Ocean Sci. Discuss., 12, C339–C348, 2015
www.ocean-sci-discuss.net/12/C339/2015/

Ocean Science
Discussions
Open Access

Interactive comment on "Distribution of intermediate water masses in the subtropical northeast Atlantic" *by* I. Bashmachnikov et al.

J. L. Pelegrí (Referee)

pelegri@icm.csic.es

Received and published: 26 June 2015

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 de-

scription 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.

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).

- 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.

- 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.

- 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.

- 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.

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.

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.

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.) 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.

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. 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.

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.

Minor issues

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

p. 772, l. 22: ENACW

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.

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

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

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.

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

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.

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

p. 777: “Argo” rather than “ARGO”

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

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

p. 779, l. 3: “sea floor bottom”

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

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

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

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

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

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

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

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

p.786: “three different”, “three MW”, etc., i.e. one-digit numbers are to be written in letters except when followed by units.

p. 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.

p. 788, l. 8 and 27: “lack” instead of “luck”.

p. 790, l. 25-27: please clarify.

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

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

p. 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”).

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

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

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

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

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.

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

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.

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

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

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

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

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.

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.

References

Álvarez, M., Pérez, F. F., Bryden, H., and Ríos, A. F.: Physical and biogeochemical transports structure in the North Atlantic subpolar gyre, J. Geophys. Res., 109, C03027, 2004.

Castellanos, P., Pelegrí, J. L., Campos, E. D., Rosell-Fieschi, M., and Gasser, M.: Response of the surface tropical Atlantic Ocean to wind forcing, Progr. Oceanogr., 134, 271-292, 2015.

Gill, A. E.: Atmosphere-Ocean Dynamics, Academic Press, New York, 1982.

Machín, F. and Pelegrí, J. L.: Northward penetration of Antarctic intermediate water off Northwest Africa, J. Phys. Oceanogr., 39, 512–535, 2009.

Pastor, M. V., Peña-Izquierdo, J., Pelegrí, J. L., and Marrero-Díaz, A.: Meridional changes in water mass distributions off NW Africa during November 2007/2008, Ciencias Mar., 38 (1B), 223-244, 2012.

Rosell-Fieschi, M., Pelegrí, J. L., and Gourrion, J.: Zonal jets in the equatorial Atlantic Ocean, Progr. Oceanogr., 130, 1-18, 2015.

Zika, J. D., McDougall, T. J., and Sloyan, B. M.: Weak mixing in the eastern North Atlantic: an application of the tracer-contour inverse method, J. Phys. Oceanogr., 40, 1881–1893, 2010.

Interactive comment on Ocean Sci. Discuss., 12, 769, 2015.