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Ocean Science
Discussions

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

Anonymous Referee #2

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A long term mean flow field for the subpolar North Atlantic region with a horizontal resolution of approximately 25 km is created by gridding Argo-derived velocity vectors using two different topography-following interpolation schemes. The deviation from the topography-following component is interpreted as the eddy contribution. The results from these procedures compares favorably against EKE calculated from a number of long-term moorings in the region. With the circulation field thus verified, the authors then interpret different dynamical regions by comparing the mean advection against the eddying contribution, via computation of a Peclet number. The result of this comparison is reasonable: advection is more dominant in boundary currents, eddies relatively more dominant in the sluggish interior of the basins. Finally, the manuscript compares the

deep (nominally 1000 m EKE) against surface EKE from satellite altimetry, and show that there are several regions where deep eddies appear to be more energetic than surface eddies.

The manuscript is well written, and many of the figures are beautiful. There is broad interest in the circulation of this region, and the statistics of the deep EKE advances the field. The interpretation of the key results is well supported, except in a couple of places of concern that I outline below. I believe the paper is worthy of publication following attention to a small number of major points, and some additional minor details.

Major:

I was suprised that the authors chose to merge the 1000 m and 1500 m float displacements (Line 143) without doing any thermal wind adjustment. A back-of-the-envelope calculation suggests that the assumption that flow is barotropic enough to justify the lack of attention to this step is questionable. For instance, the manuscript states that the density varies by 0.2 kg m-3 at 1500 m. If this density change occurs over a distance of 100 km or less (not unreasonable according to Figure 5a), the velocity will change by more than 10 cm s-1 over the 500 m separating the 1000 m and 1500 m displacements. So, by averaging without accounting for this difference, you might bias your estimate of the mean flow and the eddies.

Second, I am a little bit hesitant about the interpretation of the Peclet number. Specifically, it is not clear how Ld chosen. This choice will strongly effect the result. If a spatially-varying Ld is chosen (as might be appropriate, given varying latitude, stratification, and ocean depth), this should be described. If a constant is used, this should be clear and also justified. Related to this point, I do not see clear evidence that eddies must dominate below Pe < 0.2. The stated justification for alpha = 0.25 is not physical, and there is uncertainty in Ld, so it seems that this Pe has quite a bit of uncertainty. Overall, I think the calculations should be better explained. I also think that this discussion should be more about relative strength of eddies versus advection, rather than

which dominates in an absolute sense. The low Pe regions are certainly locations of weak mean flow, where the eddying is a more important part of the momentum budget than the high Pe regions.

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.

Minor

Line 45 - Not sure what is meant by "hindsight"

Line 77 - "proof" -> "prove"

Line 190 - I don't understand this sentence, which seems critical to the method. Some equations would help.

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

Line 215 "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?

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 is evidence that this procedure is appropriate (e.g. Line 249).

Line 290 - It is not clear what is "not shown."

Line 314ff - Make a reference to Figure 5a to guide the reader to the appropriate figure.

Line 356 - A priory -> a priori

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.

Line 410-412 - quotes are unnecessary around 'convection' and 'Bravo'

Line 438 - no verb

Line 540 - "thus there are areas with and with larger EKE at mid depth." Seems to be a word missing.

Line 555 - 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.

Figure 5 - I assumed the color of the boxes used the same colorscale as the background contour map, but this should be spelled out in the caption. Colorbar and axis labels all too small

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