

Reply to Referee #2

We appreciate the stimulating comments from referee #2. We have replied to his/her comments below.

The first reviewer has already provided a summary of the paper, so I will just go straight to my points. However, everything else I say below solely reflects my opinion and view on the complex process of water mass formation and variability in the North Atlantic.

The issues the authors address in the paper are highly relevant and important for water mass analysis and prediction of their changes over time, and dissecting transformation and mixing of water mass is a big and nontrivial problem overall, so any novel solid approach and a study based on it would be much awaited here.

However, I cannot understand how a trans-Atlantic snapshot (not to mention that the section does not end in St. John's, Newfoundland) and a simple model operating with only four members at once can be used to depict complex interaction and mixing of 14 water masses. I am not in position to judge the previously published paper of the same authors that is used as a basis for the current one, but if I had to review it, I would come with critical suggestions pretty much similar to those presented below.

Let me explain why I believe that a four-member approach does not work for this specific task:

(1) First of all - the case is not two-dimensional (2D distance along section vs depth). The water masses interact in over the entire subpolar North Atlantic. So, for example, any two waters appearing as neighbors on the OVIDE line may be separated by other waters elsewhere in the region. Therefore, the only way to solve this problem for the subpolar North Atlantic and its water masses is through solving a full system of equations where each end-member is carefully defined, and this creates another challenge.

We agree that the water mass circulation, formation and transformation in the subpolar North Atlantic is a complex problem to solve. That is the reason why we carefully defined the SWT properties in their formation area based on values available in the literature. We would also like to emphasize that OMP offers tools to verify the assumptions made and the consistency of the analysis. Most importantly, we verified that the residuals of the OMP equations were small enough to conclude that all samples could be described by the mixing of selected SWT (p6 L4-13): “We tested the robustness of the methodology through a Monte-Carlo simulation (Tanhua et al., 2005), where the physical and chemical properties of both each SWT and each water sample were randomly perturbed within the standard deviation of each parameter (see Text S1 and Table S1). This allowed an assessment of the sensitivity of the eOMP analysis to potential measurement errors and temporal variations in the physical and chemical properties that define the SWTs (Leffanue and Tomczak, 2004). A hundred Monte-Carlo simulations were performed and the eOMP equation system was solved for each of them. The average standard deviation of the X_i s (last column in Table 1) is lower than 12%, which indicates that the methodology is robust. Additionally, our eOMP analysis is consistent since its residuals (r in Eq. 3) lack a tendency with depth (Fig. S1), with the standard deviations of the residuals being slightly higher than the measurement errors (Table 1). Besides, the ability of our eOMP analysis to reproduce the measured values is given as the correlation coefficient (R^2 , Table 1) between the measured values (water samples) and the expected values for the SWT mixing (values of the properties of each water sample obtained by when substituting X_i s in

Eq. (3)). The R^2 values are higher than 0.993, which again indicates the reliability of our eOMP analysis”.

(2) Now, a whole list of problems concerning the end members: a) The authors use end-member properties as they appear on a snapshot of an arbitrary section line (OVIDE or any) and not the properties of the studied water masses that these waters acquired at the times of their formation. Most critically here, both DSOW and ISOW should start from sub-zero temperatures. Both ISOW and DSOW are equally fresh the sills. However, ISOW gains its salt through mixing as it spread through the Iceland Basin. So taking the water that is already salty is not good for telling how it was formed from start – note that it has already been mixed with SPMW. Same is true about the other waters.

We understand your concern. As indicated in page 5, lines 1-23 from the submitted manuscript, the properties selected to define the SWTs (end-members) were taken from the literature, from the regions where the water masses are formed and were not an arbitrary selection to fit the properties of the GEOVIDE cruise. In the case of the overflow waters, we considered that these water masses are formed once they had crossed the sills, that is, once they overflowed and entrained ambient waters. Solving the composition of ISOW and DSOW themselves is not the aim of the present manuscript, and could not be done with the data from the GEOVIDE cruise. We believe that this approach is legitimate because those characteristics of ISOW and DSOW (i.e., after the overflow process) are the most commonly used to track the overflows in the Atlantic Ocean (e.g., Dickson et al., 2002; Fogelqvist et al., 2003; Tanhua et al., 2005;2008; Yashayaev and Dickson, 2008).

(b) By no means, LSW remains undiluted between Labrador Sea and Iceland Basin. However, Figure 4 suggests 90% of original LSW in any other LSW all the way through the region. Well, the Labrador Sea is a very powerful engine, but can it pump so much water that stays unmixed for so far and so long?

In fact we observed LSW proportions up to 90%, but the bulk of it is found in the Irminger Sea, which is another proposed area of LSW-like formation (e.g., de Jong and de Steur, 2016; Piron et al., 2017). Besides, LSW is up to 2000 m thick in the Labrador Sea and, therefore, it is not that surprising that its core characteristics experience little change (about 0.1°C in temperature and 0.01 in salinity as shown in, for example, Yashayaev et al., (2007)) while being advected towards the Iceland Basin. This explains the high proportion of LSW found in the Iceland Basin at the core of the water mass.

(c) The depth of LSW was not 2000 m in 2014, and there cannot 50% of LSW at 3000 m – at the depths where water is already as saline as ISOW modified through entrainment.

We agree that the deep convection in the Labrador Sea was not as deep as in the late 1990s. However, our distribution of LSW in the Labrador Sea could reflect the diapycnal mixing of LSW with ISOW or the entrainment of LSW into the ISOW layer all around the subpolar gyre. In his talk at Ocean Sciences 2018, Bill Johns showed that ISOW is modified through entrainment of LSW on its way along the eastern flank of the Reykjanes Ridge downstream of the Iceland-Scotland sill. High diapycnal mixing has been observed in the Deep Western Boundary Current in the Irminger Sea by Lauderdale et al. (2008). Entrainment and mixing would explain finding high percentages of LSW at depth, associated with the circulation of ISOW. We have added the following information in the text to explain the distribution of LSW in deep layers of the Labrador Sea: “The distribution of LSW in the Labrador Sea that extends deeper than 2000 dbar reflects the diapycnal mixing with ISOW

(Lauderdale et al., 2008) and/or the entrainment of LSW in the ISOW layer all along the subpolar Gyre". However, we would be very interested in evidences that show that there cannot be 50% of LSW at 3000 m.

(d) Then, ISOW is fresher in Labrador Sea than in the Irminger Sea, because it is more diluted, but the corresponding fractions seem very much comparable in Figure 4. Does ISOW really reach 2000 m in the Labrador Sea adding about 50%? Or is it something else? How can we be so sure that another water mass contributing to the mid-depth exchanges and arriving from outside the Labrador Sea is not missed in this formulation? It must be something else rather than 50% of ISOW...

According to the general knowledge, the Deep Western Boundary Current transports ISOW to the Labrador Sea (e.g., Schmitz and McCartney, 1993; Rudels et al., 2002; Tanhua et al., 2008), where it circulates cyclonically (Xu et al., 2010). We are not aware that this general view has been recently questioned. Our water mass distributions are in agreement with that circulation. In addition, the residuals of the OMP equations are small enough to conclude that all samples can be described by the mixing of the selected SWTs. On the other hand, we have not found in the literature more water masses described in the Labrador Sea. However, we would be very interested in the knowing the existence of another possible water mass contributing to the mixing in the Labrador Sea.

(e) I totally agree that a more careful approach is needed for the two chemical variables used in the work. However, using a certain universal model for utilization may lead to overconsumption of oxygen at greater depth. I say this, because the oxygen section suggests weak biological utilization, whereas applying parameterizations used in biochemistry (I cannot expand further here, but any quick assessment would show a comparable result) would reduce dissolved oxygen more than what we see in the section. If we assume a strong bio-consumption, then how would we explain that dissolved oxygen closely follows salinity which in turn is not altered by living organisms?

We do not see how is it possible to determine oxygen consumption rates from an oxygen section without explicitly calculating it. AOU is not negligible along our section. Our OMP setting is adequate to explain the complex mixing of water masses and ventilation/respiration processes that occur in the section, as evidenced by the low residuals and the fact that the selected SWTs and their mixing explain 99.99% of the temperature of the section, 99.9% for salinity, 99.4% for oxygen, 99.9% for silicic acid and 99.4% for nitrate. Regarding the similarity between the oxygen and salinity sections, this is mainly due to the circulation of MW. MW is characterized by high salinity and low oxygen concentration, the latter one related to the high temperature of this water mass.

So far I was talking about using static end members assuming the picture does not change with time. But there is another set of complications coming into play if we introduce temporal variability of water properties. Yes, the source waters change in time, but any static model assumes invariance of the source waters. How long does it take for LSW to cross the basin? Let's say N years? How would the authors introduce the temporal changes previously observed in the source or sources of LSW? Note that convection was not strong in 2010 and 2011, and that it was that water that had probably been seen in the Iceland Basin in 2014! LSW does change a lot in its source in 3-4 years. How would this knowledge be transpired into 3.00 and 34.87 with such narrow error bars? At the season of formation the waters are even more different. Oxygen saturation is probably >95%. Taking the transit time into consideration, the version of LSW seen on the OVIDE line in the southern Labrador Sea may not be directly

related to that transferred to Iceland basin first through DWBC and then under NAC... The properties of the original waters can be much greater than the error bars used through the work. I bring LSW only as an example but the same may be true about other waters brought into the equations.

LSW represents a continuum of vintages whose properties do not change so dramatically from one year to the next (Yashayaev and Loder, 2016). In addition, the LSW vintages mix with the vintages from previous years both in the Labrador Sea and along its way to the Iceland Basin. Therefore, the signal of a concrete vintage is diluted, but the LSW entity itself is conserved due to the large volume that this water mass represents in the Subpolar Gyre. Therefore, only consecutive and persistence changes are observed far from its formation area (Sy et al., 1997). On the other hand, following the comments of referees #1 and #3, we have performed a new OMP run where we slightly modified the temperature and salinity (TS) properties for LSW and ISOW to match those found in the most recent period. The TS properties for LSW in this new run are 3.4°C and 34.855, thermohaline properties chosen from LSW formed in 2008 (LSW₂₀₀₈, Kieke and Yashayaev, 2015, Yashayaev and Loder, 2009, 2017), which, according to the transit times proposed by Yashayaev et al. (2007), would have reached the Irminger and Iceland basins by 2014. The TS properties for ISOW in this new run are 2.7°C and 35, that is, an increase in temperature of 0.1°C and an increase in salinity of 0.01, according to the changes observed in the overflow properties since 2002 (Hansen et al., 2016). We have also revised the standard deviations of the properties that define the SWTs taking into account the temporal variability (you can see the new STDs in Table 1, copied below): “Text S1

The standard deviations (STD) of the potential temperature and salinity that define the source water types (SWTs) were taken from the literature. For Central Waters and SPMWs, the STDs were set as $\pm 0.6^\circ\text{C}$ for temperature and ± 0.06 for salinity, according to the thermohaline variability reported by Robson et al. (2016) for the first 700 m of the water column of the subpolar gyre. For LSW, the STDs were set as $\pm 0.4^\circ\text{C}$ for temperature and ± 0.01 for salinity, to include both the thermohaline properties used in García-Ibáñez et al. (2015) and those used in this work. For SAIW, the STDs were set as $\pm 0.5^\circ\text{C}$ for temperature and ± 0.03 for salinity, based on the variability of the thermohaline of its source waters, i.e., Central Waters and LSW (Iselin, 1936; Arhan, 1990; Read, 2000). For MW, the STDs were set as $\pm 0.2^\circ\text{C}$ for temperature and ± 0.07 for salinity, according to the work of Carracedo et al. (2016). For ISOW, the STDs were set as $\pm 0.1^\circ\text{C}$ for temperature and ± 0.02 for salinity, to include both the thermohaline properties used in García-Ibáñez et al. (2015) and those used in this work. For DSOW, the STDs were set as $\pm 0.16^\circ\text{C}$ for temperature and ± 0.008 for salinity, according to the work of Jochumsen et al. (2012). For PIW, the STDs were set as $\pm 0.2^\circ\text{C}$ for temperature and ± 0.03 for salinity, according to the work of Falina et al. (2012). For NEADW_L, the STDs were set as $\pm 0.03^\circ\text{C}$ for temperature and ± 0.003 for salinity, according to the work of García-Ibáñez et al. (2015). For NEADW_U, the STDs for potential temperature and salinity were calculated using the STDs of its components: MW, LSW, ISOW and NEADW_L (Sect. 2.3 of the main text).

For oxygen, the STDs were set equal to 3% of the saturation value (Najjar and Keeling, 2000; Ito et al., 2004), whereas for nutrients they were obtained by one of the following methods:

- a) For LSW, ISOW and NEADW_L, the STDs for the nutrients was calculated using the STDs in the water samples with more than 95% of those SWTs, following Karstensen and Tomczak (1998). This method was used when the number of water samples for a SWT was greater than 50.*
- b) For the Central Waters, DSOW and SPMW, which are defined by more than one SWT (multi-SWTs), the multi-SWT contributions were obtained by adding the contributions of their respective components. Then, water samples with proportions of the multi-SWT greater than 95% were selected. The property values of each component of the multi-SWT were subtracted from the values of the water samples and linear regressions were performed between potential temperature and nutrients. The STDs of the multi-SWT nutrients were taken equal to the error*

of the intercept. We used the STDs of the properties of the multi-SWTs to each of their components.

c) A modification of the methodology (b) was applied to MW, where samples with proportions greater than 75% were selected to perform the linear regressions.

The STDs of the nutrients of SAIW were assigned equal to those of the Central Waters, because not enough water samples presented proportions greater than 95%. The STDs of the nutrients of NEADW_U were calculated using the STDs of its components: MW, LSW, ISOW and NEADW_L (Sect. 2.3 of the main text).

References:

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Najjar, R.G., and Keeling, R.F.: Mean annual cycle of the air-sea oxygen flux: a global view, *Global Biogeochemical Cycles*, 14 (2), 573–584, doi:10.1029/1999GB900086, 2000”.

The perturbation around the new STDs generates uncertainties in the proportions of the different SWTs lower than 12% (Table 1), which indicates that the methodology is robust against the temporal variability in the properties that define the SWTs.

Table 1: Properties characterising the Source Water Types (SWTs, see footnote a) considered in this study with their corresponding standard deviations^b. The square of correlation coefficients (R^2) between the observed and estimated properties are also given, together with the Standard Deviation of the Residuals (SDR) and the SDR/ ϵ ratios from the data below 400 dbar. The ϵ (standard deviation of the water sample properties) used to compute the SDR/ ϵ ratios are listed in Table S1. The last column accounts for the uncertainties in the SWTs contributions.

	θ (°C)	S	O_2^0 ($\mu\text{mol kg}^{-1}$)	$Si(OH_4)^0$ ($\mu\text{mol kg}^{-1}$)	NO_3^0 ($\mu\text{mol kg}^{-1}$)	Uncertainty
ENACW ₁₆	16.0 ± 0.6	36.20 ± 0.06	246 ± 7	1.87 ± 0.12	0.00 ± 0.15	9%
ENACW ₁₂	12.3 ± 0.6	35.66 ± 0.06	251 ± 8	1.3 ± 0.9	8.0 ± 1.1	10%
SPMW ₈	8.0 ± 0.6	35.23 ± 0.06	289 ± 9	2.7 ± 1.9	11.4 ± 1.3	11%
SPMW ₇	7.1 ± 0.6	35.16 ± 0.06	280 ± 8	5.20 ± 0.15	12.83 ± 0.15	6%
IrSPMW	5.0 ± 0.6	35.01 ± 0.06	310 ± 9	5.9 ± 0.4	14.1 ± 0.4	12%
LSW	3.40 ± 0.4	34.86 ± 0.01	307 ± 9	6.9 ± 0.7	14.8 ± 0.7	10%
SAIW ₆	6.0 ± 0.5	34.70 ± 0.03	297 ± 9	6.0 ± 2.4	13.3 ± 1.2	9%
SAIW ₄	4.5 ± 0.5	34.80 ± 0.03	290 ± 9	0.0 ± 2.4	0.0 ± 1.2	3%
MW	11.7 ± 0.2	36.50 ± 0.07	190 ± 6	6.30 ± 0.15	13.2 ± 0.2	2%
ISOW	2.7 ± 0.1	35.00 ± 0.02	294 ± 9	11.8 ± 0.9	14.0 ± 0.6	9%
DSOW	1.30 ± 0.2	34.905 ± 0.01	314 ± 9	7.0 ± 0.5	12.9 ± 0.8	7%
PIW	0.0 ± 0.2	34.65 ± 0.03	320 ± 10	8.4 ± 2.5	13.4 ± 1.2	9%
NEADW _U	2.5 ± 0.5	34.940 ± 0.07	274 ± 8	29.4 ± 0.6	18.1 ± 0.5	^c
NEADW _L	1.98 ± 0.03	34.895 ± 0.003	252 ± 8	48.0 ± 0.3	22.0 ± 0.5	3%
R^2	0.9999	0.9984	0.9939	0.9978	0.9941	
SDR	0.009	0.005	2	0.4	0.2	
SDR/ ϵ	2	2	2	1	1	

^aENACW₁₆ and ENACW₁₂ = Eastern North Atlantic Central Water of 16°C and 12°C, respectively; SPMW₈, SPMW₇ and IrSPMW = Subpolar Mode Water of 8°C, 7°C and of the Irminger Sea, respectively; LSW = Labrador Sea Water; SAIW₆ and SAIW₄ = Subarctic Intermediate Water of 6°C and 4°C, respectively; MW = Mediterranean Water; ISOW = Iceland–Scotland Overflow Water; DSOW = Denmark Strait Overflow Water; PIW = Polar Intermediate Water; and NEADW_U and NEADW_L = North East Atlantic Deep Water upper and lower, respectively.

^bThe standard deviation of the properties of the SWTs were obtained following the method described in the Supplementary Information (Text S1).

^cNo uncertainty is given for NEADW_U since it was decomposed between MW, LSW, ISOW and NEADW_L (see Sect. 2.3).

Is it really true that DSOW has no LSW mixed into it? I find it strange because in the northern Irminger Sea DSOW is cascading down the slope entraining both NEADW (ex-ISOW) and LSW and warmer waters.

It is true that DSOW mixes with LSW, NEADW and Atlantic waters when cascading the Greenland-Iceland sill. This mixing was taken into account by defining the properties of DSOW after the overflow process, since we assume that DSOW is formed when the water has crossed the sill, like other authors do (e.g., Fogelqvist et al., 2003; Tanhua et al., 2005; Yashayaev and Dickson, 2008). This point is included in the text by the following statements: “DSOW forms after the deep waters of the Nordic Seas overflow the Greenland–Iceland sill and entrain Atlantic waters (SPMW and LSW) (Read, 2000; Yashayaev and Dickson, 2008). [...] The thermohaline characteristics chosen for DSOW were selected from those found by Tanhua et al. (2005) downstream of the Greenland-Iceland sill”. In addition, one of our mixing groups (mixing group 3) allows the mixing of DSOW, PIW, LSW and ISOW to account for any additional mixture of LSW and DSOW downstream of the sill.

The Monte Carlo technique would only help if the errors were random respecting central tendency. I have no doubt that each of the linear 4-member solutions would converge even with larger seeded errors. However, the present case is subject to more systematic rather than random biases, raising a question like “How would each solution change if LSW was 0.3 warmer at time of formation?”

The Monte Carlo technique has been commonly used to test the robustness of the OMP analysis for temporal variations in the properties of the end-members (e.g., Tanhua et al., 2005; Jeansson et al., 2008; Pardo et al., 2012). Besides, the residuals of the OMP analysis in terms of error in salinity, temperature, oxygen and nutrients do not generate any bias in relation to the SWT proportions. Therefore, the Monte Carlo technique is suitable to test the robustness of the selected SWT. However, it is true that the variability introduced in the properties that define the SWTs is less than the long-term variability of LSW properties, for example. That is why, as stated before, we have revised the standard deviations of the properties that define the SWTs taking into account the temporal variability, resulting in uncertainties of the water mass proportions lower than 12%, thus confirming the robustness of the method.

Saying that the task of unscrambling water mass composition in this highly dynamic and variable area is well worth pursuing, I, unfortunately, cannot agree that the presented method, data and results help much solving this task. There must be a solution, but based on a more extensive synthesis of three-dimensional (3D) data, on a proper definition of source waters and their changing properties, on accounting for multiple pathways, etc.

We agree that the North Atlantic circulation, and water mass formation and transformation is a complex problem. One of the main criticisms to the OMP analysis is that its results are sensitive to the number and definition of the SWTs, and that the analysis is limited to distinguishing only as many water masses as there are distinct water properties. However, in our study we performed an OMP analysis using 14 SWTs defined by five properties. In order to solve an over-determined system, the SWTs were organized into 11 subsets of maximum four SWTs each, which were set according to the characteristics and/or dynamics of the water masses in the Subpolar North Atlantic. These 11 subsets were vertically and

horizontally sequenced, and they share at least one SWT with the adjacent subsets to ensure water mass continuity. This methodology allowed us to solve the complex water mass circulation in the section, the OMP results being consistent with the water mass circulation in the Subpolar North Atlantic, and explaining more than 99% of the properties observed in the section. Therefore, we argue that the OMP method is suitable for the study water mass distributions, and their formation and transformation (e.g. Álvarez et al., 2004; Tanhua et al., 2005;2008; Jeansson et al., 2008; Pardo et al., 2012; Carracedo et al., 2016), and it allows distinguishing the relative importance of conservative mixing from non-conservative processes on tracer distributions (e.g. Llanillo et al., 2013, de la Paz et al., 2017).

Concerning the transport part... The water mass transport and transformation are two related problems. I don't think a simple geostrophy (note a coarse grid in the Irminger Sea and missing profiles in the western Labrador Sea - both are important for budgeting the fluxes) is sufficient for constraining the transports. Frankly, I would not even bring the transport part in the work discussing the contributions of source waters. I think the most important part for now is building a method adequate for the task and thoroughly investigating every aspect of interaction by taking into account a huge baggage of what is known and available to this date and developing something better than a static 2D approach for analysing a strongly time and space variant 3D dynamics and variability – essentially 4D.

Regarding the velocity field, you are probably right about the Labrador Sea. However, we present in the paper the results of water mass transport across the OVIDE section, which geostrophic velocity field was solved by the box inverse model technique that has been validated by favorable comparisons with independent measurements (Gourcuff et al., 2011; Danialt et al., 2011; 2016; Mercier et al., 2015). The subsampling in the Irminger Basin was taken into consideration by Zunino et al. (2017), who determined that the calculations of the transports through the GEOVIDE radial was robust despite the subsampling of certain regions and concluded that the final errors of the dynamical structures in 2014 are of the same order of magnitude as the errors estimated in previous OVIDE cruises. Regarding the method used to solve the dynamics and variability of the water mass circulation and transformation in the Subpolar North Atlantic, please refer to the answers to your previous comments.

To conclude my review I share my thinking of this problem as cookbook analogy – all we try to come up here with is a recipe. Think of real ingredients and not those appearing someplace somewhere – if you use the latter, the results are not going to tell much about your true ingredients. On the other hand, by weighting the real properties of the waters with the sought and found fractions, one should come to a section plot similar to that observed.

Considering the amount of data, effort and work needed to address the issues I raised in my review, I recommend rejection. This only reason why it is not revision is that by redoing the paper the authors would come with a totally new method, sets of results and visions. Sorry, but I cannot see it any simpler than that.

Considering all the arguments compiled in the answers to your comments, we demonstrate that the OMP analysis is a suitable tool to study water mass distributions. We also prove that our choice of SWTs is appropriate to describe all the cruise samples, as evidenced by the low residuals and high correlation coefficients (R^2 , Table 1) between the measured values (water samples) and those resulting from the mixture of the SWTs. The water mass distribution resulting from our OMP set up is in agreement with the accepted knowledge of the Subpolar North

Atlantic circulation. Therefore, we are confident that the submitted work is suitable for its purpose, to provide a framework for interpreting the observed distributions of the trace elements and their isotopes along the GEOVIDE cruise.

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