Biogeosciences Discuss.,
doi:10.5194/bg-2016-33-RC1, 2016

Biogeosciences Discussions

Interactive comment on "Characterization of "dead-zone" eddies in the tropical Northeast Atlantic Ocean" *by* Florian Schütte et al.

Anonymous Referee #1

Received and published: 6 April 2016

Summary

Schütte et al. use an extensive compilation of observation based data comprising of shipboard measurements, mooring data, Argo float profiles, glider data as well as satellite based products to characterize mesoscale activity in the Eastern Tropical North Atlantic (ETNA). In particular, their analysis focuses on cyclonic eddies (CE) and anticyclonic modewater eddies (ACMEs), the associated oxygen depletion within these mesoscale structures and their potential contribution to the pronounced low oxygen environment within the shadow zone in the ETNA with the subtropical gyre to the North and the equatorial region to the South. They find that almost all observations of low oxygen concentrations below a canonical value of 40 μmol kg^{-1} are co-located with either CEs or ACMEs that show negative oxygen anomalies which are most

pronounced right beneath the mixed layer. These anomalies are attributed both to high productivity in the surface waters and the subsequent respiration of organic material as well as to the dynamically induced isolation of the mesoscale structures with respect to lateral oxygen resupply. The authors conclude that the investigated eddies represent en essential part of the total consumption in the open ocean of the ETNA and partly contribute to the shallow low oxygen environment in the investigated region.

1 General comments

The presented work extends and complements previous work carried out by the community and the authors. In particular, the compilation of different observation based and quality-controlled data sources that extend previous records allow the authors to draw conclusions on the general characteristics and oxygen depletion within CEs and ACMEs in the studied region that advances our scientific understanding of mesoscale structures and their contribution to the mean distribution of biogeochemical properties. Moreover, the work is generally well-written, well-structured and results are presented in a clear and concise way. In my opinion, this manuscript thus represents work that is well suited for publication within the scope of Biogeosciences. Nevertheless, of course, I would like to make some comments and suggestions that should be addressed before publication and hopefully help the authors to further improve their work.

A) The use of the term "dead zone"
The authors use the term "dead zone" as a very prominent catchword throughout the whole manuscript. This term serves its purpose, but in my opinion, its use is not unproblematic. I think the use of this catchword is very colloquial and does not

acknowledge our scientific understanding of hypoxic environments that still provide habitats to specifically adapted species. Thus, it might potentially lead to premature interpretations and misunderstandings. To avoid these challenges, my suggestion is that the authors concentrate on phrasings such as “anoxic” and “hypoxic” and do not use “dead zone” in this context. If this term is used, it needs to be motivated, most importantly, but also discussed in the introduction in a more differentiated manner and the difficulties involved with interpreting such a catchphrase need to be appropriately addressed. In addition to specifically adapted species making use of these environments, marine organisms experience a highly non-linear sensitivity to low oxygen concentration and thresholds for hypoxia vary greatly among marine taxa (Keeling et al. 2010, Vaquer-Sunyer and Duarte 2008). A more elaborate motivation and differentiated discussion of the term can for example be found in the introduction of the review paper by Keeling et al. (2010) (see References at the end).

B) Quantification, Significance, Relevance and Implications

In my opinion, the presentation of some results in the current manuscript could be strengthened by clarifying certain paragraphs, putting results into a broader context and touch upon the relevance and potential implications of this work for other studies and concepts. Putting the results into a broader context can help a non-expert in mesoscale oxygen dynamics to better understand the relevance of this work. Reviewing some parts of the draft could add to the work presented here. Even though this is a major comment, let me get a little bit more specific here, to better convey my request:

Page 1, Line 24:
“increased consumption within these eddies represents an essential part of the total consumption...”. First of all, I think that this specific sentence of the abstract could benefit from some quantification. Second, in the discussion (Page 11, Line 18) you

present the results from your budget analysis of the SOMZ oxygen consumption, stating that mesoscale structures contribute to about 6% of the observed low oxygen distribution. Even though this value is probably underestimating the total effect, as you argue in your work, 6% is not an essential part, in my opinion (please correct me if I misunderstood the line of argumentation). I think it’s important that these paragraphs (abstract, discussion and conclusion) reflect each other and causal conclusions are drawn and described in a way that numbers and descriptions add up to the whole picture, even if this means being careful with catchwords such as “essential” or “significant”. (Wouldn’t a phrasing such as “the investigated contribution of mesoscale eddies only amounts to 6% of the observed low oxygen in the SOMZ. This value, though, is very likely to be underestimated due to...” also reflect the results but be more consistent when comparing the numerical and descriptive presentation?)

Page 8, Lines 20-21:
Can these estimates of oxygen consumption be put into the context of other observations, studies or estimates? How do these values in general compare with available estimates of average oxygen consumption? Are the results presented in the order of magnitude that the authors expected them to be, or is the effect stronger/weaker than what the authors expected? The way the results are presented here makes it hard for the reader to understand the magnitude of the mesoscale effect. Providing more context and comparisons would really help here.

Page 11, Lines 8-26:
This is a very important part of your work. I think it could be strengthened by re-phrasing some parts, putting the numbers into a broader context by providing comparisons that help the reader to better understand the magnitude of the discussed effects, and consistently present these findings in the abstract and conclusions (see comment above). I think this budget estimation is a central part of your work and very

well motivated on page 2 (lines 39-40), thus, in my opinion, it should be mentioned in the conclusions and the abstract. Please note the technical comments below to correct errors in this paragraph that, unfortunately, hinder the clear communication of these results.

Last but not least, your work naturally has implications for the nitrogen cycle. I am aware of some of the co-authors having submitted a manuscript on this issue as well (Karstensen et al. 2016). Nevertheless, I think it might help to at least mention some of the major implications for the nitrogen cycling within these mesoscale structures and the whole investigated region. Interested readers of this work might expect the authors to at least touch upon this or refer to the relevant literature.

2 Specific comments

A) Chosen threshold of 40 μmol kg^{-1}
Given a more differentiated discussion of the term "dead zone" (see comment above), can the authors elaborate on why they chose the specific threshold of 40 μmol kg^{-1} and whether and how they would expect their results to change when choosing, e.g. a higher threshold (e.g. 60 μmol kg^{-1} as mentioned in Keeling et al. 2010)? Would that significantly change the number of eddies considered as "low oxygen eddies" and thus increase the investigated sample or even strengthen the results?

B) Physical contribution to the observed anomalies
In the abstract, the authors state that the most pronounced oxygen anomalies are found right beneath the mixed layer and that this signal has been attributed to a combination of high productivity in the eddies' surface waters and the isolation of their

cores with respect to oxygen resupply. I do agree on this reasoning. However, I would like to mention an additional effect that has not been discussed in the manuscript and potentially plays a role here. The mere fact that the strongest anomalies are found at the base of the mixed layer hints at a pure physical contribution to the observed anomalies. Since density structures are shifted within the investigated eddies, this results in shifting the oxycline (i.e. shifting the isopycnals) and thus creating an oxygen anomaly that is of pure physical origin. If this is the case, can the author at least discuss the contribution of this mechanism on the observed concentrations, and if possible comment on the strength of this effect?

C) Preconditioning through coastal environment
The presented apparent oxygen utilization rates range from about 0.1 (CEs) to 0.2 (ACMEs) μmol kg^{-1} d^{-1}. Even if the mesoscale structures are completely isolated and propagate offshore for, let's say, 2 months, this results in a oxygen decrease of only 12 μmol kg^{-1} compared to its initial oxygen concentration. It seems thus very challenging for this mechanism alone to cause "dead zone" eddies. I think it is important to note somewhere that not only do enhanced productivity in the mesoscale structures and their physical isolation cause these very low oxygen eddies, but that there is a substantial contribution to the generation of these structures from the coastal environment, where most of them originate from. The above mentioned oxygen consumption alone would never be strong enough to result in a "dead zone" eddy, if it hadn't evolved from waters already low in oxygen along the upwelling region. I think this preconditioning is an important piece of the whole picture and should be briefly discussed somewhere.

D) The use of the term "accuracy" (Page 4, Lines 13, 17, 20 and 25)
The use of the term "accuracy" in the discussed context on page 4 confused me. To my knowledge, this term refers to the closeness of a measurement to a standard

or known value with “high accuracy” referring to “close measurements” and “low accuracy” describing rather poor measurement results. In general, one thus aims at high accuracies when observing natural phenomena and comparing to standard values. Here, the authors argue that the measurement methods have a rather high accuracy, but then state very low absolute values. Since the authors are describing measurement errors in the corresponding paragraph, I suggest they at least consider re-phrasing the sentences to ease the reader’s understanding (e.g. using the term measurement error). I am glad to learn something about the correct use of the term “accuracy”, in case I am wrong here.

E) Discussion of other mesoscale features (anticyclonic eddies)
On page 4 (line 30), the authors mention that their work also includes anticyclonic eddies. This eddy type is however not mentioned again. Even though I understand that the oxygen dynamics in eddies are strongly asymmetric between cyclonic and anticyclonic eddies, I wonder whether there is a compensating effect of anticyclonic eddies that stronger ventilate the water column. Could the authors elaborate on this, and maybe include a very brief comment on this in the manuscript?

F) Figure 7 and Figure 9:
As I understand, Figure 7 depicts mean profiles of apparent oxygen utilization of all eddies derived from the corresponding initial and actual oxygen profiles assuming a linear oxygen consumption (correct me if I am wrong). According to the corresponding figure caption of Figure 9, this figure shows the same property (μmol kg^{-1} yr^{-1} instead of μmol kg^{-1} d^{-1} in Fig. 7). This confused me because the magnitude shown in these two figures does not compare well. Can the authors comment on the difference between the two figures, if necessary elaborate on the corresponding text (Page 11, Lines 2-4) to better differentiate between the two results and maybe adjust the figure captions to help the reader understand their difference?

3 Technical corrections and minor issues

What follows is a list of minor technicalities and other issues I noticed while reviewing. I kindly ask the authors to correct typos and misspellings, reply to my questions and at least consider suggestions and comments on the (re-)phrasing of some sentences that might help to improve the reader’s understanding.

Page 1, Lines 24-25: consumption of what?

Page 2, Line 28: consumption of what?

Page 3, Line 4: The use of “However” in this sentence is rather confusing since it doesn’t contrast to what has been said before. Suggestion: “Due to the absence of other ventilation pathways in this zone, the influence of “dead-zone” eddies on the shallow oxygen minimum budget may be important and a closer examination worth the effort.”

Page 3, Lines 10-11: As mentioned above, the mere fact that the density structure changes within these structures might add a purely physical contribution to the observed anomalies. Thus, it is not only due to biogeochemical processes that the anomalies are strongest at 100m depth, but rather due to a combination of both a purely physical displacement of the oxycline and biogeochemical processes in the water column above. This sentence should be re-phrased.

Page 3, Line 35: as THE last modification

Page 4, Line 27: as A final result

Page 4, Line 41: provided BY (phrasing of sentence is rather confusing)

Page 5, Line 7: data ARE considered (plural)

Page 5, Line 9: provided BY the NASA. The data WERE

Page 6, Line 1: Full stop missing (. . . propagation time is derived. We assume a mean. . .)

Page 6, Line 6: less saline and colder water than surrounding water

Page 6, Line 13: Depending on the status of isolation of the eddy, lateral mixing could take place (comma missing)

Page 7, Line 13: At its closest, the eddy center was . . . (comma missing)

Page 7, Line 18: blank space in unit missing

Page 7, Line 22: westward PROPAGATING eddy

Page 7, Line 37: data REVEAL (plural)

Page 8, Lines 26-27: If Figures 8 really depict normalized radial distances (as I assume), I suggest this is mentioned not only in the text, but also in the figure caption. Maybe the axis labeling needs to be adjusted as well. The same comment goes for Figure 6.

Page 9, Line 6: for THE ETNA

Page 9, Line 20: As discussed in Schütte et al. (2015), in case . . . (comma missing)

Page 10, Line 6: In the discussed context of eddy generation mechanisms, this formulation could be a little bit confusing, i.e. the word “generate” could be confused with eddy generation. Suggestion: I assume the authors would like to say “However, both eddy regimes feature eddies which locally ESTABLISH open ocean upwelling systems with high productivity at the surface and enhanced respiration beneath the ML during their westward propagation.”

Page 11, Line 2: each year are propagate from the upwelling system near the coast

into the SOMZ and dissipate THERE.

Page 11, Line 8-10: This sentence should be re-phrased.

Page 11, Lines 16-19: Lines 16-19 (Attributing the oxygen concentrations. . .) are lacking in clarity and don’t convey the intended message. Line 17 has an unnecessary parenthesis. Needs to be corrected and re-phrased.

Page 17, Line 7: Maybe a reference to Table 1 might be useful here for more information on M97.

Page 17, Line 9: around 80m depth (not plural)

Page 18, Line 3: Map of THE ETNA

Page 22, Line 4: b) CEs (use the introduced acronym)

Page 22, Line 5: when compared TO the SLA and SST

References

Keeling, RF, Kortzinger A, Gruber N. 2010. Ocean deoxygenation in a warming world. Annual Review of Marine Science. 2:199-229.

Vaquer-Sunyer R, Duarte CM. 2008. Thresholds of hypoxia for marine biodiversity. Proc. Natl. Acad. Sci. USA. 105:15452–57

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Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-33, 2016.