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Interactive comment on "Detailed temperature–salinity distribution in the Northeast Atlantic from ship and Argo vertical casts" *by* I. Bashmachnikov et al.

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We would like to thank Referee 2 for detailed comments and suggestions which improve the manuscript. The responses to the comments and the changes introduced are listed below.

Main comments:

The Authors do gather data of 3 data sources and further reïňĄne existing mapping methods and append them with novel ideas to develop a climatological hydrographic map of the NE Atlantic. The manuscript is divided in brieïňĆy two parts, one describing the new climatology, the other the regional features of the North East Atlantic. This division makes it hard for the reader to follow trough the manuscript. The authors should put more emphasize on a continuous story, focusing more on either topic. In my opinion the manuscript would improve in readability if it describe the climatology in detail, with the results of regional features interspersed within the appropriate comparison paragraphs. Despite their promising results and valuable ideas, the current manuscript lacks in detailed description of the methods applied. Yet it is not possible to reproduce or follow the authors through their ïňĄltering and mapping procedures with the details provided.

Response: We detailed the description of methods following the referee comments further on. With those additions, description of the method is the principal part of the manuscript. Comparisons of the results of the gridded climatology with the known features of the study region allow evaluation of validity of the results, obtained in the present climatology in comparison with those obtained in alternative climatologies. Regional descriptions and comparision between climatologies (Section 3) follow the corresponding description of the method (Section 2).

Concerning the language: It would be preferable if the complete manuscript would be written in present, not varying between present and past. The use of climatic is a bit confusing, would it be more appropriate to use climatological? If possible, have a native speaker proof read the manuscript (this does not impact my scientiïňĄc opinion of your work, which is very interesting!)

Response: The manuscript is changed to keep all its parts in present; typos are corrected.

My comments and questions in detail:

Abstract: The two-parted abstract is confusing for the reader, while the ïňĄrst part is solely on the mapping, the second part is very regional. The Authors should try to focus the abstract on one of these topics. E.g. Focus on the data set and describe the climatology with a brief comparison of the regional features discussed, or focus on the

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regional features and describe the climatology more focused.

Response: The description of regional features is mostly removed from the abstract. Short description of the gridding results is left to compare the known regional features with those obtained with the presented methodology.

Introduction: The introduction does not ïňĄt to the abstract, while the abstract starts with the climatology, the introductions extensively deals with regional features.

Response: We agree that this is a certain drawback of the Introduction. Still the Abstract follows the structure of the whole paper, which is constructed in the sequence: Methods- Results. Introduction presents the existing climatologies and inter-compare them, describing to what extent the climatologies reproduce the well the known thermohaline and dynamic features in the study region. In this sense, we believe that it is easier for a reader first to get an overview of the regional features first. At the same time, we agree that description of the regional features contained a number of details which are not used for the comparison. In the new version of the manuscript, the regional description is significantly reduced, limited to description of the most important frontal zones and currents in the region.

p1474-17: more -more than what?

Response: We changed the phrase to: "The obtained MEDTRANS climatology gives more details of the distribution of water characteristics in the Subtropical Northeast Atlantic than other alternative climatologies and is able to reproduce a number of dynamic features described in literature."

p1475-7 'was felt necessary' -please rephrase Methods: The methods lack in necessary detail of the steps taken to compute the climatological ïňĄelds, please be signiïňĄcant more precise!

Response: We changed the phrase and moved it to the end of Introduction: "The methodology of this study (described in Section 2) complements the existing gridding methods (WOA09, WOA13, Schmidtko et al., 2013, Troupin et al., 2010) and the resulting climatic fields provide additional details on temperature-salinity distributions (described in Section 3)." Additional details are further given in Methods, now sensibly extended, and Results.

p1479-4-7 'comparing' -you write you compare the proïňĄles but give no information of how you compare them, please be precise.

Response: We changed the phrases to "The first step consists in screening for major instrument malfunctions or calibration errors. This consists in verifying whether vertical temperature/salinity profiles significantly deviate from the WOA09 climatological profiles interpolated to the observed profiles' positions. Assuming normal distribution of temperature/salinity deviations from climatology at any depth level, a profile is considered to be bad if the whole profile deviates from the climatological mean profile by more than 5 standard-deviations. Mean standard deviations of 0.07 for salinity and 0.35ïĆř C for temperature, derived from WOA09 data-set, are used to form the criterion."#

p1479-7 'a part of the proïňĄle' how do you deïňĄne 'part' is there a minimum or maximum length? data points? percentage of data points limit?

p1479-7 'spike' -since you talk about density inversions I assume you mean density spike? What about spikes in T & S that are density compensating?

p1479-9 'deviates from the climatological mean proïňĄle for more than 5 standard deviations' -standard deviation of what data? Which climatology? How is this mean proïňĄle computed, derived? Why is 5 standard deviations used? Was this analysis used on isopycnals, if on isopycnals, how do you deal with the outcropping?

p1479-9 'top 100m and the MW layer' why are these layers excluded? Since they have signiïňĄcant larger data spread from the start, a 5 standard deviation ïňĄlter should work here as well. -How is the MW layer deïňĄned here? Is the MW layer not ïňĄltered in your whole domain?

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Response: The data were filtered at fixed depth levels. This is now explicitly indicated in the text: “A quality control procedure was applied to all z-level profiles from NODC data-set, attributing different flags to the data-points according to their quality.” Additional filtering, performed on neutral density surfaces, further on starts with the phrase “The gridding is done on neutral density surfaces (McDougall, 1987; Jackett and McDougall, 1997).” (p.7-15) The paragraphs describing the initial filter is, in fact, not clear enough and are changed to: “Assuming normal distribution of temperature/salinity deviations from climatology at any depth level, a profile is considered to be bad if the whole profile deviates from the climatological mean profile by more than 5 standard-deviations. Mean standard deviations of 0.07 for salinity and 0.35ïĆř C for temperature, derived from WOA09 data-set, are used to form the criterion. If only a part of the profile deviates from WOA09 over 5 standard deviations, the profile is considered “dubious” and the data reliability is verified by eye-checking. The expert decision is made based on persistence of the same “dubious” structures in other local profiles. The filter is not applied for the upper 100-m layer and the MW layer (700-1500 m), since extreme natural deflections from WOA09 climatology in those layers significantly exceed the 5 mean standard deviations used as the filtering criterion.” This was a very rude initial filtering to eliminate the most obvious errors in the profiles, including errors in calibration of the sensors. To set-up of the criterion we used the mean standard deviation over the whole area. Eye screening of the profiles showed that this criterion is often violated in the upper 100 m (close to the upper mixed layer) and in the MW layer, especially near the Iberian coast. The errors in those layers were further filtered out using more sophisticated filters. Use of 5 standard deviations is a typical statistical criterion for outlier detection in normally distributed data, as temperature and salinity are. In the normal distribution 99.7% of data lie inside 3 standard deviations. Thus, there is a very high chance that whenever some value deviates from the mean over 5 standard deviations, something is wrong with the data.

p1479-14: ’the attempt to correct’ -please describe how you correct data!

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Response: Now the description is extended to explain better the procedure: “For all cases we tend to correct first the salinity spikes, since salinity is typically more vulnerable to observational errors, and then, if necessary, the temperature spikes. Whenever the correction leads to an increase of local standard deviation around the spike in either temperature or salinity, the correction is not applied.”

p1479-17: ’eye-checked to make an expert decision’ -please state the criteria for this procedure, why is it necessary? How many proïňĄles were removed and why? Were all proïňĄles visually controlled, since you state 54429 proïňĄles in table 1, I doubt it is possible to visually inspect all of them.

Response: Only profiles previously marked as “dubious” were eye-checked. The phrase is changed to: “If only a part of the profile deviates from WOA09 over 5 standard deviations, the profile is considered “dubious” and the data reliability is verified by eye-checking. The expert decision is made based on persistence of the same “dubious” structures in other local profiles.” The number of profiles removed is listed in Table 1 (now extended), discriminated by tdata-type. From 96 to 99% of the original profiles were preserved to more than 50%, and 91 to 96% of the original profiles were preserved to more than 90%.

p1479-22: ’polynomially’: which order polynomial? Why not linear?

Response: We added the following phrases to explain those details: “If a data gap exists only in the temperature or in the salinity profile, and its size does not exceed 200 m, the gap is linearly (low resolution OSD) or polynomially (CTD and PFL) interpolated. In the latter case the piecewise cubic Hermite polynomial interpolation is used. This interpolation produces a resulting profile differentiable to the second order, which preserves its extremes and does not suffer from overshooting at the places of abrupt variations in the rate of vertical change of a measured property.”

p1479-22: the blanking of bad parts of the proïňĄle and interpolating over the gaps: How do you deal the top or the bottom of a proïňĄle? And what do you do if the gaps

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are larger than 200m?

Response: We added the phrase: "Whenever the vertical extension of a gap exceeds 200 m no interpolation is performed. Also no extrapolation is performed at the profile edges."

p1480-1" below 1900m: this is due to the Argo ïňĆoat diving depths. It would be worth adding the data source amount in ïňĄgure 1 as a stacked plot. Thus the reader can easily access the distributions by source.

Response: Yes, this is due to PFL data sample down to 1900-2000 m depth. The information on vertical distribution of the data quantity is now discriminated by data-types in Fig. 1d.

p1480-4 temperature data-points: since you do an isopycnal mapping further below, why do you, and how do you use temperature data with missing salinity data? Shouldn't be all T data with missing S data be blanked?

Response: This is true. Further on only data-points with both, T and S are used. We removed this phrase.

p1480-10 'were eliminated': How? please be signiïňĄcant more precise in your data handling. Further more it would be helpful to state the reason for this approach here. Why do you eliminate them and what is the beneïňĄt of this, how would the results be if you do not eliminate them?

Response: We followed the method developed by Richardson et al. (1991), now described in the text: "The casts across meddies are detected using Richardson's criterion (Richardson et al., 1991), i.e. a cast is considered to pass through a meddy, when a salinity anomaly of more than 0.2 over at least a 200-m layer is detected in the depth range between 500 and 1500 m." The temperature-salinity anomalies formed by meddies can be found as far as the Mid-Atlantic Ridge (and, seldom, on the other side) and they are exceptionally strong (up to 4ïĆřC in temperature and 1 in salinity). A

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number of ocean experiments have been performed to study meddies. Those data are included in the NODC. Therefore, in some areas, we may have a few dozens of casts through a meddy. If not eliminated this forms an artificial temperature-salinity anomaly, deviating the gridded ocean state from the true local climatology.

p1480-13: 'was excluded' why was this excluded, what is the reason for this and how is the area deïňĄned, which bathymetry used for this criterion?

Response: We extended the phrase: "The MUC also forms a strong local temperature-salinity anomaly relative to WOA09 climatology, since area of the MUC is not correctly represented in WOA09. To avoid confounding this climatic feature with meddies, the slope region off Iberia with depths less than 2500 m is excluded from this latter filtration." In fact, bathymetry data-source should have been mentioned in the beginning of the Method section. We added the following paragraph to the beginning of the section: "Bathymetry data, used for conditioning of some data-filters and for gridding procedures, are obtained from ETOPO2 data-set (http://www.ngdc.noaa.gov/mgg/fliers/01mgg04.html)."

p1480-15: 'Further experiments' -what kind of experiments? The reader does not know what you did here to come to your conclusion.

Response: "Experiments" is not a correct word in this context. We changed the phrase to: "Plotting temperature-salinity distributions for different depth levels, we found consistent positive anomalies of temperature and salinity (relative to WOA09 climatic profiles) in OSD profiles along some of XCTD routes, especially noticeable below the 1500-m level."

p1480-19-23: The sum of T & S deviations -compared to which climatology, deviations between the proïňĄle and what data? What does 'close to zero' mean, which threshold did you use? Why do you assume a random error, please discuss this hypothesis. What about systematical biases like: if one data source is more commonly used in one season while the other is year round etc? Since one data source is signiïňĄcantly

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biased towards historical data, might it be that you ïňĄlter out correct data from a different oceanic state?

Response: Deviations were taken relative to WOA09 data-set. We have 2 reasons to doubt the OSD results. First, in the same areas CTD and PFL data do not show any consistent deviation from WOA09. Second, there is a known drawback in algorithms for estimation of the depth of XCTD instruments (Levitus et al., 2008), which should result the same artificial warming and salinity increase, intensifying at deeper levels, as observed. Since we do not have information on the algorithm used to compute XCTD depth of any individual profiles, we opted to filter out those data. We performed mapping of those 3 types of data for different 5-year periods, as well as on seasonal bases, to assure this is not a part of interannual or seasonal variability (those results are not included in the manuscript). The results showed the pattern to exist exclusively in OSD data, which confirms the suggestion that the bias is artificial.

Any particular cast deviates from the climatological state in the point due to combination of small-scale, synoptic, seasonal and interannual variations. Experimental results suggest that distributions of a number of oceanographic parameters (including T and S) are close to the normal distribution. Therefore, a sum of deviations from the climatic mean should be close to zero, within the error of computing the mean. For normal distribution the error is proportional to standard deviation of the data, multiplied by the number of summed items. We further added the phrases: “For the normal distribution of the data deviations from climatology we expect the sum of the deviations to be close to zero, at least within the limits n*1.96*std/sqrt(n), where n is number of data-points and std is the typical standard deviation of temperature/salinity from climatology (see above). For the typical =30, we exclude OSD casts whenever the sum of the deviations from WOA09 climatology exceeds 1 in salinity or 4ïĆřC in temperature.”

p1480-23: Iterative ïňĄltering: what is this and how did you do it ...

Response: This phrase is removed, since the final algorithm used for this correction

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does not required iterations. Sorry for this typo.

p1480-24: How was the degradation of the salinity sensor of PFLs determined? Please be precise about your data handling!

Response: We added the phrase: “Sensor degradation is detected whenever the PFL salinity value starts persistently decreasing with time, at some final part of the float trajectory deviating to more than 5 climatic standard deviations (see above) from the climatology (WOA09) and/or from the mean of the surrounding PFL data over the same time period.”

p1480-25: ’Further gridding’ -why further? So far no gridding was described.

Response: This is corrected: “The filtered data entered into a gridding procedure. The gridding is done on neutral density surfaces (McDougall, 1987; Jackett and McDougall, 1997).”

p1480-26-28: I don’t understand the paragraph, neutral density was computed for each data point of a proïňĄle, but what kind of ’following’ neutral density surfaces are used?

Response: We changed the paragraph to: “The filtered data entered into a gridding procedure. The gridding is done on neutral density surfaces (McDougall, 1987; Jackett and McDougall, 1997). For this study, pressure, temperature and salinity distributions are obtained at 53 neutral density surfaces ( ): 25.50, 25.60, 25.80, 26.00, 26.20, 26.40, 26.60, 26.70, 26.80, 26.90, 26.95, 27.00, 27.05, 27.10, 27.15, 27.20, 27.25, 27.30, 27.35, 27.40, 27.45, 27.50, 27.52, 27.54, 27.56, 27.58, 27.60, 27.62, 27.64, 27.66, 27.68, 27.70, 27.72, 27.74, 27.76, 27.78, 27.80, 27.82, 27.84, 27.86, 27.88, 27.90, 27.92, 27.94, 27.96, 27.97, 27.98, 27.99, 28.00, 28.01, 28.02, 28.03, 28.04. The G- surfaces are selected to be fairly uniformly distributed in depth, keeping the mean distance between the surfaces less than 50 m in the upper 1500-m layer, and less than 100 m below.”

p1481-1 which ones were selected. using a maximum distance of 50m in the surface

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is quite large, if you plan on mapping 25m in the vertical.

Response: Distance between neutral density surfaces changes across the study region. Here we talk about the mean distance between the surfaces, which does not EXCEED 50 m. As it follows from figure 5 it changes from 10 m in the upper ocean to about 50 m in the mid-depth. We believe that the interval of 50 m or less in the upper 1500-m layer and 100 m - below, is sufficiently small to capture all features of vertical climatic structure of the ocean in the study region. After gridding T-S data on the neutral density surfaces we re-grid the results to z-levels with 25-m interval using Hermit cubic interpolation between the density surfaces.

p1481-3 'an additional ïňĄlter' what kind of ïňĄlter? how and on what data points was it used?

Response: We changed the phrase to: "After calculation of the neutral density surfaces, an additional filter is applied. The computed depth of a neutral density surface can change from one cast to another by as much as 50-400 m within short distances of a few dozens of km. This can be due to a jet-like mean current, as well as due to remnant instrumental errors and eddy noise."

p1481-8 'parameters' which parameters?

p1481-10, please deïňĄne all terms in formulas used. dI is not deïňĄned. is dz used in between all available isopycnals, or over a larger vertical range?

Response: We changed the phrases and defined the parameters: "Removing points with along-isopycnal pressure gradient over some critical value, limits the vertical velocity variation (dV) across a G-surface: dV/dz=N2/f*dG/dl, where N is buoyancy frequency, f is Coriolis parameter, and l is the direction along the G-surface." dz is some small distance in vertical direction across the neutral density surface.

p1481-10-19: if you use varying dV, please show a map how this varies and justify the use of such an approach. Please discuss how your results would vary if this ïňĄlter

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would not be applied, are your results biased by a locally strongly adjusted ïňĄltering?

Response: The filter designed to detect rapid variations in depth of neutral density surfaces. As it was stated in the paragraph, dG/dl of 200 m per 200 km is chosen as the critical value. The rest of the paragraph discusses consequences of such filtering. In particular, we examine a possible artificial reduction of horizontal current velocity gradients (dV/dz) by the filter. The selected critical dG/dl clearly exceeds the expected gradients of dV/dz even for the strongest currents in the region. This justifies that the filter only removes local outliers, but does not distort the structure of large-scale flows through artificial over-smoothing of isopycnal depths. The description is given in the text.

p1482-1: Since it is crucial for your mapping, please deïňĄne your e-folding scale R and discuss your reasons for using the R you do use.

Response: In this place we just describe the gridding method. Definition and discussion of the parameters used, including R, is done a few paragraphs below (p. 12)

p1482-3-10: please make sure all terms of your equations are deïňĄned. They are not yet, currently the reader has to assume some of your abbreviations and terms in the equations!

Response: Definition of the wavelength “alfa” was missing and now this is corrected. The rest of the notations are defined.

p1482-13: provide reference. p1482-17 reference of formula?

Response: Reference is inserted: Pedlosky, J. (1987), Geophysical fluid dynamics, 2nd ed., Springer, New York, 1-710.

p1482-21: 'stretched' how did you stretch the Gaussian weight function along isobaths? Schmidtko et al. use a bathymetry following algorithm, is the same applied here, how do you deïňĄne the distance between data points over rough topography? Whish topography is used?

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Response: Stretching is done by modification of the shape of the weight function (see equation 2 and description below). This modification of the weight function (W) by topography is discussed in the next paragraphs (see also examples in Fig. 2). We adopted the form of the depth-dependent function A_k (see description below equation 2) from the paper by Schmidtko et al. (2013), and further modified it to get the bottom-slope dependent weight distribution, influencing the gridding point. Bathymetry is obtained from ETOPO2 data-set, which is now indicated in the second paragraph of the Methods.

p1482-22: 'automatically' -how? What does automatically mean in this context?

Response: By the use of parameter "c". The paragraph below equation (2) presents the expressions used for this procedure.

p1483-9: w (see below) -where below?

p1483-22: 'Care has been taken' -how?

Response: This is discussed in the paragraph at p1483-22 - p1984-4. This paragraph is modified: "It is noted that the expression (2) applied over steep topographic slopes strongly reduces the area with high weights (Fig. 2c, with w=1). In the areas of very steep bottom slope (continental margins, banks or seamounts), the resulting mean weight over the gridding window may decrease to as much as 30%, increasing the noise level of the gridded climatology near the topography. Further on, this noise manifests itself in strong divergence of the computed geostrophic flows, for example. To avoid this effect, the parameter <=1 is introduced as the ratio of the mean weight inside the gridding window (2) with locally computed c, to the mean weight with c=0. Use of this parameter in expression (2) increases the weights within the gridding window, not affecting the mean gridding radius (Fig. 2d)."

p1483-18-20: please give references for your values used.

Response: With the ratio N/f∼80-90 was derived from WOA09 data-set. Now this is

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stated in the text.

p1484-8: give reference!

Response: Reference is inserted: Kantha, L.H., and C.A. Clayson (2000), Numerical models of oceans and oceanic processes. International Geophysics series v.66, Acad. Press, San Diego, 1-887.

p1484-29: How do you deal with coastal regions with less than 30 data points? Or near steep topography when less than 30 data points are available on a given isopycnal layer. -Same for the upper ocean, near outcropping of any given isopycnal, how is the outcropping region handled?

Response: We computed the size of the window that contains at least 30 points around every data point. Fig. 3 presents the results of such calculations gridded by linear interpolation. It has an illustrative value. The function of influence (defined by the weight function W) is stretched along the steep slopes. The problem of some reduction of the area at very steep slopes is assessed by an increase of the weights within the gridding window (inserting parameter w in eq.(2) – see the modified paragraph in the top of p.11). The procedure assures sufficient number of grid-points used for computation of climatic means. The problem of reduction of number of points near the land masses is assessed by positioning of the closest grid points at sufficient distance from the coastline (adopted from AVISO grid). The problem of isopycnal outcropping is not considered in the gridding procedure. This may induce some additional noise near the bottom and the sea-surface. At the same time, near the bottom isopycnals have small inclination angle and the final gridding is done down to the minimum depth of the lowest isopycnal. In the upper 100-m layer isopycnal gridding is substituted with isobaric gridding. This is now stated in the text: "In the upper 100-m layer, water stability is generally low impeding robust computation of neutral density surfaces. In this layer the isopycnal gridding is substituted by isobaric gridding (Gouretski and Kolterman, 2004)."

p1485-1 How is your R deïňĄned? this is described below, please rewrite so the reader

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can follow easier through the manuscript.

Response: R is defined as a function of data density and eddy radius. We transferred the text on eddy radii in the region to Appendix 1. Thus, the main text becomes shorter and easier to follow.

p1485-19: why are R values a proxy for eddy radii? The deïňĄnition of R in the paragraph above states that R is more a proxy for data density, especially in the vertical!

Response: To justify the relation to eddy radius we included the paragraph in the text: “Mesoscale and submesoscale anomalies of water properties are one of the main sources of noise in climatic data. The effect of eddies is partly removed by the filtering procedures described above. At the same time, some remnant eddy noise still affects the temperature-salinity fields. To further reduce this noise we chose the radius of the gridding window considerably exceeding the typical eddy radius (Appendix 1).”

We agree with the referee that in the central part of the region the limitation based on data density is very close to that based on eddy radii. Therefore, we changed the paragraph at p1486-21-24 to: “Combining the information above (Figs. 3 and A1), we see that in the upper 1900-m layer in the eastern part of the study region, Ris limited by eddy size, while over the rest of the region it is limited by data density. From 2000-m level down R should be doubled, everywhere limited by the data density.”

p1486-1st paragraph: why this extensive eddy size discussion?

Response: The discussion is important, but complicates following the manuscript. We moved this discussion to Appendix 1.

p1486-17: ’our results’: I cannot follow how and why this study to this point in the manuscript can conclude about eddy sizes.

Response: The discussion is indeed too long. We condensed the discussion into one shorter paragraph and moved it to Appendix 1: “The histogram of the decorrelation scales, a proxy for eddy radii, (Fig. 4) shows that the scales have two modes: at 10-20

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km and around 30 km, and ranging from 10 to 100 km. The latter mode is close to the Rossby radius of deformation in the study region (Emery et al., 1984). The distribution of the decorrelation radii does not show any significant variations across the study region, neither with latitude, nor with the proximity to steep topography. The latter values compare well with the previous studies in the region. In-situ observations suggest eddy radii between 40 and 100 km in the area of the AzC (Gould, 1985; Pollard and Pu, 1985; Pingree and Sinha, 1998; Alves and Verdiere, 1999; Alves et al., 2002; Pingree, 2002; Mourino et al., 2003) and 10 to 60 km in the northern part of the study region (Arhan and Colin de Verdiere, 1985; Mercier and Colin de Verdiere, 1985; Shoosmith et al., 2005), as well as in upwelling area near the Iberian peninsular (Pingree and LeCann, 1992; Oliveira et al., 2004). Derived from the satellite altimetry, the characteristic eddy scales in the study region, are estimated to be 40-80 km (Le Traon et al., 1990) and 60-100 km (Stammer, 1997; Jacobs et al., 2001; Chelton et al., 2011). The overall larger radii derived from the AVISO altimetry are biased by the cut-off length of 40-50 km, below which eddies cannot be detected with the gridded altimetry data. In summary, for further reduction of eddy related noise we shall choose R significantly exceeding the characteristic eddy radius of 30 km and preferably exceeding the maximum eddy radius of 50-100 km. The value of 60-70 km looks to be a reasonable compromise between the urge towards the maximum spatial resolution and the maximum reduction of mesoscale noise level. ”

p1486-24: ’twice’ -twice the size of what?

Response: We changed the paragraph at p1486-21-24 to: “Combining the information above (Figs. 3 and A1), we see that in the upper 1900-m layer in the eastern part of the study region, Ris limited by eddy size, while over the rest of the region it is limited by data density. From 2000-m level down R should be doubled, everywhere limited by the data density.”

p1486-27: ’1900m layer (G>27.96)’ -what was used, an isobaric or isopycnal criterion? Since the neutral density surface is varying in depth over the size of your domain, these

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are not identical or close everywhere.

p1486-29ff see comment above. Further more are these increases of R done as a step function or linearly increased over increased rho (or depth, depending on what you did?!).

Response: In the study region the neutral density surfaces are nearly isobaric at those depths (see zonal and meridional sections in fig. 6). So we have chosen the surfaces with the mean depth to be the closest to the referred levels. Increase of R is done as a step function.

The paragraph is changed to: “To get the highest possible resolution with the decreasing data density away from the Iberian Peninsula, we used spatially varying R. For G less or equal to 27.96 kg m-3 (in the upper 1900-m layer), is set to 70 km near the Iberian Peninsula and to 200 km in the open ocean. Both radii increase 1.5 times for G from 27.97 to 27.98 kg m-3 (in the layer 1900 to 2000 m depth) and double for G more or equal to 27.99 kg m-3 (below 2000 m).”

p1487-3 ’smooth transition is assured’ -how?

Response: The phrase is changed to: “A smooth transition between the areas with the minimum and the maximum radii is assured by gradual variation of R across the study region (Fig. 4a-b)..”

p1487-10 prior to the ïňĄnal regridding on density surfaces if would be interesting / necessary to describe how you performed isopycnal mapping at the surface and at the bottom of a proïňĄle. – While e.g. Gouretski and Kolterman (2004) do an isobaric mapping of the top 100m layer and isopycnal mapping below, Schmidtko et al. (2013) do a separate mixed layer mapping and stitch that product to the interior. Since isopycnal mapping has its caveats at the top and bottom this part of the mapping should be described in detail! How do you deal with outcropping isopycnals at the top and the bottom of your mapped proïňĄle (data of any given isopycnal only to one side of your

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mapped location, due to e.g. seasonal variations in density, internal waves near steep topography etc.) ?

Response: In the upper 100-m layer we used isobaric gridding, as in Gouretski and Kolterman (2004). This is now stated in the text: "In the upper 100-m layer, water stability is generally low impeding robust computation of neutral density surfaces. In this layer the isopycnal gridding is substituted by isobaric gridding (Gouretski and Kolterman, 2004)." Seasonal variations are not taken into account for the basic gridding: the annual climatic means. For obtaining seasonal climatic means only profile within the selected month limits are used. The final results are obtained by z-interpolation of T-S characteristics from the isopycnic levels to z-levels in each gridding point. With isobaric mapping of the upper layer, isopycnal outcropping does not form a problem in the upper ocean. Over most of the study region the bottom is significantly below the lowest of the gridded z-levels. Choosing the densest G-surfaces to be always below the lowest gridded z-level (2300 m), we avoid problems with outcropping of isopycnals over most of the region. At steep topography the outcropping of lower isopycnal is resolved by extrapolating isopycnal characteristics under the topography during gridding procedure: the points under the bottom, but close to the bottom slope, have some data within the radius of influence (R = 70-200 km) and are set to some value during gridding. Those "under bottom" results are used for vertical interpolation to final z-level grid. Further on all data below the ocean bottom (ETOPO2 bathymetry is used) are blanked. Finally, there may be a situation when a nearly flat bottom lay below an isopycnal (for example in a shelf region). In this case the z-gridding is done down to the lowest isopycnal. Since the distance between isopycnals is not too different from the desired 25-m vertical resolution (10 to 40 m in the upper 300-m ocean and 50 to 100 m in the mid-ocean depths) appearance of blank areas over a bottom are generally within limits of the desired vertical resolution. Here we also note that the ocean bottom data are also not precise.

p1487-7 please show the full depth sections. -this is especially crucial, to see how

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your R transition near 2000m does work and how you compensate the signiïňĄcant decrease in data, due to missing ïňĆoat data below.

Response: The lower part of the profiles (2000-2500 m) is used exclusively to assure more robust gridding. Otherwise, as mentioned by referee, a drop of mapping quality in the places of outcropping of the lower isopycnals reduces the gridding accuracy. The final results cover the area down to 2300 m depth (this is now stated in the text). We agree that from the methodological point of view it is important to have a picture on the results of gridding below 2000 m. Now sections in Fig. 6 are extended to 2300 m depth (the highest level of the densest isopycnic surface used for gridding).

p1488-7ff for the comparison with WOA13 and MIMOC it would be helpful to know which season of the given climatologies were used? Did you compute the annual average? How large would you expect the seasonal cycle in 600/1200m to be. Is there a seasonality in eddies, that could bias the results?

Response: The following information is added to clarify the issue: “In this section, the temperature-salinity fields of MEDTRANS climatology, obtained in this paper, are compared to other existing annual climatologies: WOA09, WOA13 and MIMOC (Figs. 7-9).” Seasonal cycle is discussed in the next section 3.2: only minor differences are noted between the warm and the cold 6-months periods. There is no information on seasonality of mid-depth eddies in the area (even for the most studied eddies, as meddies). In the work by Siedler et al. (2005), authors describe the unique experiment of the 20-year long observation in the Kiel mooring (33oN, 22oW). They showed rather significant interannual variability of meddies passing through the mooring, but seasonal variability in a number of meddies was not pronounced.

p1488-10: ïňĄgures 7-9 should have identical regions for all panels or ordered differently. Further more a different color map for difference-maps would be helpful.

Response: We followed the logic to have new climatology in the first raw, an alternative climatology- in the next raw and the difference between the climatologies - in the last

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raw. We inter-changed plates g and d in figures 7-8, so that the figures are easier to read. We also limited colorscales in plates d, g and h.

p1489-17: these differences, are they due to better mapping or mapping bias? Since this is a crucial point in your manuscript, please show a vertical mapped proïňĄles of identical locations with WOA13, MIMOC and MEDTRANS and the complete unïňĄltered raw data within 30-50km as background (similar to your Fig. 9d with one panel each for each region and raw data included). Only such an approach can guarantee that your results are statistically signiïňĄcant and not biased due to applying a signiïňĄcant amount of ïňĄlters based on eddy occurrences and topography!

Response: We added the WOA13 and individual CTD profiles to Fig. 9 d. The following text is added to figure captions: "WOA13 profiles (black) are not seen since they are identical to MEDTRANS profiles for points 1-3 and to MIMOC profile for point 4. Dashed line over climatic profiles 3 are the observed CTD casts within 10-km distance from this point." Anomalously low N follows from anomalously low vertical variations of temperature and salinity in deep ocean (see Fig. 9d, point 3). This explanation is added to the text.

p1489-28 geostrophic currents are computed for what season? which month, annual average? same with the AVISO data set, is this the annual average, which years of data? Since the hydrographic data set is composed of data since 1950 or even older, the AVISO data set is using a different period.

Response: In this section we talk only about annual mean climatologies. This is now added to the title of section 3.1. AVISO data are averaged over the period 1992-2012. This is different form the period of CTD data used for construction of the climatology (1950-2011). Still AVISO currents represent the most reliable high-resolution data set for the ocean surface circulation and can be used as the reference. Comparing climatology results with AVISO we assume that the main current patterns in the region have not changed during the latest 50 years. This is certainly not a very good assumption,

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but there are no indications of the opposite in literature.

p1491-19: 30 or 50% how was this increase precisely determined.

Response: We changed the phrases to make the procedure more clear: “The mean window size containing at least 30 points increases by 30-50% (depending on the G-level and the season), as compared to that derived from the complete data-set (Fig. 3a-c). Therefore, to keep the distributions smooth enough, we increase the Barnes gridding radius by 50%: relative to the annual climatic mean (Section 3.1). Thus, for G less or equal to 27.96 kg m-3 (the upper 1900-m layer), R is taken 150 km near the Iberian coast and 300 km in the western side of the region. Its spatial variation is similar to the one presented in Fig. 4 (a).”

p1491-24: a comparison to WOA and even MIMOC in the seasonality would be of signiïňĄcant interest, since both climatologies do put signiïňĄcant emphasize on the seasonal variations and the computation of those. What does you make to choose May-October for summer? Why only the computation of 2 seasons and not 4 like WOA? or 12 months like MIMOC?

Response: We computed climatology for 2 seasons (warm, months 5-10) and clod (months 11-4) and not for 4 seasons, not to run into a problem of drastic decrease of the number of data. The latter would result in either the sensible decrease of the spatial resolution of the data-set (due to increase of R) or in less robustness of the resulting climatologies. Following the suggestion of the referee, we computed the seasonal variation of the temperature-salinity and geostrophic currents for WOA13 and MIMOC cold and warm season climatologies. The results of the comparison are now given in the text. In brief, in the upper ocean the correspondence is rather good with MIMOC climatology, but WOA13 climatology lucks those details in the seasonal variations. At mid-depths MIMOC presents seasonal patterns, but they are not as clear as in MEDTRANS seasonal climatology.

p1495-9-14, since you cover neither the polar nor equatorial regions, why is this nec-

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essary here?

p1495-15, Schmidtko et al do a zonal stretching only within 15âŮę latitude of the equator, thus not in the area discussed in this manuscript.

Response: We assume that the features of the gridding method can be applied for the World Ocean. So here we think it is appropriate to discuss some issues of extending the method over a larger region.

Table1: please extend the table to indicate further more proïňĄles with more than 75, 90, 99% of good data, to better detail the amount of data points that were removed by your data handling.

Response: Statistics in Table 1 was re-computed with 50% 75% and 90% of good data. We modified the computations by performing statistics not for the original profiles with variable resolution, but over the profiles binned to 25-m bins, since those are the latter data which enters into the interpolation procedure.

Figures: F1d -how comes the increase in data at 2500m, did you stop mapping at 2500m?

Response: With the discrimination between the data sets it is now clear that this feature results from OSD data increase (2500 m is a standard sampling level for Nansen bottle series). The data filtering is initially performed down to 2500 m depth, but the final mapping is done for the upper 2300 m, limited by the depth of the position of the lower isopycnal selected. Otherwise the mentioned by the referee drop of mapping quality in the places of outcropping of the lower isopycnals reduces the gridding accuracy at deeper levels. The final results cover the area down to 2300 m depth (this is now stated in the text).

F2 please use different color and possibly add contour lines to better see the differences. why is the colorbar extending to negative values and above 1? Please label axes.

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Response: Colour scale is changed from black&white to RGB. There are no weights below 0 and above 1. The colour scale is now set to the real limits of the weights; axis labels are added.

F3a does show the obvious locations of regular hydrographic ship based sections due to sudden decrease of window size. Please discuss the bias introduced to this in the text.

Response: We added the discussion to Metraial and Methods, the 3d paragraph: "Within the study region, there are over 54000 profiles (Table 1). The spatial distribution density of CTD (and to less extent of OSD) profiles show a tendency to decrease from the continental margin seawards. The PFL profiles cover the region rather uniformly, but mostly the areas away from the continent and with water depth exceeding 1000 m. A large amount of the OSD data is concentrated along standard sections, as WOCE A03 section (around 36ïĆř N), WOCE AR21/A16 sections (around 20ïĆř and 25ïĆřW), Portugal-Greenland sections, etc. OSD data mostly cover the years 1970-1995 (with maximum number of casts between 1985 and 1990), CTD data – 1985-2000 (with maximum number of casts between 1990 and 1995), while PFL – 2000-2011 (with maximum number of casts after 2005). Therefore, local biases of final climatology may immerge since different parts of the study region are predominantly covered with casts taken in a particular span of years. The climatic maps are constructed under a priory assumption of no strong interannual variations of water properties in the region since 1950. In limited parts of the region some seasonal biases are also possible, but they should not be significant, since the data are rather homogeneously distributed across the seasons in any part of the region."

F3c -please add uncertainty, like standard deviations to the lines. an additional panel showing window size over bathymetry depth would be interesting.

Response: Error of the mean window size for each meridional section, equal to +-1.96* std(window_size)/sqrt(N_data) is added to the panel 3c. Since the variations of

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the error in zonal direction are small, only mean value of the error is added.

Fig4 this decorrelation scale plot is for which data point, which depth, which location? This is, if I understand the text correctly, computed individually for each grid point. Is this one of the best, or why was this chosen? please give spread of correlation scales in ïňĄgure. -the bars of the histogram are not described in the ïňĄgure caption, is a 2nd y-axis necessary?

Response: Figure 4 (now A1) presents the mean histogram over all points and all depth levels. Bars present the normalised histogram of regional-mean eddy radii. This information is now added to figure caption. Practically no differences between the histograms are found for different pats of the region or different depth levels (this is stated in the text).

Fig5a&b: please use different color map and possibly add contour lines of 100, 200, 300 and 400 km.

Response: Colour map is changed. Isolines of R=100, 200, 300 and 400 km are added to the figure.

Fig6. why is the plot limited to 2000m, ïňĄg 1 indicates 2500m. Further more, please add a more detailed plot of the top 300m for both sections, since isopycnal mapping near the surface needs special handling. Why does the data pass through the topography?

Response: The lower part of the profiles (2000-2500 m) is used exclusively to assure more robust gridding. Otherwise the mentioned by the referee drop of mapping quality in the places of outcropping of the lower isopycnals reduces the gridding accuracy at deeper levels. The final results cover the area down to 2300 m depth (this is now stated in the text). Now sections in Fig. 6 are extended to 2300 m depth (the highest level of the densest isopycnal surface used for gridding). Data do not pass through topography. Some overlay is a result of use a plotting function that attributes a point of

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a certain size to each data point. The good thing about this plotting program is that it does not automatically interpolate the data - we see each point as it is obtained in the climatology data-set.

Fig7 please reorder the panels to make a visual comparison easier. F8, see F7 F9: see F7

Response: We followed the logic to put the new climatology at the top and difference between the climatologies at the bottom of the figure. We inter-changed plates g and d in figures 7-8 to put MEDTRANS climatology and its insert together. We also limited colour scales in plates d, g and h.

F9b, this panel is very interesting and deserves an additional ïňĄgure, with WOA proïňĄles and raw data added. Please see my comment above in the text.

Response: We added the WOA13 and individual CTD profiles to Fig. 9 d. The following text is added to figure captions: "WOA13 profiles (black) are not seen since they are identical to MEDTRANS profiles for points 1-3 and to MIMOC profile for point 4. Dashed line over climatic profiles 3 are the observed CTD casts within 10-km distance from this point."

F10 -why is WOA09 used here, while all other ïňĄgures use WOA13?

Response: Results for WOA09 are replaced with those for WOA13.

F11, 13 please plot only every 2nd, or 3rd arrow and increase arrow size for a better comparison. Further more please add reference arrows in all panels.

Response: The corresponding changes have been made.

Missing ïňĄgures: While the isobaric layer comparison of the climatologies was performed in detail, it would be necessary to compare at least one, if not 2 (meridional and zonal) sections and compute the differences. Further more a ïňĄgure dealing with the comparison in a theta-S diagram, showing the difference in isopycnal and isobaric

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mapping, in particular for the region of signiïňĄcant eddy activity.

Response: The figure is added: new Fig.6.

Please also note the supplement to this comment:
http://www.ocean-sci-discuss.net/11/C849/2014/osd-11-C849-2014-supplement.pdf

Interactive comment on Ocean Sci. Discuss., 11, 1473, 2014.

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