

Dear Reviewer,

First of all we would like to thank you for the time and effort you spent on reviewing our manuscript. We very much appreciate your comments that clearly have identified parts of the paper that needed more attention. We tried to address all the questions and comments you raised and are convinced that the manuscript will be improved significantly.

In the following we sorted all comments/questions (**RC:**) by numbering these (according to your numbering) and providing for each an answer (**AC:**).

Best regards,
B. Fiedler & Coauthors

1. Reviewer #1:

Review of Fiedler et al., Oxygen Utilization and Downward Carbon Flux in an Oxygen-Depleted Eddy in the Eastern Tropical North Atlantic.

bg-2016-23

General comments:

Reviewer Comment (RC)1_: *Boundary definition: The authors are rather vague about the definition of eddy boundaries (the lateral boundaries, but in particular the lower boundary of the eddy core), as well as the processes leading to exchanges, or lack thereof, across those boundaries. I would suggest the authors provide more details on eddy boundaries, the depth of the mixed layer, and the depth of euphotic zone, as well as provide a stronger case for rationalizing why eddy waters don't mix with the surrounding ocean waters. For example, is the depth of the euphotic zone (Dez) and mixed layer depth in the eddy core equal to the outside water? Or is Dez different due to higher light attenuation by particles?*

Author Comment (AC)1_: We agree that details about the physical boundaries of this eddy are not well described in this manuscript. However, we left this out on purpose as Karstensen et al. (2016, this special issue) are presenting an elaborate physical analysis of boundaries for this particular eddy. But we have to admit that this link was not made clear enough. Therefore we will add a sentence to section 3.1:

“Isolation of this eddy was found to be caused by high eddy rotation speed and stratification and their joint impact on the propagation of internal waves (Karstensen et al., 2016, this special issue).”

We also edited two sentences in section 3.1 in order to explicitly mention the mixed layer depth in and outside the eddy.

“The upper bound of the eddy core is the mixed layer base at a depth of 70 m which has the same magnitude as the mixed layer outside the eddy (Karstensen et al., 2016, this special issue). A very sharp gradient exists between 70 – 77 m depth which amounts to 0.73 in salinity, 3.98°C in temperature and 165.8 $\mu\text{mol kg}^{-1}$ in dissolved oxygen.”

Unfortunately, light/PAR measurements failed during the surveys due to sensor problems. Thus, we can't give reliable information about the euphotic zone for this particular eddy. We removed speculative connections between the eddy core and the euphotic zone from the abstract and the conclusions.

RC2_: *Episodic events: Throughout the paper a steady state biogeochemical system is implied (or at least a slowly evolving biogeochemical state). Yet the physical processes that allow for these balances are episodic and submesoscale (e.g. evidence for the re-supply of nutrients into the upper layer is lacking, and yet required for the equilibrium biogeochemical state in the mixed layer). Is this vertical nutrient flux driven by interaction of eddies with the overlying wind field, Ekman pumping, or internal waves displacing isopycnals and mixing? How frequent are these episodic events?*

AC2_: This is indeed an important point. Unfortunately, these processes are extremely difficult to observe, even with the tools we applied during this study (autonomous glider, see Karstensen et al., 2016, this special issue). We disagree that evidence for re-supply of nutrients into the upper layer is lacking as we clearly found elevated nutrient concentrations in the surface during one of the two ship surveys (section 3.2, 2nd paragraph). Since methodological biases of these samples can be ruled out, this observation can only be explained by an upward vertical flux. Observations for chl-a also indicate elevated levels of phytoplankton towards the upper part of the mixed layer, likely being facilitated by upwelling of subsurface waters. Finally, Karstensen et al. (2016, this special issue) derived a physical concept which provides a mechanism for upwelling occurring at the rim of the eddy, followed by horizontal distribution. The process is mainly driven by

downward propagation of internal near inertial waves. Since details about this mechanism are described in Karstensen et al. (2016), we added a sentence in section 3.2 (2nd paragraph) in order to provide a link to that paper:

“As such, this finding is interpreted as being a signature of a vertical flux event. The physical mechanism is described in more detail in Karstensen et al. (2016, this special issue).”

Further, we cannot derive information about how intermittent/sporadic the upwelling is and we will rephrase that to a more general statement on “upwelling processes”. The analysis of an oxygen float (Karstensen et al., 2015) showed that the respiration derived from 5 day oxygen profiles over a period of several month showed a surprisingly constant decrease which in turn suggests that particle sinking is also constant and probably also the upwelling to ensure a constant bloom. However, the finite duration of a bloom also applies a “running mean” to any intermittence of the upwelling. This is definitely a process that requires further attention.

RC3_: *Downward carbon flux - POC export: This seem to be the weakest part of the paper. The authors need to provide more details on physical and biological assumptions in the simple downward flux model, and whether its assumptions are valid in the eddy core. Is a steady state balance implied for model? A constant diffusivity? What about small non sinking POC export by eddy flow field subduction of surface waters with high POC concentrations? (See Mahadevan et al., 2015, Science). Consequently, the carbon flux model and Figure 8 may not make a meaningful contribution to the study.*

AC3_: The reviewer addresses the right issues if the applied model would have the intention to fully explain the total vertical carbon fluxes. We have to admit that the way we introduced the downward POC flux calculations might be a bit misleading and makes the reader to expect a more comprehensive model that also accounts for physical transportation processes such as diffusion or subduction. Our main intention, however, was to look whether observed carbon remineralization of sinking particles inside the core follows a classical Martin curve and how this amounts to an export of POC out of the euphotic zone over the lifetime of this eddy. In order to assess the magnitude of observed carbon remineralization compared to other ocean environments (e.g., open ocean ETNA, coastal upwelling, North Atlantic, eddies) we decided to translate our results to a mean daily POC flux. Our results suggest that the Martin curve fits well the carbon remineralization rates and thus sinking particles are likely to be the major driver for our observations. The 3-fold enhanced carbon export also matches very well with the independently determined production rates which were also enhanced by a factor of 3 (Löscher et al., 2015).

Regarding the subduction of high POC waters as described in Omand et al. (2015) we think that this mechanism would not affect our POC calculations for two reasons: 1) Described subduction in Omand et al. occurs rather at the perimeter of the eddy and 2) the special physical conditions that characterizes an ACME would not allow for an intrusion of subducted (high POC) waters into the ACME core.

Finally, given that the core of the eddy is a transient system (oxygen consumption without reventilation) and only the limited number of observations in space and time we won't be able to come up with a more detailed approach.

Thus, we use a simplistic approach to observed change in biogeochemical water properties in order to give quantitative estimates of the POC flux and carbon export. For more sophisticated export flux models we lack information but this does not mean that carbon export can be fully explained by a Martin-type function. In general, however, the models fit our data well and is thus only used to extract some quantitative information for further comparison. We therefore think that this section still make a meaningful contribution to the manuscript. In order to clarify this topic we edited the beginning of section 2.5 as follows: *“In order to estimate the amount of carbon exported from the euphotic zone as sinking POM we used CRRs to derive the shape of the vertical export flux curve for particulate organic carbon (POC). This approach assumes the*

absence of major physical transport processes between the mixed layer and the ACME core beneath except for sinking particles of POM which is generally being described by the established Martin Curve (Martin et al., 1987a):”

RC4_: *Significance (and a Budget): What is significance of 1 to 2 ACMEs generated every year that propagate into the open ETNA waters for biogeochemistry and ocean acidification of the region, or even the ocean basin? Is this a phenomenon that might be expect to occur elsewhere (does the study have much broader implications? To help address this it might be worth carrying out at a budget/balance exercise to quantify, for example, whether the supply of nutrients exceeds export of nutrients, which leads to than increased productivity.*

AC4_: To estimate the significance of these eddies was also one of our main objectives of the “Eddy Hunt” project the special issue is concerned with. Besides a local impact, that is important for a process understanding, the larger scale impact, at least for the eastern tropical Atlantic is of interest. The occurrence of the dead-zone eddies was analyzed in a study submitted in parallel to ours (Schütte et al., 2016, this special issue). The authors analyzed satellite (SLA, SST) and in-situ oxygen and T/S profile data for the eastern tropical North Atlantic. They estimated that about 1 to 2 low oxygen eddies disperse in the region every year but because of the anomalous low oxygen they found an at least 6% contribution of these low-O₂ ACMEs in the maintenance of the shallow oxygen minimum zone (centered at about 70m depth and about 250m above the core OMZ). The conclude that their estimate is a conservative one since the detection of ACMEs from satellite data is challenging (because of a weak SLA signature of ACMEs) and the actual number of ACMEs is likely to be higher. We will add a sentence at the end of the 2nd paragraph of the conclusion as follows:

“As revealed by Schütte et al. (2016) these ACMEs appear to play a small but significant role in maintaining the shallow OMZ in the ETNA.”

Probably related to the improvement of multidisciplinary autonomous and high resolution satellite-borne observing techniques, much attention has been devoted to investigating local processes in and large scale impact of ocean mesoscale eddies. Many studies have recently been published. What is specific for the ETNA region is the cold/fresh core of the eddies, an imprint from the coastal current and definitely different from the Pacific, where the coastal current carries warm/saline water. This could matter for the temporal evolution of the stratification and as such for the isolation (see Karstensen et al. 2016). For the purpose of this paper we limited our regional examples of other studies to the North Atlantic (McGillicuddy et al., 2007) and the South Pacific (Altabet et al., 2012).

Specific comments:

Abstract:

RC1: Page 1, Line 13: Define the extreme low oxygen environment.

AC1: Sentence edited: *“The occurrence of mesoscale eddies that develop suboxic environments at shallow depth (about 40 to 100 m) has recently been reported for the eastern tropical North Atlantic (ETNA).”*

RC2: Page 1, Line 22: Define the lower boundary of the euphotic zone.

AC2: Changes mentioned under AC1_, see above.

RC3: Page 1, Line 27: Define the lower boundary of the surface mixed layer. Is this shallower than the euphotic zone?

AC3: Information will be added as: “Vertical distributions of particulate and dissolved organic matter (POM, DOM) generally show elevated concentrations in the surface mixed layer (0 – 70 m), but particularly DOM also accumulates beneath the oxygen minimum.”

Direct observations of light attenuation are missing, see also AC1_.

RC4: Page 2, Line 1: an enhancement of apparent oxygen utilization rates: : at what depth?

AC4: We will modify two sentences of the abstract to define the depth of the eddy core as follows:

“At the time of the survey the eddy core showed lowest oxygen concentrations of less than 5 $\mu\text{mol kg}^{-1}$ and a pH of approx. 7.6 at a depth of approx. 100 m.”

“Considering reference data from the upwelling region where these eddies are formed, we determined the oxygen consumption through remineralization of organic matter and found an enhancement of apparent oxygen utilization rates (aOUR, 0.26 $\mu\text{mol kg}^{-1} \text{d}^{-1}$) inside the core by almost one order of magnitude when compared with typical values for the open North Atlantic.”

RC5: What is the significance of your findings for the biogeochemistry of ETNA?

AC5: As this study does not directly determine significance in a quantitative way we cannot define this in the abstract. However, we will edit the last sentence of the abstract as follows: “The observations support the view that the oxygen depleted eddies can be viewed as isolated, westwards propagating upwelling systems of their own and thereby represent re-occurring alien biogeochemical environments in the ETNA.”

Introduction:

RC6: Page 2, Paragraph 2: A figure showing ETNA, OMZ, EBUS, CVFZ would be helpful. Maybe add on to Figure 1.

AC6: We appreciate this constructive comment from the reviewer and will add some of the acronyms to Figure 1 (both main and inlet panels).

RC7: Page 3, Line 9: Define anticyclone mode-water eddies.

AC7: We will follow this suggestion and add one sentence after the introduction of ACMEs: “The latter ones are characterized by a water lens of mode which is being formed by up- and downward-bent isopycnals towards the eddy center.”

RC8: What is the main objective of this paper?

AC8: We will add one sentence to the last paragraph of the introduction: “Here, we present the first biogeochemical insights into low-oxygen ACMEs in the ETNA based on direct in situ sampling during two coordinated ship-based surveys. The main objective of this study is to reveal and quantify biogeochemical processes occurring inside a low-oxygen ACME in the ETNA.”

Methods:

RC9: Page 5, Line 8: At what depth were water samples collected?

AC9: Information will be added: “Water samples in the upper 500 m were collected with a rosette water sampling system...”

RC10: Page 6, Line 26: At what depth were the water samples for DO collected?

AC10: Niskin bottles during both cruises were closed following a certain depth grid. Only depths of Niskin bottles close to the eddy core/oxygen minimum were slightly adjusted in order to adequately resolve this part of the water column. All samples (for various parameters) were collected for each depth. Since sampling depths for each parameter can be also deduced from figures 4 – 6 we decided to not explicitly mention this information in the text.

RC11: Page 7, Line 10: same as above, define depth of DIC and TA samples.

AC11: see above (AC10).

RC12: Page 7, Lines 21-24, using which software?

AC12: We will add this information to the text (and add the respective reference) as follows:
“Results from DIC and TA analysis were used to compute the remaining parameters of the marine carbonate system (pH, pCO₂ and Ω_{Ar}) using a MATLAB version of the CO₂SYS software (Van Heuven et al., 2011). Calculations were based on carbonic acid dissociation constants after Mehrbach et al. (1973) as refitted by Dickson and Millero (1987).”

RC13: Page 9, Line 4: Define apparent oxygen utilization rate.

AC13: We will modify the respective section as follows: “Changes of oxygen and carbon due to remineralization of organic matter are being expressed as the apparent oxygen utilization rate (aOUR) and the carbon mineralization rate (CRR). In order to determine these rates not only the anomaly but also the age of the eddy, the time between formation on the shelf and the time the eddy surveys took place, needs to be known.”

Results and Discussion:

RC14: Page 10, Lines 15-20: This text is repetitious, and can be omitted.

AC14: We agree and will remove this part from the manuscript.

RC15: Page 10, Lines 22-24: Specify depth.

AC15: Since the sentence describes the characteristics of ACMEs in the ETNA in general we can't specify a certain depth. However, we will add the word “subsurface” to this sentence: “The Temperature-salinity (TS) characteristics of the subsurface core of ACMEs in the open ETNA (Schütte et al., in prep. for this issue; Karstensen et al., 2015) were found to be nearly unchanged, compared to coastal regions.”

RC16: Page 11, Line 20: (100m)- not clear if is this at 100m, or from 0 to100m?

AC16: We will slightly rephrase this sentence to avoid potential confusion: “In comparison to the reference profile from the Mauritanian Shelf we find a maximum oxygen decrease in the eddy core at a depth of 100 m of about 57.0 μmol kg⁻¹ to suboxic levels (<5 μmol kg⁻¹; Figure 3).”

RC17: Page 11, Lines 16-18 (sentence 2): move to after describing the results.

AC17: Thank you for this remark. We decided to fully delete this sentence as it is redundant with the second last sentence of this paragraph.

RC18: Page 11, Line 27: Why elevated nitrate, nitrite and phosphate but not silicate? Also, did you look into nitrate:phosphate ratio as evidence of denitrifying bacteria?

AC18: We interpret the depletion of silicate as a consequence of high abundance of diatoms in the surface mixed layer. From sediment trap data at CVOO (Fischer et al., 2015, this special issue) we know that diatoms were the dominant species during the passage of an ACME in 2010 at CVOO. Koeve (2004) also reported on high nitrate:silicate ratios in the North Atlantic and explains this with a different uptake ratios than the nitrate:silicate ratio of upwelled waters. We extended the discussion of surface nutrients as follows: “In contrast, silicate concentration remained low which could be explained by an enhanced abundance of diatoms in the mixed layer. Further, Fischer et al. (2016) reported on high opal concentrations in sediment traps at CVOO which were associated with the passage of a former ACME passing the observatory. High N:Si uptake ratios, also reported for the North Atlantic (Koeve, 2004), could explain observed nutrient concentrations.” We also looked into nitrate:phosphate ratios but couldn't find deviations pointing towards denitrification. However, genetic analysis of the microbial community clearly revealed active

denitrification in the core of the eddy. N:P ratios as well as the discussion on denitrification of this particular eddy are already published (Löscher et al., 2015).

RC19: Page 12, Lines 1-5: How often do these sporadic events occur? Are they wind induced, or due to passage of internal waves?

AC19: Please see above (AC2_).

RC20: Page 12, Lines 7-9: This sentence is a repetition from Methods and can be omitted.

AC20: We fully agree and will omit this sentence.

RC21: Page 12, Line 26: “phytoplankton communities are exposed to these acidified conditions”. How often? (the pH minimum is located just below the euphotic zone).

AC21: The reviewer is right in asking about abundance of phytoplankton in the core at this depth (beneath or close to the euphotic depth). However, here we wanted to give a general statement about acidified cores of ACMEs. Indeed, the particular ACME surveyed during this study has its core relatively deep. Other ACMEs may have more shallow cores which are located within the euphotic zone. Karstensen et al. (2015), for instance, observed an ACME in the same region in which the core reached up to even 40 m water depth which is very likely to be within the euphotic zone. In order to keep the respective sentence more general we will slightly edit it as follows: “Absolute values of pH inside the eddy exceed these predictions and plankton communities inside shallow low-oxygen cores of ACMEs may get exposed to these acidified conditions.”

RC22: Page 13, Lines 25-29: is this consistent with your Chl-a observations?

AC22: This finding is consistent with discrete samples for chl-a which are presented in Löscher et al. (2015). Chl-a concentrations as illustrated in Figure 3 (most right panel) were derived from different fluorescence sensors mounted at the rosette water samplers during different cruises. Computation of chl-a concentrations were based on factory calibrations for each individual sensor. The fact that calibration of fluorescence data for the determination of chl-a concentration is not well developed and daylight quenching of fluorescence at the surface (Xing et al., 2012) biases this data as well, we doubt that absolute concentrations derived from fluorescence sensors are robust for a quantitative interpretation. We rather see these sensor measurements as a qualitative proxy that describes rather the vertical distribution of phytoplankton in the water column.

We will add two sentences to the methods section as follows: “Additional sensors such as an oxygen sensor (SBE43, Seabird Electronics) and a two channel fluorometer (FLNTURT, WETLabs) were attached to the CTD. Note that factory-calibrated fluorometer data in this study can be only used as a qualitative proxy for phytoplankton distribution in the water column due to a lack of elaborate sensor calibrations.”

Conclusions:

RC23: Page 17, Line 30: “intense increase”: : specify where.

AC23: We will add this information to the sentence as suggested: “Likewise, nutrient concentrations as well as pCO₂ levels showed a large increase within the eddy core which created significant anomalies when compared to ambient open-ocean ETNA conditions.”

RC24: Page 18, Line 9: Is the 3-fold increase in primary productivity consistent with your observed Chl-a?

AC24: Please refer to AC22.

RC25: Page 18, Lines 20-25: I would replace this text with a sentence on the significance of the 1 to 2 ACMEs generated every year that propagate into the open ETNA waters for biogeochemistry and ocean acidification of the area.

AC25: Thank you for this suggestion. However, please see AC4_ for details – as we decided to not replace this text with findings of Schütte et al. (2016), as at this part of the conclusions we explicitly want to look forward and give an outlook about open questions which need to be addressed by future studies. Instead, we will add a sentence to the end of the second paragraph of the conclusions as follows: “As revealed by Schütte et al. (2016) these ACMEs appear to play a small but significant role in maintaining the shallow OMZ in the ETNA.”

Tables:

RC26: Table 1: Add: negative values correspond to : : Are these average anomalies over some depth range?

AC26: We will edit the caption as suggested: “Overview of detected concentration anomalies (Δ_{total}) within the ACME core ($\sigma_{\theta} = 26.35 \text{ kg m}^{-3} - 1000$) during the two surveys referenced against prevailing conditions at the shelf. Rate estimates are based on the lifetime of the ACME derived from satellite sea level anomaly data (ISL: 163 days, M105: 173 days). Values for the average shelf profile are given in order to illustrate local variability at the corresponding isopycnal ($= 26.35 \text{ kg m}^{-3} - 1000$). Negative values correspond to a decrease of the respective parameter over the lifetime of the ACME.”

Figures:

RC27: Figure 2: SACW missing?

AC27: Both water masses are represented by gray dashed lines. We find the gray scale sufficient to get noticed both in its digital and printed form. However, we will double this once it comes to the technical processing of this manuscript.

RC28: Figure 8: This figure and the associated carbon flux model (eqn 2) does not make a significant contribution to the paper as it stands. See general comment #3 above.

AC28: Please refer to AC3_.

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