

Interactive comment on “Occurrence and characteristics of mesoscale eddies in the tropical northeast Atlantic Ocean” by F. Schütte et al.

Anonymous Referee #1

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The paper presents an appropriate overview of eddy properties off the west African upwelling (TANWA region) and of their contribution to transports. It uses most of the data of the upper ocean available, either in situ or satellite-based, and nicely synthesizes the results. The methodology both for identifying eddies, tracking them or establishing their properties is appropriate. The introduction gives a fine state-of-the-art review, and the discussion/ conclusion sections presents a fine analysis of the relevance of the results and of possible limitations of the approach.

What follows are some minor comments/queries.

Data and methods: Right choice of satellite data products. Among the data, glider data collected in this region have not been used. Is it because of insufficient vertical reach of the glider CTD data? (maybe not before end 2013?). Among the Argo data, 40%
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rejection with the three criteria chosen seemed particularly high: which of the criteria used explains that in this region? In CVOO mooring profiles, the reference profile is chosen before the eddy passage. Any reason for not also taking into account profiles collected afterwards? Bottom page 8, line 30-33: not completely clear. Is it that for each profile inside an eddy, one checks whether there are reference profiles outside of the eddy filling the criteria and then estimate (or not) an anomaly. . .

Results; The average eddy-radius is 56 km, so it is rather close to the resolution in the AVISO products that are used to estimate the eddy statistics (size, velocity). What is the implication on these statistics?. I understand that the uncertainties in tracking due to errors in mappings (insufficient altimetric coverage) requires to check whether same eddy reemerges a while after. I did not fully understand what is the criterion used to identify a same structure after a tracking interruption.

3.2 formation and propagation (pages 12-13). Very interesting and informative sections. On the other hand, the arrows and eddy corridors delineated on figures 8 (right panels) are rather schematic. Is there a way to be more quantitative there. At least, it should be possible to add average speed (both zonal and meridional) as well as its rms for these different 'average' vectors (and each eddy type). For ACME, however, statistics might not be that relevant. Seems that they are mostly in the north?

p. 14 interesting strong seasonal dependency on cyclone formation that is correctly identified and analyzed. I see it much less with anticyclones (except after removing ACME), and not so sure that the July isolated peak is so relevant (or at least, this should be further argued; as one could as well state the maximum at the end of the year...). Obviously, ACME have a formation peak in spring, during the core upwelling season. Is it the influence of the undercurrent water, which would also explain why they have such a strong SACW signature? (otherwise, could a cap be formed in spring time over a structure that would be cold and fresh at subsurface, with the formation related to subsurface eddying).

3.4: eddy structure. Very nice summary statistics. However, on table 3, I am not sure of the consistency of the 5 comparisons. One difference; for 2 to 5; diff of T-anomalies in anticyclone-cyclone=1.2°C, whereas for 1 it is 2.1°C? (also larger diff for salinity in 1: 0.29, compared to 0.15 for 2 to 4). Why is there such a large difference, which seems to have mostly originated from cyclones? Could it be that at the chosen distance from cyclone core, one tends to be into an anticyclonic structure: this would be somewhat surprising, but...?

3.5: when estimating, volumen (mass), assumption of coherency (close streamlines) to 350m. Why this choice? (as one goes down, geostrophic velocity would diminish, thus water would be less trapped). I am slightly flustered by the estimates of transport and relase based on ASA and AHA, as they assume implicitly that there is horizontal compensation of mass, thus heat/salt by reference water (thus rather different when one uses reference profile or the climatologies 2 to 5). Also, clearly, one expects partial compensation between the cyclones and anticyclones, and thus the net estimate will be very dependent to how structure are identified, how the statistics are established and how they are tracked. I am not convinced that these computations bring any relevant estimate (at least order of magnitude).

After, bottom page 19 and 20, estimate of transport of SACW yields more robust results with strong differences between structures that are carefully analyzed (better transport by ACME and then cyclones. . .; but for anticyclones, is it compatible with earlier statement on U/C on good water trapping in these structures). An important role of eddies is identified to transport SACW from the coast to the west (and all the way to Cap Verde front?)

Figures 2 and 3: the tracks on fig.2b of cruises do not always cover the red dot of cruise CTDs in Fig. 3 (for example near 15°N or 13°N)

Figure 9, I am wondering how one can separate ACME from other anticyclones for their generation in the source region? (to be more specific: at what point in eddy life is SST

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used to characterize whether anticyclone is an ACME or not)

Figure 12: for ACME, left sections and right average profiles are compatible for ACME, but show same anomaly sign for T and S through the vertical profile. It is worth mentioning that there S dominates over T for horizontal density gradients below the eddy core (somehow, I did not see that mentioned in the text; lines 12-13 of 3.4.2 state the opposite, but probably only refer to cyclones and anicyclones). (fully compatible with figure 15)

Interactive comment on Ocean Sci. Discuss., 12, 3043, 2015.

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