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Interactive comment on "Mean circulation and EKE distribution in the Labrador Sea Water level of the subpolar North Atlantic" *by* Jürgen Fischer et al.

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We first would like to thank the reviewer (2) for the detailed review and the very constructive remarks to this manuscript. We appreciate the recommendations in form and content, which we generally accepted – this will lead to a considerable improvement of the paper.

In addition to the textual remarks, the reviewer raised several substantial remarks, which we will thoroughly address.

RC2:The reviewer was surprised that the authors chose to merge the 1000 m and 1500

m float displacements (Line 143) without doing any thermal wind adjustment, and the concern would be a bias in both the mean and the eddy field if such an adjustment is not done.

AR2: Indeed we had a discussion about a thermal wind adjustment earlier in the writing phase of the manuscript, and we re-discussed this concern with respect to the remarks of reviewer 2. We came to the following conclusions: for various reasons, discussed below, we will not use any geostrophic adjustment, but we agree that more details are needed to convince the reader that joining the data from the two depth levels is appropriate. Thermal wind shear adjustment of individual data (displacement vectors) would need synoptic measurements of T/S profiles in the vicinity of the data point. Argo trajectory data of individual floats are synoptic, but would only support the cross – component, and with the concept of f/H following flow this would correspond to cross-bathymetric flow; near the topography this is generally small compared to boundary current speeds. In order to estimate the perpendicular velocity component one would need simultaneous T/S profiles from near-by floats. Within time scales of days to month this is rarely the case. The second possible version is to map the T/S field and calculate the mean shear on a similar grid. Based on the high resolution MIMOC climatology the effect is small in the western SPNA, and it is only the southeastern SPNA where the shear is stronger due to the presence of the more baroclinic NAC. Thermal wind adjustment based on this mean T/S-field will mix time and space variations in an arbitrary manner and can only be applied to the mean velocity field. In consequence it would not be possible anymore to separate the raw data inot u' and <U> components.

However, there is other information that may be used as justification for combining the data from both levels. First, we inspected current shears from moored records from both, the boundary current regime (Fischer et al., 2010, for mean velocity profiles in the DWBC in the Labrador Sea; Fischer et al., 2015 for the DWBC at various place around the SPNA) and interior flow (Figure 7 of this manuscript). From the Argo data we performed the gridding procedures for both depths independently, but with lower resolution and larger interpolation radii. The fields from the 1000m floats resemble that of the final product and there are no large scale biases (basin wide means). This is different for the 1500m floats where the data coverage is smaller and there are several large gaps. In general, the flow field along the topography will be different because the shallower level between 1000 and 1500m depth is only represented by the 1000m field. There were some areas in which the data density appeared large enough in both fields; e.g. southern Labrador Sea and Irminger Sea (see attached figure 1):

For the Irminger Sea, an area with boundary currents, interior advection, and sluggish circulation, we find rather similar circulation patterns in both levels. Beside the two levels we include the velocity difference of the 1000m level minus the 1500m velocity (right subplot of figure 1) with the largest differences in areas of low data density, e.g. along the DWBC off East-Greenland. We obtained the following statistical values (see table) with the individual means being much larger than the mean difference. The standard deviation of individual components are also larger than the standard deviations of the difference field. This supports the conclusion that a statistical significant difference is not detectable and that we may combine the two fields into a single layer without any adjustment.

Field Mean (cm/s) Standard Dev (cm/s)

U1000 -0.90 2.45

V1000 -0.55 3.17

U1500 -0.78 2.86

V1500 -0.64 3.24

U10-U15 -0.12 1.60

V10-V15 0.09 1.77

RC2: The reviewer has some concern about the interpretation of the Peclet number as

a quantitative measure of advection vs. diffusion.

AR2: We totally agree on this point and would also see the Peclet number as it is defined here in a rather simplistic way as a qualitative rather than a quantitative measure. Thus we interpret the regional differences in the Pe distribution as regional variations of the relative importance of advection and diffusion. This is now made clearer in the manuscript.

RC2: Finally, it is not clear which altimetry product was used. If it is the gridded product, it should be noted that it has much lower EKE than that produced from an along-track product (Zhang and Yan 2018). Thus, the comparison of the surface and deep EKE might have fewer regions with deep EKE > surface EKE. The overall interpretation of Figure 8 need not change, but I believe a note regarding what size eddies the altimeter product resolves is in order.

AC2: We used the gridded altimetric product which has a horizontal resolution of 0.25° – comparable to the resolution of the grid used herein. However, as the typical eddy size in the subpolar area is of similar size (10 to 50 km), the EKE estimates are certainly biased low compared to estimates of EKE from the along-track records. Thus, the ratio of the top to deep EKE should not be seen as an absolute measure, but for identifying regional differences between deep and surface EKE fields. This is now better explained in the text.

RC2: Line 194 - I don't see how the three float trajectories plotted on top of the mean flow field is an indication of how well the PV-constraint works

AC2: The trajectories show that individual floats stay for a long time (up to years) in a narrow bathymetric depth range which is indicated (figure 3a) through the colored topographic range. While the Coriolis force (f) does not change much along the trajectories this behavior is an indication that the floats indeed follow the bathymetry and therefore f/H as a measure of the large scale potential vorticity.

RC2: All data within a radius of 110 km and at locations with similar water depths – less than 1000m difference – were used in the OI." How was 1000 m chosen? Were there any sensitivity tests that informed this decision?

AC2: The rational behind the choice of the cut of radii was twofold: first, across the boundary currents the bathymetric slope is of the order of 2000m and we would not like to smear out the boundary currents by more than half of its width. This was similar to the parameters chosen for the Gaussian interpolation (GI) method. For the GI we performed some variational tests with the interpolation scales including the cut-off radii. The chosen parameters were a compromise between resolution and smoothness of the resulting fields.

RC2: Is Figure 3 made using the gridding procedure with a penalty in the cross-isobath direction? It would be helpful to point toward the exact subsection in the text where plotting procedure is described in the Figure caption. If this procedure is used, then it seems circular to argue that the coherence of the velocities along isobaths.

AC2: This is an important point – we used the cross isobaths penalty for generating Figure 3a, and based solely on this field it is in fact circular to conclude on along isobaths coherence. However, this coherence still exists when the cross isobaths penalty is removed; although the boundary currents are becoming wider through spreading towards the basin interior. This is now stated in the text.

RC2: Line 395ff - The manuscript goes through the exercise of comparing the two mapping methods (GI and OI). In this comparison, it comes to light that de-spiking to remove the top 1% of the largest velocities as part of the GI method, increases the bias relative to the OI method by 400%. This seems like a cautionary tale for users of each method, but the authors stop short of explaining how to avoid biases from anomalous velocities. It would be useful for the authors to give a more extended judgement on the promises and pitfalls of each method.

AC2: In fact any of the mapping procedures requires some despiking of the velocities

(especially the eddy component has some spikes close to the bathymetry, see Figure 3b). These are treated differently in the respective method. While the OI method uses least square techniques in which individual spikes have little influence, the GI method uses the data more directly, and for grid points close to the spike position these get a strong weight, such that the EKE is biased low. The magnitude of this bias depends on details of the editing method. When a simple sort and remove procedure is used then the bias increased from 1 cm2 s-2 to 4 cm2 s-2. Adding additional statistic constraints (e.g. removal only values exceeding 2-standard deviations within the area used for the interpolation — order 100km range) would reduce the bias to the range of 1-2 cm2 s-2. However, we agree that a quantitative interpretation of the EKE and local variations of it should be made cautionary. This extended description is now incorporated in the text.

RC2: - This study does not look at flow at the Grand Banks, and southward flow around Flemish Cap appears strong in Figure 3 and 5a, with relatively high Pe in Figure 5c. Therefore, I do not understand the evidence provided in support of this conclusion: "While the Irminger Sea route appears strong and robust, the flow along the topography (Flemish Cap and Grand Banks) is relatively weak." I suggest removing this sentence or including some references to support it.

AC2: This has been rewritten with a not so rigid statement regarding the Flemish Cap pathway, and we added the reference (Schott et al., 2004) that has a focus on the circulation along the Grand Banks.

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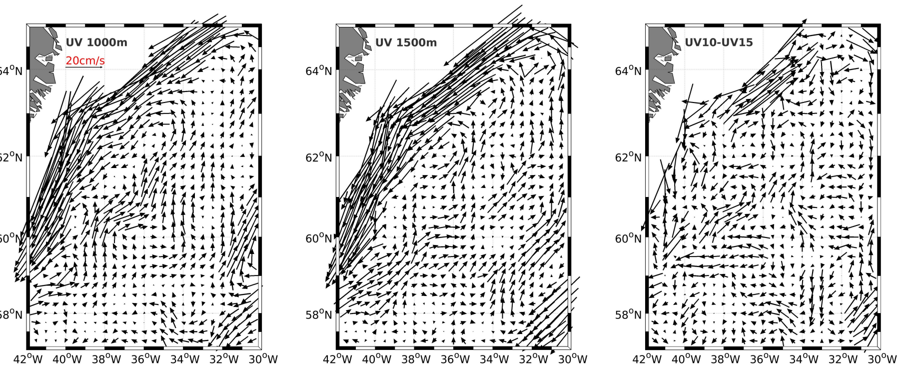

Fig. 1. Irminger Sea circulation in depth layers