Response to Reviewer #2.

C. The results presented are very important for our understanding of the role of Faroe Bank Channel (FBC) overflow in the global thermohaline circulation. In the simplest picture, FBC-overflow provides ≈2 Sv of ≈0°C water, which entrains ambient waters of ≈6°C so that the total contribution to NADW is ≈3°C. This implies that the overflow entrains similar amounts of ambient waters. Thus, if there is no detrainment, as claimed by Mauritzen (2005), then FBC-overflow should supply 3-4 Sv of overflow + entrained water to NADW. From the ≈1-2°C warming of bottom water in Geyer et al. (2006), most of this entrainment should also have occurred before the downstream array (the S array) in the present study. Although this calculation is very much simplified and probably is an overestimate, we would have expected more than 2 Sv of water <3°C through the downstream array (S array). But, Ullgren et al. report only 0.8 Sv. Instead of considerable volume transport increase due to entrainment, volume transport is substantially decreased due to detrainment. If these claims are supported, the impact of FBC-overflow on the THC is much less than what has usually been assumed. Unfortunately, I am not convinced that this is the only way to interpret the observational results, because they are based on an assumption that is not well justified in the manuscript. This assumption is that both mooring arrays (sections) cover the entire overflow plume. For the upstream array (C section), this is probably fairly correct although Figure 4 clearly shows overflow water to the left of (south of) mooring C3. For the downstream array (S section), this assumption is less convincing. This section is on the slope (Figure 2) and the moorings do not cover the deepest part of the channel. Thus, there is a priori no guarantee that the array covers the whole overflow plume, but he authors claim this (p.2319, l. 8-10 and p. 2335, l. 8-9). I have not found any arguments, on which they base this claim, but I assume that the main reason is the weak flow at S4.

R. The reviewer raises a valid concern. We now address the question about whether both mooring arrays cover the entire plume in the discussion section as follows: "Our data set, however, shows no corresponding increase from mooring array C to array S. To explore this surprising result, we must first ask ourselves whether our measurements covered the entire plume at both sections.

At array C, overflow water on average occupied the lower 120 m at the southernmost mooring (see section 3). The southern boundary of the plume must thus be south of the mooring section, but because the flow here is constrained by topography the plume cannot extend far to the south. Our horizontal extrapolation adequately compensates for the edges of the plume; our mean volume transport estimate for water colder than 3C of \$1.3±0.3 Sv is within the range of earlier estimates for the FBC overflow varying between 1.1 and 2.1 Sv (van Aken and Becker, 1996; Hansen and Østerhus, 2000; Kanzow and Zenk, 2014). The estimated volume transport at array C is about 2/3 of

the kinematic overflow estimate at the sill (Darelius et al., 2015, and Appendix B).

The S section was not located in the channel, but on the open slope. The question of how that mooring array was positioned with respect to the plume path is therefore important. First, we consider the statistics of the observed temperature and velocity at the moorings at section S. There was a clear presence of overflow water throughout the year at the two moorings S2 and S3 in the centre of the array, while at the two moorings at the edges, cold plume waters were only occasionally present. At the northernmost mooring, S1, overflow waters (T≤3°C) were found at 80 mab (the only measurement level here) less than 10% of the time and modified overflow waters (T≤6°C) less than 35% of the time. At the southernmost mooring, S4, the corresponding percentages at the same height above seabed were less than 2% for T≤3°C and about 36\% for ≤6°C. At both moorings, the lowest temperatures were correlated with stronger along-stream velocity, a clear indication of plume presence at such occasions. Cold water was present at these moorings only during periods with energetic oscillations (Darelius et al., 2015), suggesting that it is brought here by the eddy motion. Most of the time, however, the overflow was found in the centre of the array while absent at the outer moorings (i.e. the boundaries of the plume were found within the mooring array).

While there are no channel walls here to contain the overflow, the shallow topography north of array S likely prevents large excursions of the plume to the north (upslope). There is no evidence in literature of a plume path north of mooring S1. The mean current of the overflow mainly follows isobaths (Geyer et al., 2006), until it begins to descend under the influence of friction (Seim et al., 2010). Are there then downslope excursions that make the overflow pass south of section S? As mentioned above, the occurrence of cold water at mooring S4 was rare, and the long-term average velocity there was approximately zero (see figures 1 and 3). Our observations thus do not show any evidence of significant overflow at (or beyond) the southern end of array S.

C. From Geyer's (2006) Figure 3, we know, however, that the overflow plume splits into (at least) two separate branches. If the splitting occurs before the S array, then the weak flow at S4 could be because this mooring was located between the branches and the S array would not cover the whole of the overflow plume. The authors of the present manuscript are well aware of this branching (p.2317, l. 13-16), but they are apparently convinced that it occurs after their S array. That may perhaps be the case, but in view of the high-impact results presented, this has to be better justified. I have not been able to find observational evidence to support or reject this assumption conclusively, but Mauritzen et al. (2005) have a section (G section), which seems to run very close to the S array of Ullgren et al. On that section, Mauritzen et al. found overflow water below 1000 m depth (their Figures 23 and 24) and even two XCP profiles with bottom depths > 1000 m and core velocities exceeding 1 m/s (their Figure 25). Thus, it is not obvious to me that the basic assumption in this manuscript is valid. In conclusion, Ullgren et al. need either to present more convincing evidence that their assumption is valid, or modify the analysis and results of the manuscript accordingly.

R. We thank the reviewer for bringing up this important question. We are indeed aware of the bifurcation but as the reviewer suggests, we believe that it most likely occurs west (downstream) of the S array. We have now added several paragraphs to the Discussion section (see below) where we explain our reasoning, and added a new map (new Fig. 17) showing the location of our moorings along with those of observations in several other papers. For completeness Fig 17 is included in this response letter. The caption can be found toward the end of the response letter. In addition to the revised text of the manuscript, we would like to reply to some of the specific points raised by the reviewer as follows: Mauritzen et al. (2005) mention that the first occupation of section G, shown in their Figures 23 and 24, took place during a storm that was severe enough that operations temporarily had to be suspended, and they note that volume transport doubled after the storm (although it is uncertain whether this is directly linked to the storm). For Mauritzen's figure 25, showing XCP profiles with high velocities at depths >1000 m, only the profiles with the highest velocities were shown. The profiles down to just under 1100 m are similar in depth to mooring S4, where high velocities were also observed, but only rarely and seemingly associated with eddy activity (cf. Darelius et

al., 2015). Our year-long measurements from several moorings, in combinations with several repeats of the co-located CTD section (see Ullgren et al., 2014, referred to in the revised Discussion text) complement Mauritzen's two occupations of CTD section G, and – given the high temporal variability in the region – significantly improves the picture. Mauritzen et al. write (p. 910): "Between section G and the final section H the plume cannot sink further, and it decelerates. It also appears to separate into two paths, separated by a plateau in the topography." We agree with them on where the splitting occurs, namely after their section G (and thus after our section S); the plateau in question is most likely the one labelled in our new Fig. 17. Mauritzen et al. (2005) further write that "Such a splitting of the plume is also known from historical data." The historical data in question are not specified in their 2005 paper, but after studying reports from e.g. the Overflow 1960 and Overflow '73 expeditions, we are convinced that most evidence points towards a likely splitting of the flow a short distance downstream of our mooring array S – as outlined in the revised Discussion, where we write:

"A second important issue to consider is the bifurcation of the flow. The overflow is known to split into two branches. The shallower branch flows westward, approximately following the isobaths along the Atlantic flank of the Iceland–Faroe Ridge, and the deeper branch turns southwestward, and descends more directly into the southern Iceland Basin (Beaird et al., 2013; Hansen and Østerhus, 2000). The splitting appears to occur between the secondary sill at about 9°W and a topographic "bump" or plateau further downstream (Mauritzen et al., 2005; Beaird et al., 2012, 2013). In designing our mooring array S, we chose a location to the east (i.e., upstream relative to the overflow) of the suspected bifurcation location to ensure an as complete as possible lateral coverage of the plume. Could the weak flow at mooring S4 indicate that the bifurcation occurred upstream of array S and that S4 thus was located between the branches? We turn to literature to find out where this bifurcation is most likely to occur. Repeat hydrographic sections from the multi-vessel "Overflow" survey in June 1960 (Lee, 1967; Tait, 1967) showed the cold core of the overflow centred at about 800 m depth, 10°W (station F1 at 61°54'N, 10°06'W – very close to the 'bump' centred at

61°54'N, 10°07'W; cf. Beaird et al., 2012). However, maps of overflow thickness and percentage overflow water based on the same survey data suggested a bifurcation of the overflow beginning already upstream of this (see figures 3:108 and 3:109 in Hermann, 1967). Synthesizing the observations of the 1960 survey, the "Overflow 1973" experiment (Müller et al. , 1979), and some evidence from geological studies, Hansen and Østerhus (2000) suggested an "alternative path" (see their fig. 43 and 49) for the overflow, turning southwestward and descending more directly and deeper in the (south-eastern) Iceland Basin. In a more recent study, the bottom temperature distribution measured by gliders indicated a branch of the overflow descending deeper than 1000 m, somewhere in the region close to our mooring section S (purple line in fig. 17; cf. Beaird et al., 2013). This branch appeared to the west of our array S, and could not be resolved because of 1000-m depth rating of the gliders. During a survey conducted in June 2000, Mauritzen et al. (2005) found dense waters and high velocities as deep as between the 1000 and 1200 m isobaths at their section G, only about 5 km downstream of our array S (fig. 17, for comparison, mooring S4 was located at a depth of 1082 m). The authors remark, however, that the occupation of section G shown in the paper (their figures 23 and 24) took place after a storm when the overflow transport doubled. A better temporal averaging is provided from the survey in June 2012 reported in Ullgren et al. (2014). In a section repeated five times over the course of 70 h, located about 10 km upstream of our mooring array S, the high-velocity core of the overflow was mostly centered close to, or shallower than, the 1000-m isobath, with reduced flow at the outer two stations (Ullgren et al., 2014). Mauritzen et al. (2005) mention that a division into two branches at about 900 m depth is indicated at their westernmost section, H, located about 50 km downstream of our array S. (Section H is marked by green dots but not labelled in figure 17; it is the section the lower part of which is parallel and close to Geyer's section A.) They suggest that the bifurcation occurs somewhere between their sections G and H, where the flow decelerates and is "separated by a plateau in the topography" (Mauritzen et al., 2005, , p. 910). Most likely this flow separation is thus caused by the same topographic feature as Beaird et al. (2013) refer

to as the 'bump' (see fig. 17). Mean current velocity vectors from July–November 1999 presented by Geyer et al. (2006) show a split into two branches at about 11°W (their Fig. 3), also west of our mooring section S (see Fig. 17). Note that this is a split of a deeper flow around a different topographic feature, southwest of the bump of Beaird et al. (2013). The location of the knoll dividing the flow in the paper by Geyer et al. (2006) fits with the observation by Swift (1984) that data sets from the Overflow 1960 and 1973 expeditions (Tait, 1967; Müller et al. , 1979) both show a "similar three-way splitting of the overflow tongue at about 61°45'N and 11°W". The complex topography that varies strongly on small scales is a major factor controlling overflow pathways in simulations (Chang et al., 2009). Tracer-weighted average plume paths from model experiments by Seim et al. (2010) and Riemenschneider and Legg (2007) generally turn more southward and descend below the 1000 m isobath close to (but typically west of) the location of array S. Further evidence of the overflow splitting into a deeper and a shallower branch in the region just west of array S is found in velocities measured at 64 mab by a deep towed vehicle (Fig. 7 in Duncan et al., 2003). The current below about 900 m veered downslope at a section located about 20 km downstream of array S (Fig. 17). Two out of the three bottom-following float trajectories shown by Prater and Rossby (2005) followed roughly along the 1000-m isobath until at least 11°W, while one track descended earlier (closer to mooring array S). It should be noted that for the latter, as well as for one of the other floats, the positions along this part of the track were only estimates, not directly determined by sound ranging. We conclude that while the mooring array S is located in the region where the FBC overflow divides into two branches, a detailed comparison with earlier work suggests that the split takes place downstream of array S. We are reasonably confident that mooring array S did not miss a branch of the overflow because the bifurcation (or one bifurcation) is most likely caused by the topographic bump centred at about 61°54'N, 10°07'W, approximately 30 km downstream of array S. The deeper branch of the overflow and its path into the Iceland Basin merit further studies."

DETAILS C. 1) In the first line of the abstract, it says ten moorings. In the first line of Part

2, it says nine moorings. In Table A1 and the paper as a whole, there are 8 moorings. R. Corrected. It now says eight moorings in the abstract and in text throughout.

C. 2) On p. 2322, l.21-22, it says that time-average u-velocity reaches 94 cm/s at mooring S3, but that is not what you see on Figure 3c. R. Figure 3c was by mistake made using an older version of the processed data set, and there was an inconsistency in the cut-off threshold for percent good data for the figure and the cited numbers. This has now been corrected. The correct time-average u velocity is 90 cm/s and this is also shown in the updated figure.

C. 3) P. 2326, l. 27: Should "S2" here be "S3" ? R. Yes, thanks for noticing. This has been corrected.

C. 4) Caption for Figure 12: Have colours been swapped? R. Yes, the colours were the wrong way around in the caption – this has now been rectified.

C. 5) Caption for Figure 12, first line: "aray" -> "array" R. Corrected.

C. 6) Caption for Figure 13, last line: "for for" -> "for" R. Corrected.

CAPTION FOR FIGURE 17 (plain text):

Map showing the positions of the moorings in this study together with the mooring positions of Darelius et al. (2011); Hansen and Østerhus (2007); Saunders (1990); Geyer et al. (2006). Also shown are CTD station positions from Ullgren et al. (2014) as well as station locations from Mauritzen et al. (2005), and hydrographic sections with CTD/LADCP and deep towed vehicle from Duncan et al. (2003). Shaded bathymetric contours are plotted every 200 m; the 1000 m contour is bold. Our mooring array S, Geyer's (2006) mooring array A (AG06), Duncan's section S (SD03), and Mauritzen's section G (GM05) are labelled. Current arrows from SD03 and AG06 are shown schematically, not to scale. The topographic bump associated with bifurcation of the flow is marked by a yellow arrow, and a purple line roughly corresponds to the portion

of the 1000-m isobath where the deeper edge of the plume was found to be deeper than the depth range of Seagliders in Beaird et al. (2013) (see their figure 6).

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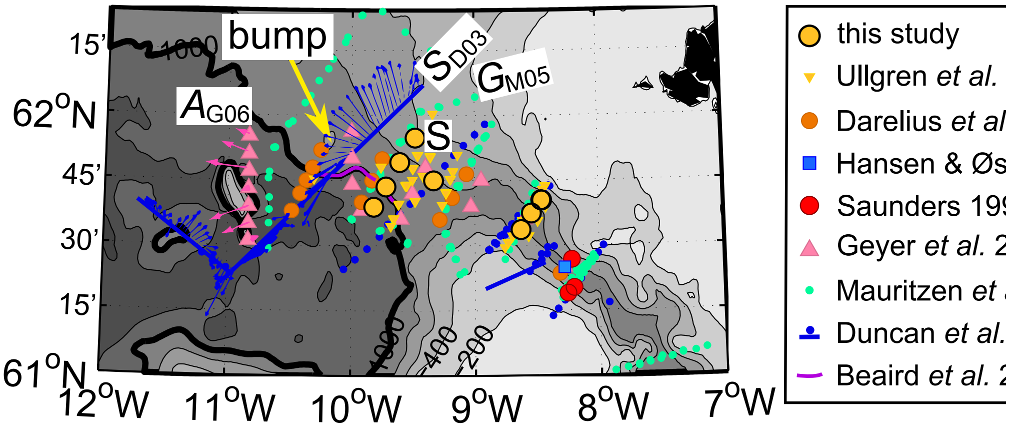

Fig. 1. For caption see the end of the response letter.