We are very grateful for the referee for his thorough analysis of the manuscript, suggestions and comments, as well as for a high evaluation of the results of this study. The detailed answers to referee comments and the changes introduced in the text are listed below.

Referee 2

The manuscript presents a comprehensive study of the intermediate water masses in the subtropical northeast Atlantic. This turns out to be an excellent description of the spatial distribution of the different water masses, accompanied by estimates of the associated velocity fields. I would like to praise the authors for all the work they have done. It is an extensive analysis that certainly must have represented a major effort, and I have no doubt that it may eventually become a principal reference for future studies of the intermediate waters in this region.

Unfortunately, the manuscript has a poor description of the velocity fields, both on the way these fields are calculated and on the related diffusive and vorticity analyses. These limitations are not central to the main objective of the manuscript (i.e. the description of the distribution of water masses), so they should not jeopardise the intrinsic value of the study, but they raise doubts on the credibility of some of the work that is shown. Generally speaking, I get the impression that the authors have tried to put into the paper much information beyond the distribution of water masses: an assessment of the velocity fields at the cores of the different water masses, an analysis of the diffusive versus advective contributions leading to the propagation of the water masses, and an analysis of the mechanisms for water mass propagation. Each of them could possibly be a paper on its own so it is not rare that placing them all together, within a rather limited space, has led to a somewhat incomplete and complicated story. Further, too often the manuscript lacks a clear and simple language and becomes difficult to read, contributing to add confusion onto the reader.

In my view, these deficiencies should not be very difficult to solve but they need to be carefully addressed before the manuscript can be published in Ocean Sciences. Perhaps the authors should view this advice as an opportunity to split this long work into two different contributions: one describing the distribution of the water masses and the other one dealing with the mechanisms and water-paths leading to these distributions. Hence, I recommend major revisions before publication. Nevertheless, I would like to congratulate again the authors for their comprehensive analysis and encourage them to provide a carefully revised version of the manuscript, so that it can be rapidly published in Oceans Science for the benefit of the oceanographic community.

I will next explain my main concerns, followed by a number of minor issues.

Reply:

We are grateful for the referee for a thorough analysis of the manuscript, suggestions and comments, as well as for high evaluation of the results of this study. As it is correctly stated above, the manuscript primarily is a description of distribution of the mid-depth water types in the NE Atlantic, 3D positions of their cores and boundaries. The basic data-sets are the most recent climatologies of various water properties. The material on advection and diffusion patterns is complementary and is designed for an additional demonstration of the realism of the patterns obtained in the OMP analysis. At the same time, we **did not intend explaining** the observed distribution of the water types (those issues will be investigated in a separate paper in full detail). Here we only demonstrate the possible role of advection effect on phenomenological level, orienting mostly on the direction of the flows and not on the velocity values. Meanwhile, the reader can appreciate the continuity of the circulation patterns obtained in the study from averaging of actually independent vector data-sets in each of the squares. The mid-depth flow patterns also include the well-known features, as the LSW flow along the eastern slope of the Azores plateau and the Mediterranean Undercurrent. This suggests the qualitative picture of the general circulation patterns is rather robust.

At the same time, we agree that description of the methodology for obtaining current velocities is too condensed and now give a more complete description of the method in the added Appendix 1.

In the revised version of the manuscript we also made an effort to put more emphases on description of water mass distributions and removed unnecessary details. The detailed responses to the referee comments are given below.

Major issues

1) Calculating the velocity fields (Section 2.1)

The explanation on the procedure followed to obtain the velocity fields is incomplete and, hence, confusing. My understanding is that the velocities are directly calculated from the RAFOS and Argo positioning, after removing those portions of the trajectories linked to the float traveling within an eddy. These velocity fields are then used as the reference velocities for calculating the velocity at all depths under the assumption of geostrophy. (Incidentally, the thermal wind equation in page 778 is wrong.) This is a not trivial procedure and in the manuscript is not properly explained, which raises several doubts:

-How are the Argo velocity fields calculated? Argo floats have a vertical cycle to gather hydrographic data and transmit them while at the surface, spending some time away of their parking position. Is this taken into account? And if so, how is this done? This is not trivial and should be carefully explained. An analysis of the errors involved should also be discussed or, at least, proper references should be acknowledged. Possible references are Rosell-Fieschi et al. (2015) and Castellanos et al. (2015).

Reply:
We agree with the referee that quantitative evaluation of the advective fluxes requires a more careful evaluation of the errors in determination of current velocities and should be a subject of a separate study. But in this study, we used only qualitative picture of the general velocity patters, as an additional proof for reliability of the OMP results. The idea is to demonstrate that the major pathways of the main water types in the area closely follow the directions of currents at the corresponding depths. Since the two data-sets are completely independent the similarity is striking. That is the message we wanted to convey in this paper. The study of the mechanisms of the water mass transport will be a topic of the following study.

At the same time, we agree that a number of details in description of formation of Eulerian gridded velocity fields are missing. In particular, in the new version of the manuscript the current velocity errors discussed, based on the recommended reference (Rosell-Fieschi et al., 2015). This description is moved from the end of Section 2.1 to Appendix 1.

-What happens when you have more than one reference velocity? This is likely the case as you get the RAFOS and Argo floats probably drifting at different depths. Perhaps the authors have used geostrophy to check for consistency between both velocity fields. They need to explain their procedure and to discuss how consistent the results are.

Reply:
The corresponding description is changed as the following:

“The float trajectories are further collected for the selected reference depth levels, using all floats from the depth interval ±500-m around a particular reference level. Geostrophic relation is used to reduce the estimated Lagrangian floats velocities from the measured float depth to the reference depth:

$$\frac{\partial \vec{V}}{\partial z} = -\frac{g}{f\,\overline{\rho}}\left(\vec{k} \times \nabla \rho\right),$$

where $\vec{V}$ is the Lagrangian horizontal velocity, g is the acceleration of gravity, f is the Coriolis parameter, ρ is the water density (obtained from the MEDTRANS climatology), $\overline{\rho}$ is the characteristic value of ρ. Using floats from a layer, instead of a fixed level, significantly enhances the number of floats in each of the square and adds to the robustness of the results.”

-For the RAFOS floats, the authors say they have removed those portions of the trajectories when the float remains within an eddy. The eddy is identified as describing two full rotations within 20 days. This sounds fine but is questionable: a full rotation may be done in a few hours but this would not correspond to an eddy. It probably depends on the characteristics of the data, but these are not described in the text. Further, the eddy itself moves with the background flow, which in principle should not be removed.

Reply:

The minimum sampling period is 6 hours to 1 day, so the full rotation, registered with at least 3 point is 18 hours -3 days. Then we need at least 2 full rotations. So the total period of the float in rotation was at least 1.5-6 days. The typical periods of rotation registered were 5 to 10 days. The similar computation has been made without applying this eddy-filter. This filtering procedure was verified to mainly affect the currents close to the Iberian Peninsula, and has very little influence on the results several hundreds km away.

The reason for removing eddies is that in the field of the ambient zonal flow, a secondary circulation in mesoscale eddies (β-gyres) is intensified, which resist the simple advection. It is often noted that mesoscale eddies may travel against the background flow (Morel and McWilliams, 1997). Therefore, the current vectors, computed from the floats trapped within mesoscale eddies, are a source of additional noise in the estimated advection patterns, and it is better to exclude them from computations.

Meddies are known to be one of the strongest mesoscale eddies in the region at mid-depths. They are also robust meddy detection criteria, as meddies form strong TS anomalies in vertical profiles. Thus, we verified that at least the effect of meddies is removed from the Argo data-set, which is the best we can do with these data of too low temporal resolution. Those issues are explained in Appendix 1.

-For the Argo floats, meddies are detected from their high salinity anomaly and their associated motions are removed. How is this done? The data comes from the vertical profile, so how the authors know if the float remained within a meddy during its displacement? Alternatively, the float could have remained in a meddy during its displacement and get out by the time it does the vertical profile. The Argo floats can only give a mean value for every time they surface, in a sense low-pass filtering the data (removing the oscillations because of their rotation around the meddy) so, wouldn't it be better to not worry about if there is a meddy or not? Again, the meddies themselves can move with the background flow.

Reply:

For Argo floats we used Richanrson's criterion in vertical profiles: "Meddies are identified in Argo vertical profiles as salinity anomalies of 0.2 or more, persistent over more than 200-m layer within 500-1500 m depth interval (Richardson et al., 1991). The MEDTRANS climatologic salinity is taken as the reference field. A part of an Agro trajectory is removed, when a meddy is detected during at least 20-day period. For the typical 10-day Argo sampling period this means a meddy detection in at least 3 consequent vertical profiles.."

The situation suggested by the referee can certainly take place: the float could have remained in a meddy during its displacement and get out by the time it does the vertical profile. Still our experience in detection of meddies show that whenever a float is trapped within a meddy motion, it stays there for many months. The possible reason is the strong potential vorticity gradient at a meddy boundary, so that there very limited particle exchange between a meddy and the environment (in particular, this explains very long meddy life-times), as well as a significant vertical extension of a meddy dynamic signal, often reaching the sea-surface (Bashmachnikov et al., 2014).

-In any case, the authors need to explain what are the characteristics of the RAFOS and Argo data they use (it is not enough to simply refer to a web page). Further, for each case, the procedure has to be explained orderly and carefully. Please avoid placing everything together in one or two paragraphs.

Reply:

We would like to note that the currents are only use to illustrate that spreading of the water masses follows the main advection patterns in a qualitative rather than quantitative manner. We extended the description of the data-sets, but have to limit their statistical descriptions not to make already long manuscript too long. The following description is now added at the end of section 2.1:

"The RAFOS float data are downloaded from the WOCE Subsurface Float Data Assembly Center (http://wfdac.whoi.edu/). The data set of 353 floats covers depths interval from 400 to 1700 m and the time period from 1982 to 2002. The typical sampling period is from 6 hours to a day. The overall duration of the observations is 6700 float-months.

The Argo float data-set is obtained from the Coriolis operational data center (ftp://ftp.ifremer.fr). The data set of 242 Argo floats covers the same depth range as above and the time period from 1999 to 2013. The typical sampling period is 10-15 days. The overall duration of the observations is 6560 float-months.
From the float positions Eulerian currents are computed, gridded to 1° × 1° regular grid for the selected depth levels (the details are presented in Appendix 1)."

2) The advective and diffusive fluxes (Section 3.2)

Given the above lack of information, I get to look at the velocity fields provided by the authors (Figs. 11 and 12) with some caution. The velocity vectors are indeed quite variable. The authors provide some error bars when discussing Figure 12 but this is not a very complete discussion. The schematic trajectories in Fig. 11b,c are not properly justified.

Reply:
The velocity fields obtained is indeed not non-divergent, as we can expect from the geostrophic flow field. Still it is organised in continues patterns and large eddies, which suggest that the overall field is rather robust. Again, we underline that the results are used only for a preliminary interpretation of the observed spreading patterns of water masses based on coincidence of the main pathways and the direction of the advective patterns.
The trajectories of the water mass spreading, derived from the OMP (new Fig. 12), represent the major directions of the water mass spreading, but not their detailed pathways. In water mass analysis this data are used as an indirect indication of the direction of deep fluxes, often used in ocean studies (see, for example, Tomczak and Godfrey, 2003). The details of the procedure for drawing those schematic trajectories are presented in Figs. I and II below, as an answer of a specific comment.

The authors then apply the tracer-contour method (Zika et al., 2010) to the water mass concentrations. To my understanding, this method is to be applied for tracers: temperature, salinity, potential vorticity, possibly with some limitation to other non-conservative properties such as inorganic nutrients or dissolved oxygen. But can it be applied to water mass concentration, which in a sense is a combination of all these properties? The authors need to justify this application. One first check on the method could be to neglect the vertical diffusion in equation (2) and to solve it for the horizontal diffusion coefficients – these values could be hence compared with independent estimates for the North Atlantic.

Reply:
Spreading of water masses is spreading of particles with certain properties. If we use the conservative properties (or transform nutrients into conservative variables, - see Eq.(1) and explanation below), advection does not change the concentration. The variation of the water mass concentration is only due to diffusive exchange of properties with other water masses. We also assume that the mixing intensity of all water properties goes at the same rate (which is also one of the basic assumptions of the OMP). Therefore, water mass concentration fall under the hypothesis for a tracer-contour, used by Zika et al. (2010). Due to luck of sufficient estimate of the errors we do not directly apply here the method by Zika et al., which allows computation of the coefficients of vertical and horizontal diffusion, but only estimate the relative intensity of diffusion to advection, using the basic idea of the method.

3) The frictionless vorticity equation (Section 3.2)
I am confused with the arguments used by the authors to reach equation (5).
Incidentally, equation (3) is wrong: V is the velocity vector rather than the modulus of the velocity.
Reply:
Thank you. Certainly, V is the velocity vector, not properly denoted. The caption is corrected.

Equations (3) and (4) show two terms (the planetary and topographic effects) that added together are zero (P + T = 0). Hence, the argument that, under certain conditions, one term is larger than the other are, in my opinion, meaningless. For example, the authors say that over the slope the topographic term will likely be larger than the planetary term (T » P) and, hence, will be the dominant term in the equation: hence the equation simplifies to T = 0, but this contradicts the initial hypothesis, i.e. T cannot be larger than P.

Reply:

Eq. 3, $v\beta/f - \vec{V}\cdot\vec{\nabla}H/H = 0$, is the necessary condition of a full flux in the water column (or in a layer) to be geostrophic. In the equation we neglects baroclinic JEBAR effect and friction. Eq. (3) directly follows from the system of geostrophic equations $\begin{cases} fv = \dfrac{1}{\bar{\rho}}\dfrac{\partial P}{\partial x} \\ fu = -\dfrac{1}{\bar{\rho}}\dfrac{\partial P}{\partial y} \\ \dfrac{\partial u}{\partial x} + \dfrac{\partial v}{\partial y} = 0 \end{cases}$. In fact, after vertical integration and cross-differentiation of the momentum equations we get $\dfrac{\partial}{\partial y}\left(\dfrac{f}{H}v\right) + \dfrac{\partial}{\partial x}\left(\dfrac{f}{H}u\right) = 0$, from where, using the vertically integrated continuity equation, we get Eq.(3). In other words, if a flow is geostrophic, it has to obey Eq.(3), and vice versa.

The balance between the terms in Eq.(3) is archived by a simple variation of the direction of the flow. In the case of the flat bottom, it reduces to $v\beta/f = 0$, which trivially means $v \equiv 0$ or a zonal flow (there is no restriction on the zonal velocity component, so, in general, $u \neq 0$). When the bottom starts being inclined, $\vec{\nabla}H \neq 0$, the second term of Eq.(3) grows and v of a geostrophic flow, in general, no longer has to be zero. In particular, when the second term become dominating, the flow is directed nearly along isobaths: $\vec{V}\cdot\vec{\nabla}H \approx 0$, the closer the steeper is the topographic slope. So we see no contradiction here.

The answer is that equations (3) and (4) are not correct, friction does play a role. Actually, the ratio in equation (5) does not depend on friction being negligible. All it does is to tell which term will be the dominant one, balanced by friction (either interfacial or bottom friction). This needs to be clarified by the authors. This will help better understand what controls the flow in those regions where e.g. the topographic-beta effect decreases, etc. Possibly their final conclusions are correct but the arguments leading to the conclusions are now poorly explained and do confound, and fatigue, the reader.

Reply:

In fact, the more complete form of Eq.(3) includes JBAR effect and friction:

$\vec{V}\cdot\vec{\nabla}\left(\dfrac{f}{H}\right) = -\dfrac{1}{H^2}J(H,\Phi) - rot\left[\dfrac{\tau^b}{H\rho_0}\right]$, where τ^b is the bottom stress, $\dfrac{1}{H^2}J(H,\Phi)$ is the JEBAR effect.

Here we need to point out, that when friction stars playing an important role, the flow is not longer geostrophic, while for the flows with the horizontal scales of the MUC (50 to 80 km) geostrophy is known to generally be a good approximation.

We can check that. Friction can be neglected, if the Ekman number is small: $E = \dfrac{K}{fL^2}$ <<1, where K is the coefficient of vertical or horizontal viscosity and L is the spatial scale of the flow. Baringer and Price (1993) found that for the bottom friction in the MUC E~0.2<<1 within 100 km from the Strait of Gibraltar. E further decreases west (already in the study region), following the decrease of the MUC velocity. For horizontal friction have L~50 km, f~ 10^{-4} s^{-1}, K ~ $0.002L^{4/3}$~3700 m^2 s^{-1}. Thus, E ~10^{-2}<<1. Therefore, we can consider E<<1 for both, vertical and horizontal viscosity and geostrophy is a good approximation. This is now written in the text.

The JBAR effect, though, may be important over the steep continental slope, and in the new version of the manuscript we consider the following form of equation (3):

$$\vec{V}\cdot\vec{\nabla}\left(\frac{f}{H}\right)=-\frac{1}{H^2}J(H,\Phi),$$

where $\frac{1}{H^2}J(H,\Phi)=\frac{1}{H^2}\left(\frac{\partial H}{\partial x}\frac{\partial \Phi}{\partial y}-\frac{\partial H}{\partial y}\frac{\partial \Phi}{\partial x}\right)$ and $\Phi=\frac{1}{\rho_0}\int_{-H}^{0}\left(\int_{-H}^{-z}g\rho\, dz'\right)dz=\frac{1}{\rho_0}\int_{-H}^{0}g\,\rho\, z\, dz$

The full paragraph after equation (3) is also confusing, as it mixes concepts such as the topographic and planetary beta-effects and potential vorticity. This also needs to be carefully explained and clarified.

Reply:

Those concepts of topographic/planetary beta-effects and potential vorticity are very closely linked.

In fact, the equation of conservation of Rossby's potential vorticity (q) results from taking the rotor of the geostrophic equations of motions (above) and adding the continuity equation (this time, with generally non-zero gradient of the vertical velocity): $q=\frac{\omega+f}{h}$, where $h(x,y)$ is spatially variable water depth and ω is relative vorticity. For a quasi-geostrophic motion this can be reduced to: $q=\omega+\beta y-\frac{f_0}{H}\delta h$. Here δh is a deflection of the water layer thickness (or water depth) from the mean layer thickness (or depth) H: $\delta h=h(x,y)-H$. Neglecting ω for a mesoscale mean flow (see J.Pedlosky, Ocean Currents, 1998), we get that the conservation of q is equivalent to: $\frac{d}{dt}\left(\beta y-\frac{f_0}{H}\delta h\right)=0$, or (for a time-constant mean flow): $(\vec{V}\cdot\vec{\nabla})\left(\beta y-\frac{f_0}{H}\delta h\right)=0$, which, after differentiation, becomes exactly Eq.(3), where JEBAR is neglected: $\frac{\beta v}{f_0}-\frac{\vec{V}\cdot\vec{\nabla}H}{H}=0$. Therefore, conservation of Rossby's q is equivalent to $\frac{\beta v}{f_0}-\frac{\vec{V}\cdot\vec{\nabla}H}{H}=0$, and vice versa..

4) Too many typos

I would usually place typos inside the minor issue category but in this paper there are far too many typos. These include grammatical errors, wrong word selection, verbose writing, unclear sentences, misplaced commas and errors in the equations. I point some of the most relevant mistakes below, under the "minor issues" category, but this is not an exhaustive list at all. There are far too many and the authors should probably search the help of a professional, or a well-qualified native English speaker, to have them properly corrected.

The authors must make a serious effort to simplify and clarify their writing, and correct all typos. Right now this is an important handicap for their manuscript: there is a good chance that an average reader gets tired simply because of the many typos and the lack of simplicity and clarity in the writing style.

Reply:

In the new version of the manuscript we tried to make the narration more clear and to correct spell-checking errors.

Minor issues

p. 770: "transition lines", need to be properly introduced

Reply:

Word "transition" is removed. The phrase is changed to:

"The MW in the Atlantic spreads as three cores of different density: the upper MW core (northwest of the line 28° W 35° N - 14° W 44° N) is found in the neutral density range of 27.65-27.70 kg m^{-3} at the depths of

900-1000 m; the main MW core (between the line above and the line 35° W 28° N - 10° W 37° N) has neutral density of around 27.75 kg m^{-3} and is found at 1000-1100 m; the lower MW core (southeast of the line 35° W 28° N - 10° W 37° N) has neutral density around 27.80 kg m^{-3} and is found at 1250-1350 m."

p. 772, l. 22: ENACW

Reply:

Thank you, this is corrected.

p. 773: you introduce both WNACW or ENACW types but then propose one single intermediate water type, H, which can only be good for one of them. Later on in the paper you make no difference between western and eastern water types. This is confusing.

Reply:

The H for the WNACW and that for the ENACW slightly differ. To make this clear the phrase is changed to:

"In T-S diagrams the NACW lines (either the WNACW or the ENACW) are slightly curved and can be better approximated with two sections of a broken line. To account for this feature, in water mass analysis an additional water type (H, Table 2) is introduced, characteristics of which slightly differ depending on weather the WNACW or the ENACW is considered (Pérez et al., 1998; Alvaréz et al., 2004; Barbero et al., 2010)."

Initially we regarded the WNACW and the ENACW separately for the analysis. But this limited number of the intermediate-depth water types, which may play an important role in the main thermocline (down to 1000m depth). Since the intermediate water types are in the focus of this study, we were forced to reduce the number of fractions of the NACW to 3 (see also Pool and Tomczak , 1999). This is stated at p.13 (Section 2.2). Now the paragraph is extended to give more details as::

"The number water types in the OMP is limited by the number of variables observed in-situ (Eq. 1). To uniformly resolve the mid-depth source water types of interest, the western and the eastern fractions of the NACW are clamped together into: the upper NACW ($NACW_u$), the H and the lower NACW ($NACW_l$). The StrMW is treated separately due to its comparatively vast distribution in the southwestern part of the study region and to its very particular nutrient concentrations (Table 2 and Fig. 2a-b). The resulting characteristics of the $NACW_u$, the H and the $NACW_l$ are taken within the known limits of the corresponding fractions of the WNACW and the ENACW, in the way, that the mixing triangles, formed with the water types above in each of the parameter spaces (θ-S, θ-O_2, θ-NO_3, θ-Si, etc.), tightly surround all the observed data- points and in all parts of the study region (Fig. 2a-b). As the result, the characteristics of the $NACW_u$ and of the H (Table 2) are taken close to the mean of the corresponding fractions of the WNACW and the ENACW, as both influences the upper and mid-thermocline in the study region. The final characteristics of the $NACW_l$ (Table 2) are taken close to the ones of the $ENACW_l$, as this fraction is known to strongly dominate the most of the lower main thermocline study region (Pollard et al., 1996)."

p. 774, l. 19: I think Álvarez et al. (2004) is not the right reference here; instead, Machín and Pelegrí (2009) would be appropriate.

Reply:

We agree. The reference is replaced as suggested.

p. 775, l. 8: my understanding is that the MUC transport increases by a much larger factor, please check.

Reply:

We agree, the entrained volume was underestimated. From the Gibraltar strait west, the MUC transport is of order of 0.8-1 Sv and reaches 3-4 Sv at the Potimao Canyon, increasing its transport 3-5 times. The phrase is changed to: "The entrainment increases the MUC transport to 3-5 times its initial value."

p. 775: I believe the authors want to say that the final neutral buoyancy depth of the MW depends on whether we are looking at the MUC or at the seaward spreading MW, but the paragraph is poorly organized and gets confusing.

Reply:

The paragraph is restructured and separated in 2: the first one describes the MUC, while the second one describes the MW away from the continental margin.

"The MW is formed from the outflow in the Atlantic of the anomalously saline and warm Mediterranean Sea water. The MW has low oxygen and nutrient content. From the Gibraltar Strait the MW first spreads as the concentrated Mediterranean Undercurrent (MUC), trapped by the continental slope of the Iberian Peninsula. Over the first 100 km the MUC deepens from around 100 m just off the Gibraltar Strait to its neutral buoyancy level at 1200-1300 m depth (Baringer and Price, 1997). The rapid sinking is accompanied by a strong mixing with the ambient NACW and the upper North Atlantic Deep Water and with the entrainment of both. The entrainment increases the MUC transport 3-4 times of its initial value. Therefore, the original Mediterranean Sea water properties are strongly modified in the Atlantic MW (Price et al., 1993; van Aken, 2000b; Barbosa Aguiar et al., 2014). The deeper MW fractions mostly leave the continental slope south of Estremadura Promontory (39° N) to spread west and southwest, while the lighter fractions follow the continental margin further north to join the North Atlantic Current northwest of the Irish continental slope (Daniault et al., 1994; Mazé et al., 1997; Iorga and Lozier, 1999a).

After leaving the continental margin, the MW mostly spreads west to the Azores rise. This water sometimes is called the "pure" MW. Its core is at 800-1000 m. The lower fractions of the MW, presumably a mixture with the North Atlantic Deep Water, preferably spread west south of the Azores Current at 1000-1200 m (Harvey and Arhan, 1988; Iorga and Lozier, 1999a; van Aken, 2000b). The MW influence is the most pronounced from 700 to 1500 m, but some weak MW influence is detected from 500 m down to 3000 m depth (Tsuchiya et al., 1992; Arhan et al., 1994)."

p. 775, l. 16-17: this is a tautological statement.

Reply:

The phrase is changed to:

"After leaving the continental margin, the MW mostly spreads west to the Azores rise. This water sometimes is called the "pure" MW."

Section 2.1: the authors refer to the MEDTRANS climatology with no explanation of this data base. Given its relevance to the paper, this data base deserves some description.

Reply:

We changed this part of this paragraph to have more detailed description of the MEDTRANS climatology. Still the already large size of the manuscript does not permit to give all the details of the climatology, fully described and compared to other climatologies in a 22-page paper, referred in the text:

"Temperature and salinity, the principal parameters for water mass identification in the OMP analysis, were downloaded from the regional MEDTRANS climatology which has spatial resolution of 30 km × 30 km × 25 m (http://www.mare-centre.pt/en/research/medtrans-data). Nutrients (nitrate, phosphate and silicate) and oxygen are not available in the MEDTRANS climatology and were downloaded from the World Ocean Atlas database (WOA13, http://www.nodc.noaa.gov/OC5/woa13). Both the MEDTRANS and the WOA13 climatologies are based on the NODC data-set and use Barnes' Optimum Interpolation analysis (Barnes, 1964) for gridding, but the MEDTRANS climatology uses gridding along the neutral density surfaces instead of fixed z-levels in the WOA13. This has advantage over WOA13 in the representation of temperature and salinity distributions in the areas with sharp horizontal gradients of water characteristics: in particular, of the Mediterranean Undercurrent (see for further details Bashmachnikov et al., 2015a). The hydrological and nutrient data are merged into the common grid with the horizontal resolution of 30 km and the vertical resolution of 25 m."

p. 777, l. 14: the authors should avoid statements such as “presents a problem” and “has been studied with insufficient detail”.

Reply:

This phrase is removed from the manuscript.

p. 777: “Argo” rather than “ARGO”

Reply:

Thank you, this is corrected.

p. 778, equation is wrong. The authors probably mean the thermal wind equation, please see e.g. page 217 of Gill (1982).

Reply:

The two typos in this equation are now corrected: for z-up (as used here) the sign of the right-hand side is negative; *V* is the velocity vector - the vector sign is added above V in the left-hand side.

p. 778, l. 26-27: clarify the meaning of “95

Reply:

“ z_{95} is 95% confidence interval of the Student’s *t*-distribution”

p. 779, l. 3: “sea floor bottom”

Reply:

The expression is changed to “topographically trapped flows”

p. 780, equation (1): wouldn’t it be simpler to write “O” instead of “O2”, “N” instead of “NO3-”, etc.?

Reply:

We would prefer to keep standard convention as in the manual by Tomczak and Karstensen (http://omp.geomar.de). To facilitate reading the equation we used concentration sign $[PO_4]$ to separate the substance notations from their sequence numbers in the system of equations (1).

p. 780, l. 13: the authors probably mean delta-P, which is yet to be defined.

Reply:

“The unknown parameters are the source water type contributions $x_1, ..., x_5$ and the phosphate remineralisation rate ΔP .”

p. 781, 782 and Introduction: an important reference is Pastor et al. (2012).

Reply:

Thank you, this reference is included in the text.

p. 784: I understand this means the authors are using the same SDs for all water masses. If so, it needs to be justified.

Reply:

In the sensitivity experiments, for setting the variation limits for each of the parameters, we first computed SDs of each of the parameters for individual water types. Since the differences in the SDs between different water types for the same parameter are within a factor of 2, and since the definitions of water characteristics in by each of the authors has a subjective component, in the sense of their dependence on the geographical position of the study region, we simplified the sensitivity exercise, using the same mean SDs for all water types. We consider that this does not strongly affect the results of the sensitivity analysis.

p. 784, l. 18-20: please clarify.

Reply:

We changed the phrase to: "On the other hand, the errors of the sum of percentages of the LSW and the $NADW_u$, or the sum of percentages of all the fractions of the NACW, are within the 10% limit (Table 3)."

p. 784, l. 22: NADW has already been defined.

Reply:

Thank you, only the abbreviation, the NADW, is left in the text.

p.785, l. 21: these are no flux units.

Reply:

The sentence is changed to: "The SAIW core is found between the neutral density surfaces 27.65 and 27.80 kg m^{-3} (Fig. 5c,d). It deepens southeast with the isopycnals, but there also exists a diapycnal flux, which results in an increase of the SAIW core density by about 0.1 kg m^{-3} over 1000 km distance, from 34-45° N to 33-35° N (Fig. 5d)."

p. 786, l. 9: "the transitions between which occur in a jump", probably you mean "with abrupt transitions from one surface to another".

Reply:

Thank you, this is corrected.

"three different", "three MW", etc., i.e. one-digit numbers are to be written in letters except when followed by units.

Reply:

Thank you, this is corrected.

786, l. 20: again I believe Álvarez et al. (2004) is not the right reference here; instead, Machín and Pelegrí (2009) would be more appropriate.

Reply:

Thank you, this is corrected

788, l. 8 and 27: "lack" instead of "luck".

Reply:

Thank you, this is corrected

790, l. 25-27: please clarify.

Reply:

The phrase is changed to "Meddies conserve the original MW properties far away from the Iberian margin and the plaices of their rapid decay should have higher MW contents as compared to the surrounding areas. In particular, meddies together with lateral mixing and the MW recirculation, may partly compensates the effect of the eastwards advection of the NACW/LSW water along the Azores Current and reduce the gradients of the MW contents across the current."

792, equation (3): V is wrong as defined, please check and correct.

Reply:

The velocity vector ($\vec{V}$) was erroneously defined as "the modulus of current velocity". This is corrected. Otherwise the equation is a direct consequence of potential vorticity conservation a layer of constant density of the frictionless ocean, as stated in the reply to the General comments.

792, l. 12-13: dashes are incorrectly placed.

Reply:

The dashes are removed.

792, l. 21-22: Vincent, Vincente or Vicente? I recommend to use the name in the language of the country where the geographic location is located; in this case, hence, it should be "Cape São Vicente", as the authors already do e.g. with "Cadiz" (most of the time, though not always, they use this form rather than "Cadis").

Reply:

We keep everywhere St. Vincent, as the more commonly used name of the Cape in scientific literature.

p. 793: another relevant reference is Pastor et al. (2012).

Reply:

Thank you, the reference is added.

p. 795, l. 5-6: how can the density be larger?

Reply:

The reviewer probably means the phrase "Our results suggest that 3 MW cores can be detected, separated by continuous lines of sharp gradients of the core depths/densities".
This is changed to: "Our results suggest that three MW cores can be identified, separated by continuous transition lines: across each of the transition lines the MW core density and depth change abruptly (Fig. 6b and Figs. 6c-7c)."

p. 795, l. 795: this is difficult to assess because the authors did not detail the characteristics of the MEDTRANS dataset

Reply:

The reviewer probably means the phrase: "The MEDTRANS data-set fairly well represents the MW salinity in the MUC only downstream of 8-9° W (between Portimao Canyon and Cape St.Vincent), where the width of the MUC reaches 80 km and is comparable to the data-set gridding radius (Bashmachnikov et al., 2015a)."

To representation of the MUC in the MEDTRANS data-set is devoted a whole section in Bashmachnikov et al. (2015a), to which we send the reader.

p. 796, l. 25: another relevant reference is Machín and Pelegrí (2009).

Reply:

Thank you, the reference is added.

p. 797: as stated by the authors, there is much mAAIW in most of the southern boundary of the domain, in contrast to what previous works have shown. The authors should clarify that this is probably the consequence of their local definition for AAIW.

Reply:

Our study does not contradict the conclusions of qualitative analysis by Tsuchiya, 1989; Tsuchiya et al., 1992 and other authors, who detected generally zonal extension of the northern boundary of the AAIW boundary. The woks, where the OMP analysis is performed, as Pérez et al., 2001; Llinas et al., 2002; Alvaréz et al., 2004; Machín et al., 2006; Machín and Pelegrí, 2009; Pastor et al., 2012, are covering the areas along the African margin and the surrounding waters. In this particular area our results of the AAIW northward penetration are consistent with those studies. Our definition of the AAIW characteristics is close to the definitions by the authors above, and the only difference in the results may arise due to use of the 50-year mean climatology in this study instead of recent synoptic sections in the previous works. The new results are mostly obtained for the area northwest, away from the Canary Islands. This area has not been previously covered with the OMP analysis.

Caption Table 2: "The upper limit of the error comprises 99

Reply:

Now Table 3. The caption is changed to: "The upper error limit corresponds to the value, which is above 99% of the errors at all grid-points and at all depths levels."

Table 3: If this table is placed earlier in the manuscript then it will not be necessary to spell out the acronyms in the current Table 1.

Reply:

We agree. Table 3 is placed as Table 1 in the beginning of the manuscript.

Fig. 1: I would suggest keeping one same colour code for panels c and d. What do you mean by "illustrative values"?

Reply:

The colour scale is changed accordingly. The phrase in the caption is changed to: "*The isopycnals in plate (a) are referenced to 1000 m (σ_1)*".

Fig. 3: Is it possible that there is more than one depth level with 50

Reply:

For the NACW, based on climatological distributions of water properties, this does not happen. The NACW percentage decreases down monotonically.

Fig. 4: here and elsewhere, I would suggest removing these "magenta lines" as they are probably subjective.

Reply:

The arrows connect areas with high water mass content with this with low water mass content. They mark the general direction of the water mass transports. The conclusion directly follows from the principal OMP assumption: the water mass contents decreases along the transport path due to diffusion only. This follows a standard procedure for detection of the general water transport patterns at depth in the absence of other data (see, for example, Tomczak and Godfrey, 2003).

Fig. 9: what does it "dominant" mean? To me it means the water mass with the largest contribution but at the end of the caption the authors say that "only water mass contents over 25

Reply:

The beginning of the caption is changed to: “*Distribution of water masses, the contribution of which exceeds 25%*”.

Fig. 11, right panels: I have difficulties trusting these arrows and numbers, the authors would need to convince me. Additionally, I suggest keeping these right panels the same size as the left panels.

Reply:

New Fig.12. The AAIW or the MW spreading patterns in the Atlantic are actually derived, using the same procedure. We just add details to the spreading patterns. We also used the property that the isopycnals in this part of the study region and at those depths are quasi-horizontal. Thus, we can use pressure levels to depict along-isopycnal spreading of water.

In the new version we applied a more robust algorithm: instead of following the maximum percentage at a depth level, we compute the mean of the positions of the water mass contents within 5% limit from the maximum along a section, weighted with the respective percentages. A more complete description of the procedure is inserted in the text as below:

“In Fig. 12(b,d) the lines trace the maximum percentages of the MW and of the mAAIW along meridional and zonal sections, respectively, for selected depth levels. Without pretending giving details of the advection pattern at depth, the lines show a general direction the advective-diffusive transport of the water masses (see also Tomczak and Godfrey, 2003). This is based on the following assumptions: the general direction of the MW spreading is westwards and of the mAAIW – northwards; the maximum efficiency of dilution of a water mass, being mixing with other water masses, goes at the edges of the propagating water body. To add robustness to the computations the lines connect the points, each of which represents the mean of the positions along a meridional or a zonal section, weighted with the respective percentages. For the computations, only the parts of the sections over the section maximum percentage menus 5% are used. The lines obtained generally follow the principal mid-depth advection patterns (Fig. 12a,c). This supports the assumptions behind the computations.”

Additionally we present 2 maps with the percentages of the MW (Fig.I) and the mAAIW (Fig.II) at 900 m depth, which clarify the procedure of drawing the lines of the water type propagation in the Figure.

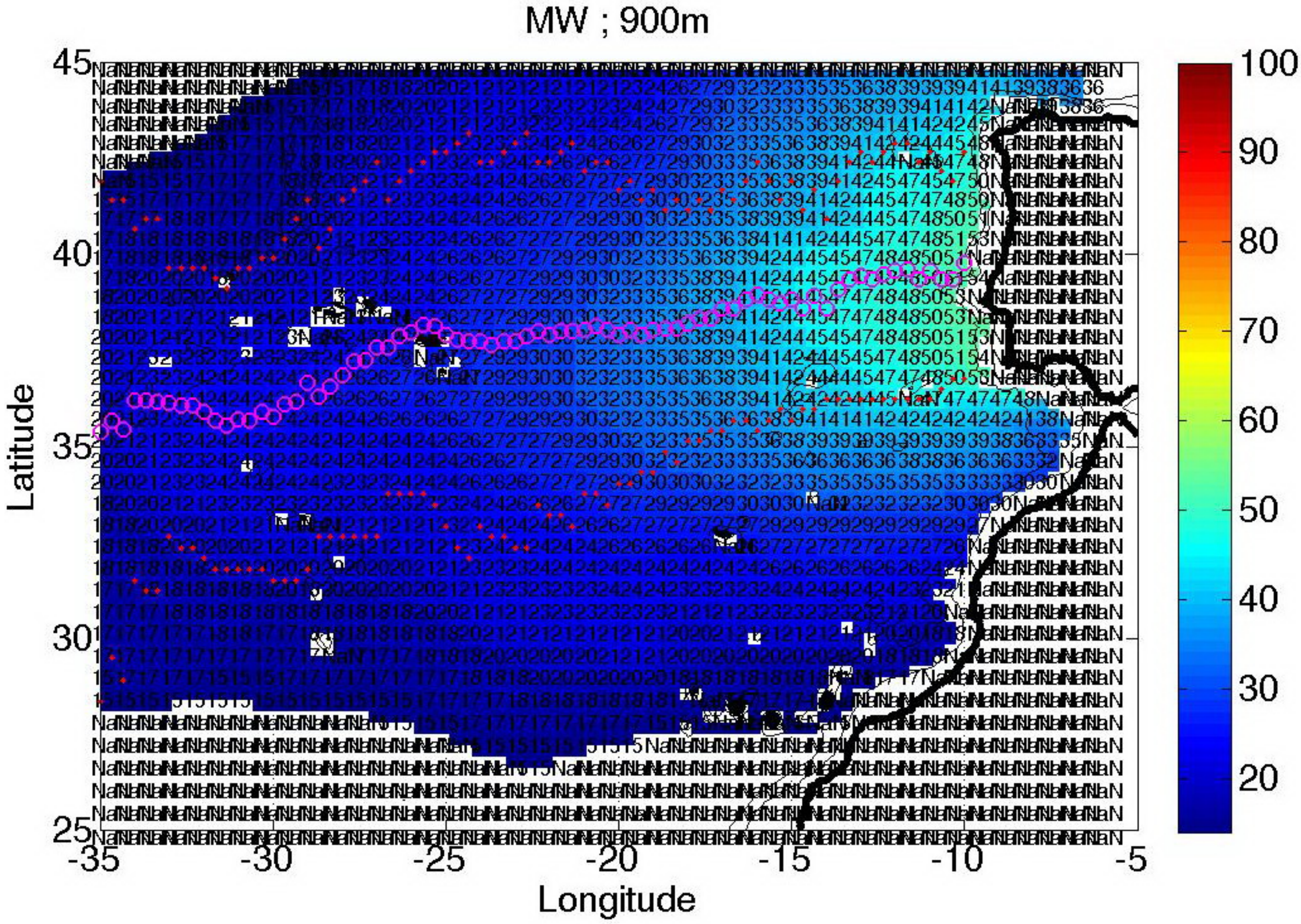

Fig.I The MW percentages at 900 m depth (colour, numbers). The upper and the lower limits of the area of the maximum percentage minus 5% are marked with red dots. Magenta circles mark the path of the MW preferable propagation from the Iberian Peninsula.

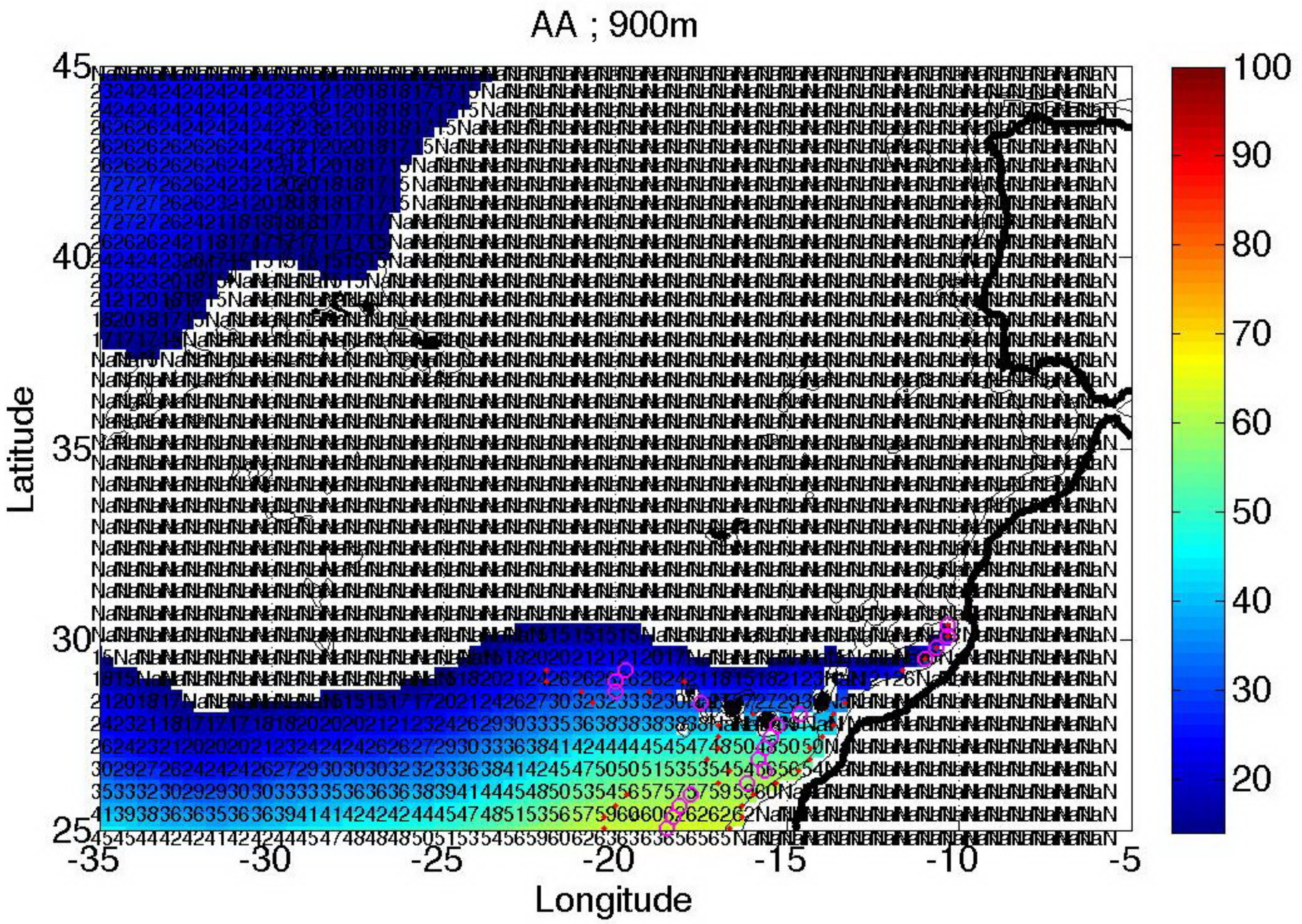

Fig.II The same as Fig.I, but for the mAAIW at 900m depth.

Fig. 12, panels b-d: what is the relative intensity of the diffusive fluxes? i.e. relative to what? The yellow contours are not labelled. Small legends on top of many figures are hardly legible. Several figures use AA instead of mAAIW.

Reply:

We replaced the beginning of the caption with "*The role of advective and diffusive fluxes in spreading of water types.*"

Yellow contours are the percentages of the water types. There role is to show weather the advective fluxes follow the isolines of equal concentration, or are perpendicular to the isolines. The absolute values are presented in Figs 5a, 6a and 8a, which is now stated in the figure caption.

Small legend on the top of the figures are removed in the figures.

AA is replaced with mAAIW in the figures.

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