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

Anonymous Referee #2

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This study provides a thorough analysis of the eddy characteristics in the Tropical North Atlantic Ocean, merging various satellite and in-situ datasets. The paper provides a very interesting description of the eddy properties, with a special focus on their vertical structures and associated cross-shore transports from the near-coastal upwelling region to the offshore ocean. I really appreciated i) the proposed discrimination between "regular" anticyclones and anticyclonic mode water eddies, ii) the use of satellite sea-surface salinity data which are barely used in studies dealing with mesoscale activity. I really liked reading this paper which is well-written and conveniently organized. The conclusions are supported by the use of appropriate methods and data. I have only some minor comments/suggestions that could probably help to improve the quality and clarity of the paper.

Abstract. As the number of eddies per year is highly dependant of the minimum lifetime used in the tracking algorithm, I strongly recommend to mention this duration.

Section 1. The introduction is concise and well written. p.3045, L18-21. It is mentionned that previous studies found a low eddy activity in the TANWA region (p.3045, L18-21). However, among the 4 major upwelling system, the TANWA region has been shown to be one of the most active in terms of eddy generation, both at the coast and around the Cap-Vert islands [see for instance Figure 1 in Chaigneau et al., 2009]. p. 3047, L11. Please, mention the latitude of Cap Blanc. p.3047, L19-20 : It is mentionned that one of the topics investigated in the manuscript is the efficiency of mesoscale eddies in dissolving existing gradients. I found this sentence rather unclear and I don't think the authors explicitely study the role of mesoscale eddies on the gradients' ditribution. Please explain, rephrase or remove. Figure 1. I recommend including in the Figure Caption, the nomenclature of the depicted currents.

Section 2. Section 2.1.1. Please, mention which SLA product was used ("two-sat-merged"or "all-sat-merged") Section 2.1.2. The authors used the geometrical approach (GEO) developped by Nencioli et al. (2010). However in this method, the eddy edge is not ientified by the longest closed streamline around the eddy center (such as in Chaigneau et al., 2009) but by the closed streamline associated with the strongest swirl velocity. This difference between the longest closed streamline and the Nencioli's criterion can induce strong differences in the eddy radius distribution (e.g. see Figure 4). Furthermore the Nencioli's method needs to specify 4 constraints for the identification of eddy centers and edges. Thus, I would recommend the authors describe in details this eddy identification method and how they adapt the constraints for the TANWA region. p. 3050, L.4-11. The eddy tracking algorithm is also unclear. Please rephrase. In particular it is unclear if the authors used a threhold of 7 days (e.g. L.5) or 14 day (L.9) to identify an eddy. How the authors dicreminate between distinct eddies that could have the same polarity in the search radius ? The exact definition of the search radius (10 or 60 km ?) is also unclear. If eddy centers are defined by local extrema in SLA (p. 3050, L-1) how can the authors find another eddy center within 10 km with a grid resolution of 25 km ? Did the authors used eddy centroids intead of local extrema in SLA ? Finally I did not understand when the OW or GEO method is preferred in the results Section. . . please, clarify.

Section 2.1.3. 20% of the eddies were classified as ACMEs. How the SST and SSS anomalies were computed ? An average within their cores or the value at the eddy center ? An average along their trajectory ? Please, explain. The authors decided to use cartesian coordinates to depict the composite maps of eddy properties. However they mix eddies having a wide range of eddy radii. It would probably have been more appropriate to use would a normalized distance. . . A criterion on the eddy amplitude (2 cm) is also applied to construct these maps. How the composite analysis presented in Section 3 varies with the eddy amplitude ?

Section 2.2. Why the authors decided to retain only Argo float profiles having data down to 1000 m if they only study the upper part (0-350m). I guess considering profiles having data only in the firsts 500 m would have considerably increased the number of available data. A Table indicating, for each dataset (Argo, mooring, ship data, etc.), the number of profiles within Ces, Aes, ACMEs or outside eddies would have been appreciated. Do Argo floats in the TANWA have a preference to be trapped within a particular eddy-type or within larger and long-lived eddies as mentionned by Pegliasco et al. [2015] ? Figure 3. The mooring location is unclear.

Section 2.3. p. 3053, L.10-15. Do Pegliasco et al. [2015], who also used Argo floats near the TANWA region but a distinct eddy detection algorithm, also found a similar proportion of floats within eddies ? p. 3053, L.15-20. Several authors (Castelao, 2014 ; Pegliasco et al., 2015) have constructed their anomalies using profiles outside eddies, within +/- 30 days independently of the considered year. It is unclear if in this manuscript the authors used a similar approach. . . if not, using such an approach would probably strongly increased the number of available anomaly profiles. Furthermore the comparison with other climatologies (CARs, WOA, Levitus) presented in the Results

Section would be probably more robust. Also, is there any justification to choose criteria of 120 km (why not 150 or 200 km ?) and +/- 25 days (why not 30 days ?) for the reference profile. p. 3054, L. 3-4. The authors mention that 95 profiles are within ACMES. However from Fig 5, we only see $\sim$20 profiles within ACMEs. Please, explain such a difference.

Section 3. Section 3.1. Is there any physical reason why the standard deviation on the eddy radius is much higher when the GEO-method is used ? p.3057, L.10 : Is the maximum lifetime constrained by the longitudinal extent of the study area ? (e.g. Do anticyclones disappear/dissipate after 280 days or they are no more detected due to the presence of the western boundary at 28W ?) p.3057, L.19-20. The dominance of anticyclones is interesting and was also observed in the polarity map of Chaigneau et al. [2009]. Is there any explanation for such a dominance of long-lived anticyclones ? Section 3.2. A visualization of the eddy trajectories in Figure 8 would be better than a rather simple schematics of the eddy propagation patterns. Or both of them (schematics and "true" trajectories) should be presented. Section 3.4.2. It is unclear why a deepening of the isopycnal below $\sim$120-150m in the ACMEs does not produce a positive temperature anomaly since it should inject warmer water in deeper levels. Please, explain. Also, the mean distribution of isotherms and iso-haline levels in Figure 12 a-b would have been probably more relevant than isopycnals. A description and quantification of the vertical displacement of the isopycnal layers with depth would have been appreciated.

Section 5. Section 3.5.1. Figure 14b. It is unclear how ESHF were computed. Please, provide more details since I could not reproduce the obtained HF values. Section 3.5.2. It is considered in this Section there are only 2 Water Masses between the surface and 350m depth (SACW and NACW). However this region can also be influenced by tropical surface water (at least in the southern part of the TANWA) in the surface layer (0-50m) [e.g. Stramma et al., 2005]. Should the TSW be considered in the WM analyis ? It is mentionned (p.3067, L.29) that anticyclones have the same SACW signature as the background. In this case, which Watter Mass anticyclones transport and which water mass explain the strong positive T/S anomalies inside their cores ?

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