Referee #1, Anonymous

This is our preliminary response to the reviewer's comments to encourage discussion when the discussion period is still open. We will provide a more complete response in our final response shortly after the discussion closes. The reviewer's comment is reproduced in black Calibri font followed by our response starting with Re in red, bold Arial font.

This work is based on a comprehensive data set, addresses important questions, and is generally well written and illustrated. Thus, I am confident that it deserves publication in something close to its present form, but it does contain some confusing aspects and details that need to be clarified before final acceptance, as elaborated below.

Re: We thank the reviewer for the detailed reading and constructive comments. We addressed all comments as detailed below.

Main comments

1) Velocity components: Figure 1b shows an (x,y) coordinate system and you expect velocity components to follow the standard notation (u,v). This is confirmed on lines 97-98: "Current components are along-slope, u, and across-slope, v", but then this statement is followed by: "(In the figures, we explicitly use the notation ua and ux, respectively.)" without explaining, which is which (and ux is not along the x-axis). Most of the manuscript seems to keep the (ua, ux) notation, but Sect. 7 with Eqs. (2) and (3) partly returns to the (u,v) notation. This is unnecessarily confusing. Stick to one notation. I would suggest (u,v).

Re: We agree that this was confusing. We realize we forgot to mention the notation, u_a for the along slope and u_x for the cross-slope component. We simply switch to the notation suggested by the reviewer because (x,y) defined on the map can be related to (u,v).

2) Projected distance: On lines 181-182, you write: "The moorings are separated by approximately 6 km (horizontal distance between the locations), and when projected onto the cross-slope section to their respective isobaths, the distance is about 8 km". How can a projected distance become larger than the distance ? Using the positions in Table 1, I get a distance of 5.3 km, which projected onto the cross-slope direction ought to be around 4 km. This projected distance is used extensively in the manuscript. I am not sure that any main results are substantially affected by this, but again it is unnecessarily confusing and has to be corrected.

Re: Thanks for pointing this out. First, agreed that the actual distance is 5.3 km (round up to 6 km is an error). The use of "projection" in this context was wrong. What we did was the following. We defined a cross-isobath section, normal to the isobaths (oriented 42° from East), through the position of MW covering between 100 m and 2500 m depth. We extracted the bathymetry for this section from ETOPO1. We calculated the cumulative distance between pairs of position on this section, giving us depth versus distance from the start of section. We obtained the distance of the moorings on the section by interpolation to the mooring isobaths of 1500 m and 655 m. The distance between them is then 8 km. The calculation is correct; however, inevitably includes uncertainties from the bathymetric data set in the steep escarpment. If projected to the cross-slope direction, because the relative angle between the mooring line orientation and the cross-isobath direction is about 20°, the projected distance is about 5 km (4 km the reviewer infers is for 42° relative orientation, which would be in error). We recalculated the transport estimates using a 5 km distance.

3) The width of the current for transport calculation: As I read your description (lines 182-187), your transport calculation is equivalent to multiplying the average transport density from the two moorings by 28 km (which according to the previous comment ought to be 24 km), but you do not mention what you do with the shallowing bottom in the 10 km inside of MN (to 250 m according to line 184). The statement on lines 184-185: “hence assign a 14 km effective width of water column to each mooring” does not indicate that this was taken into account. This should either be briefly clarified or the transport corrected if it has not been taken into account. As for many similar studies, the width of the current is probably the most uncertain aspect of the transport calculations, so is this uncertainty included in the uncertainties cited in Table 2 or are they just statistical (standard error ?). This should be clarified in the table heading and perhaps also in the text.

Re: Your interpretation is correct. We re-calculated accounting for the reduction is water cross-section area on the MN part. This effectively reduces the width to 7.6 km. The changes are as follows:

> **distance between two moorings = 5 (not 8) km**
> **width of outer mooring is 12.5 km (2.5+10)**
> **width of inner mooring is 7.6 km (2.5 + 5.1), calculated as an effective width to give the same surface area as the area when integrated using the actual topography to 10 km onshore of the 650 m isobath.**

Table 2 of the original version listed the average and 1 standard deviation (we forgot to mention this in the caption, unfortunately). Now our error estimates are improved to also include standard error and all results are reported in the Table. Following up on reviewer 3’s comment, we now also analyse summer 2016 and summer 2017 separately.

To calculate the standard error we use degrees of freedom (DOF) taking into account the decorrelation time scale of 7 days, already reported in the manuscript. DOF then equals number of daily data points in a window of interest divided by 7. The standard error, se, is calculated as the standard deviation, std, divided by $(DOF)^{1/2}$. For example, for AW, the annual values are (mean, std, se): 2.0, 1.3, and 0.2 Sv.

In addition, we now calculate a representative transport error estimate, for winter, summer, and annual data points, separately. The procedure is complex, in order to account for the time variability in statistics. We assume about 20% error (4 km) in the effective width estimate, and assume 0.05 m/s error in depth averaged current at mooring (corresponding to 30 m^2/s transport density). A simple calculation using these figures, ignoring the statistics, would lead to an error of 0.12 Sv. Using the mean and the std of observed transport density (for winter, summer and all data separately), we generate 100 random data points from a normal distribution (U_{obs}), and calculate the transport using 20.1 km width (W_{obs}) as $Q_{obs} = U_{obs}xW_{obs}$. The distribution of transport is approximately normal in each season, and this assumed distribution for error analysis is justified. We then generate 100-point zero mean and imposed rms error data for transport density (U_{err}) and width (W_{err}) from random distribution. Total transport (with error) is $Q_{tot} = (W_{obs}+W_{err})*(U_{obs}+U_{err})$. This is now 100 point time series of transport with error contribution. We calculate the residual, res = Qtot - Q_{obs}, and bootstrap the rms of the residual 1000 times. The mean of the 1000 bootstrap error estimates is the transport error.

This results in an error of 0.8 Sv for annual averages, 0.7 Sv for summer and 0.9 Sv for winter. This is typically less than the standard deviation and 3-4 times the standard error.

4) Transport of the top 50 m layer: The average volume transport cited in the abstract (2.8 Sv) seems not to include the top 50 m (line 213). I assume the reason to be that this layer is less saline than 35.17 on average (Figure 2) due to some admixture of water from the Norwegian Coastal Current, but isn't the fraction of Atlantic water in this layer still » 50% ? Using S=35.17 as a lower boundary for Atlantic water does not necessarily imply that you should use the same criterion for an upper boundary. If you want to retain this, it should in any case be better justified in the text.

Re: This is a very good point. The reason we excluded the top layer was two fold: the low salinity layer as the reviewer noted but also, more importantly, the lack of measurements. Note that we already report (in our sensitivity analysis) an estimate of the increase in transport when the uppermost measurements are extended to the surface, and all water is assumed AW in the top 50 m. See line 213 of the original manuscript "Including the top 50 m increases the total mean transport by 0.4 Sv (from 2.8 Sv)." With revised calculations, it increases by 0.3 Sv (from 2.0 Sv).

We have examined the upper layer from a freely accessible hydrological Altas of the Nordic Seas (Bosse and Fer (2018) Hydrography of the Nordic Seas, 2000-2017: A merged product https://doi.org/10.21335/NMDC-1131411242). The top 50 m layer at MW, corresponding to the core of the slope current, varies from about 35.25 to 34.95 g/kg (figure R1). Assuming values of shelf water of <34 g/kg hence implies that the proportion of AW (S~35.5 g/kg) still exceed 65% to 80%. While we do not include the top 50m into the AW transport calculation, our sensitivity calculation (by extrapolating the uppermost measurement and assuming 100% AW fraction) is informative. This is justified during most of the year, as the upper layer of the water column is well mixed, except during late summer and fall when the seasonal thermocline is present. We could integrate the upper 50 m to the main calculation if this is preferred.

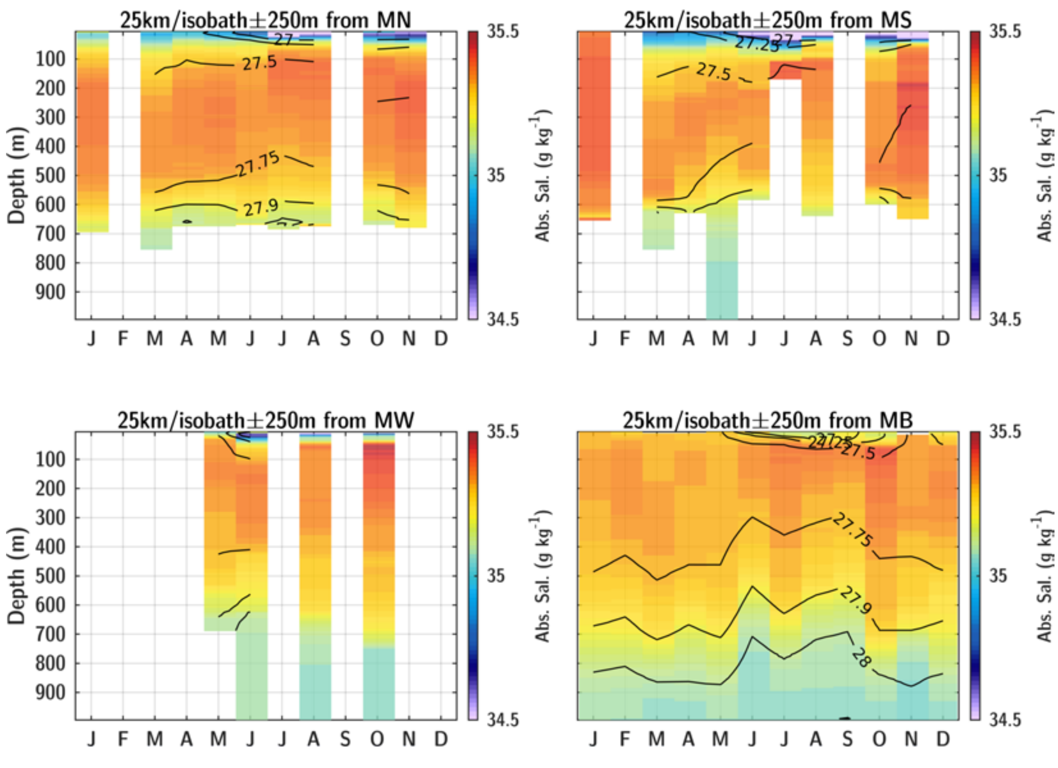

Figure R1: Monthly mean profiles of absolute salinity at each mooring position considering profiles from the hydrographical atlas at less than 25km and 250m bathymetric difference. Black contours show selected isopycnals. Minimum values for MN, MS, MW and MB are respectively 34.21, 34.75,34.95, 35.22 g/kg.

Details

5) line 7: “volume transport” -> “volume transport of Atlantic water”

Re: corrected

6) line 19: “Iceland-Faroe . . . channels” is not standard. You could use “gaps” instead of “channels”

Re: corrected

7) line 110: “Norwegian Sea Deep Water” -> “deeper water”. The NSDW is usually reserved for the deepest component, which is separated from AW by intermediate water masses.

Re: corrected

8) Figure 2, third line of caption: “typical value” -> “typical lower limit”.

Re: corrected

9) lines 118-119 : The last sentence seems to contradict the previous sentence. line 168: “the two moorings” -> “MS and MN”.

Re: corrected

10) lines 175 onwards : In line 175, the letter “Q” (with or without subscripts) is defined as “transport density (m**2/s). After that, the same letter is used for “transport” (m**3/s). This should be corrected.

Re: corrected

11) Figure 8: Consistent with my main comment 1) above, I suggest that you use (Wx,Wy) instead of (Wa,Wx). Having Wx perpendicular to the x-axis is confusing. In the caption to this figure, it might also be emphasized that the velocities in Figure 8b (presumably) are low-passed (not band-passed, as might be assumed from line 220).

Re: corrected (using W_x and W_y).

12) line 324: Isn’t there a rho-0 too much in the last equation ?

Re: corrected. This was a typo and the conversion rate calculations are correct.

13) line 330: Isn’t a parenthesis missing in the middle equation ?

Re: corrected

14) Figure A1b: In my printed version of the manuscript, I could not identify any “magenta line”. On the screen, there is something that could be magenta, but it does not resemble a line.

Re: We improved the presentation of Fig A1.

15) Appendix A: The contents of the appendix seem rather crucial to some of the results and the last parts of the abstract. You might consider pulling it into the main manuscript as a separate section just before the conclusion.

Re: We now incorporated the modelling part into the body of the manuscript.