Biogeosciences Discuss.,
doi:10.5194/bg-2016-23-RC2, 2016
© Author(s) 2016. CC-BY 3.0 License.

Biogeosciences Discussions

Interactive comment on "Oxygen Utilization and Downward Carbon Flux in an Oxygen-Depleted Eddy in the Eastern Tropical North Atlantic" *by* B. Fiedler et al.

Anonymous Referee #2

Received and published: 19 March 2016

Summary

The paper by Fiedler et al. describes a mode water eddy surveyed in the Eastern Tropical North Atlantic and establishes a link between the local biological activity and the extremely low oxygen concentration in the eddy core. To do so, Fiedler et al. present and analyze the data collected by two research vessels in correspondence of an anticyclonic mode water eddy detected in March 2014 in the region above Cape Verde Islands. They compare these measurements with both data referring to the ambient background condition in the Tropical North Atlantic and data collected on the Mauritanian shelf where the eddy was formed. The mode water eddy shows

sub-oxic conditions in the core at about 100m depth. Fiedler et al. demonstrate that the extremely low oxygen concentration is associated with a decrease in pH, a drop in the aragonite saturation level, and a higher DIC and higher nutrient concentration respect to the open Atlantic measurements. They find that the low-oxygen core is characterized by a drop in particle concentration, POM and DOM respect to the highly-productive surface of the eddy. The authors calculate a remarkably high apparent oxygen utilization rate and POC flux for the eddy core, using as a reference the data collected from the Mauritanian shelf. All these results are interpreted as a clear sign of elevated respiration rates in the oxygen-depleted eddy center, and support the causal connection between such biological activity and the remarkably low oxygen concentration.

Contribution

Oxygen-depleted mode water eddies have first been described and studied only a few years ago, and constitute a very novel and interesting object of study. Many mechanisms regarding the formation of this extremely low-oxygen environment still need to be clarified. The connection between physical and biological processes during the evolution of these kind of eddies is still unclear and the biogeochemical data are still scarce. Oxygen-depleted mode water eddies represent some interesting natural laboratories for the study of biological activity in suboxic conditions, and are relevant for the prediction of extreme biological conditions. In fact, they can release their oxygen-depleted water when crossing an obstacle such an island, drastically impacting the local ecosystem and representing a sort of wandering bomb.

Fiedler et al. present some new and exciting biogeochemical data collected in correspondence to such an eddy, contributing to clarify the characteristics oxygen-depleted eddies with some precious piece of information. They present an interesting

comparison with the open Atlantic background state that supports the hypothesis that the oxygen-depletion is due to intense local biological activity that strongly contrasts with the surrounding oligotrophic waters. They highlight the implications that the decrease in pH and the Aragonite saturation in the eddy core level may have on the local and potentially impacted ecosystems. However, they are not able to shed light on the evolution of this biological activity during the eddy lifetime (about one year) due to the scarcity of data collected by the two cruises.

Fiedler et al. also calculate the rates of apparent oxygen consumption and POC fluxes in the eddy core and find very high values for both quantities compared to the expected values for an oligotrophic region. These results strengthen the hypothesis in which the oxygen depletion is a consequence of high respiration in the eddy core. For these calculations, they use the data collected by 3 independent cruises on the Mauritanian shelf as a reference for the initial biogeochemical composition of the eddy. These cruises were conducted in 3 different years in the shelf region that is statistically the most likely region of eddy formation. The authors' calculations rely on the assumption of representativeness of these shelf measurements and on the assumption of complete isolation of the eddy core from the surrounding environment, from the eddy formation region on the shelf to the offshore region where it was sampled.

Recommendation

I find the subject of the paper novel, interesting and well suited for being published in Biogeosciences. The results of this paper are important for making progress towards the understanding of the biogeochemical activity in anticyclonic mode water eddies and explain the processes behind the formation of their oxygen-depleted core. However I think that the authors need to better motivate the assumptions

made for the POC flux and apparent oxygen consumption calculations (see "Major comments", nr.1). Moreover, aside from the scientific aspects of the paper, I feel the urge to underline the necessity to improve and adjust the style of the writing and to proof-read the paper for typos and syntax. Some of the basic information in the paper needs to be reorganized and clarified in a more efficient way to ease the communication of the scientific content. At the present state, the paper is extremely difficult to read and contains several major oversights that make it sound like a draft paper. In my opinion, an improvement of this aspect is essential for making the paper publishable. I suggest that this paper is accepted after major revisions are made.

Major Comments

1) Reference Data Set: an assumption that needs to be strengthened
The three Reference Data Sets used by the authors for the bgc fluxes calculations are from cruises that were conducted on the Mauritanian shelf around the second-half of July 2006, beginning of June 2010, and beginning of June 2014. The surveyed eddy is supposed to have formed on the Mauritanian shelf around June/July 2013. This can be inferred from the paper, but it's not explicitly stated. At page 8 lines 17-21 the authors explain how they reconstructed the region of origin of the eddy on the shelf on the base of statistical analysis of historical SLA, and how this region coincides with the location of the 3 Reference Data surveys. However, the eddy trajectory from SLA in "Figure 1" starts about two months later (Sept. 2013) at least 100km in the off-shore direction. The fact that the trajectory of this specific eddy was not retrieved on the shelf that may imply, for example, that the eddy boundaries of this eddy were not already well formed, therefore the eddy may have continued to trap water while leaving the shelf area of the Reference Data Sets, or may have been spun by a lateral filament.
At page 11 lines 4-5 the authors underline the matching between the Temperature and Salinity of the eddy core when it was surveyed and the reference station mea-

surements on the shelf. This supports the assumption of isolation of the eddy core from the shelf to the offshore waters. However, biogeochemical properties can be more variable than physical properties on both spatial and time scales, especially in active shelf regions. The authors write about the Reference Data Sets [page 8 lines 23-27] "in order to account for small scale variability [. . .] an average profile for each investigated parameter was created [. . .]. These mean profiles were assumed to represent typical initial conditions of ACMEs [. . .]".
Given the complex dynamics of the flow around the shelf edge, the time and spatial variability of biogeochemical processes, the timescale of sporadic upwelling events; given the fact that the eddy trajectory from SLA was not retrieved in the shelf region, and the complexity of the eddy formation process:
1. Can the choice of these Reference Data Sets be better justified? Are there no available data for the region in which the trajectory was actually retrieved in Sept.2013?
2. How do mean profiles account for small scale variability?
3. Is it possible to exclude strong discontinuities (input of external water, sediment resuspension, interaction with other forming eddies, etc.) in the eddy evolution between the shelf region of the Reference Data Sets and beginning of the track in "Figure 1"?
4. Several times in the article the authors refer to sporadic upwelling events fueling the high surface productivity in the eddy. How is the hypothesis of "production being boosted in the surface of the eddy by upwelling events" compatible with the hypothesis of "complete isolation of the eddy core" along the whole eddy lifetime? What is the spatial distribution of these upwelling events in the eddy?

2) Description of the surveyed eddy
The authors do not provide a clear general description of the characteristics of the surveyed eddy, among which some basic details: date (month/year) and coordinates of the eddy when forming on the shelf; date and coordinates of the beginning of the

track; date, coordinates, radius, shape and age of the eddy when surveyed. Some of these characteristics can be retrieved in different parts of the article explicitly or implicitly, but it is the work of the reader to collect them. I suggest presenting these characteristics in a dedicated paragraph where the eddy is introduced and described.
As regard to "Figure 1": it may be helpful to add a timescale of the eddy trajectory and to draw the eddy contours in the region of the cruises, to understand where the measurements were located with respect to the eddy center and boundaries.
Most of the observations described in the chapter "3 Results Discussion" would be much easier to understand if a nice description of the vertical physical structure of the surveyed eddy was given.

3) Description of the dedicated eddy surveys
In paragraph "2.1 Eddy surveys" the number of samples that were collected during each cruise is not clear and some of the descriptions are confusing. As the 2 cruises are described as "first dedicated biogeochemical surveys" of the eddy the reader may expect to see some 2D biogeochemical sections and wonder what the spatial resolution of the samples is. However, it becomes clear later that the biogeochemical data analyzed in the article consist in only 2 bottle measurements per cruise, 1 per cruise referring to the eddy center. I suggest stating this clearly in the text.
The CTD/UVP-only (no bgc) section M105 is introduced but never plotted or clearly discussed. At page 5 lines 19-24 the authors write that some stations supposed to be at a certain distance from the center of the eddy on the base of SLA "turned out probably more at the rim of the eddy than in the surrounding water representing typical background conditions". What is the reason for this conclusion? Can this be elaborated more in depth to justify this sentence?

4) Quantitative results
Some of the results presented in the sections from 3.1 to 3.4 are not well quantified. Data collected in the eddy center and data on the shelf or in the open Atlantic are often compared with not-well-defined or confusing terms. I strongly suggest giving to the descriptions a more quantitative flavor.

5) Style: English, typos
The paper, apart for a few sections, is scattered with typos, misspellings and incorrect formulation of the English sentences. Sentences are often very convoluted and difficult to follow. The frequent use of bracketed subordinates makes the reading process even more complicated.
I highly suggest a proof-reading of the paper for typos, grammar and syntax, as well as a simplification of the structure of the sentences and the limitation of the use of brackets to the very essential. Some errors are listed in the "Detailed comments" section.

Detailed comments

1) Introduction: The section "Introduction" of the paper ends with a paragraph (from page 3 line 29, to page 4 line 10) that introduces the content of the article. However, it forgets to anticipate any section about consequences and conclusions of the present study. I suggest to strengthen this paragraph in this sense, anticipating to the reader the presence of relevant conclusions connected to the results.

2) page 2 line 25: Eastern Tropical should be capitalized when defining acronym

3) page 3 lines 18-20: incorrect syntax, "that" should follow the name that it refers to (ACME)

4) page 3 line 27: commas out of place

5) page 3 line 29: word "process" is redundant

6) page 4 line 2: misspelling "describes"

7) page 4 lines 16-19: sentence beginning with brackets; confusing sentence, "in the ETNA" better after "in situ-data", maybe the sentence should be divided in two parts

8) page 4 line 28: unnecessary brackets

9) page 5 line 8-10: data is a plural word, "were", "do"

10) page 5 lines 11-12: if the quality of the measurements is lower then it's half the accuracy (not double), the error doubles, the accuracy halves; numbers should go before the "for"

11) page 5 line 21: misspelling "turned"

12) page 6 line 10: sentence in brackets should actually be better illustrated and high-lightened since it's an important piece of information for the whole paper

13) page 6 line 22: unnecessary brackets

14) page 8 line 9: “by” not “from”

15) page 8 lines 9-12: unnecessary brackets; The vertical structure of the eddy is unclear: What is the depth of the euphotic zone and how does it compare with the depth of the eddy core? What is the depth of the mixed layer? Is primary production only taking place in the shallow mixed layer as it may be hypothesized from the chlorophyll plot, or is primary production also happening in the core? Is the core still in the euphotic zone? These points should be very well clarified also in the “Results” sections

16) page 8 line 13: misspelling “resembles”; I find it not so proper to say that this sporadic upwelling resembles coastal upwelling, it’s probably Ekman pumping, which does not require a coastal boundary to happen

17) page 8, lines 17-31: this description should in part be moved to an eddy description section and in part be included in the 2.2 Reference Data Sets section

18) page 8 lines 23-25: sentence is hardly understandable, “but” is incorrectly used

19) page 8 line 29: “en route”?

20) page 9 lines 4-6: “remineralization”, not “mineralization”; words before acronyms should be capitalized; “age” doesn’t need quotation marks; very convoluted sentence, could be split in two parts

21) page 10 line 9: there should be a comma (not a dot) before “as”

22) page 10 line 24: misspelling “resemble”

23) page 10 line 25: “predominating” not the right word, maybe “dominant”

24) page 10 lines 26-27: not sure if this sentence is needed. Either the paper includes a full description of the basin and relative water masses (eg, in the introduction) or it seems out of place; also: what is the typical TS signature of SACW that is also found in the core of the eddy? A reader my not be familiar with this water mass

25) page 11 line 8: I don’t understand “vertical contrast”, does it mean “gradient”?

26) page 11 line 11: “underway”?

27) page 11 line 22: “minimal” not an adverb

28) page 11 lines 25-26: “inside the ACME” seems to refer to the whole eddy, are these lines referring to remineralization that happens in the low-oxygen core?

29) page 12 line 25: clearly missing reference (!)

30) page 13 line 5: paragraph on DIC should probably end here, not at line 3

31) page 13 line 6-7: not quantitative, not clear, terms as “minor change” and “small

but significant" should be defined

32) page 13 lines 17-18: "data not shown" used for drawing conclusion does not strengthen the paper, given the limited number of plots and their simplicity maybe some of the data could also be shown; same for the next "data not shown" in the paper

33) page 13 lines 20-32: all the detected small particles are assumed to be POM; this assumption is not explicitly stated even though it is at the base of the conclusions, I suggest to state and justify the assumption for this region. Are dust-deposition-derived particles irrelevant in this region/season?

34) page 13 line 23: convoluted sentence

35) page 13 line 24 and 27: "significantly exceeds" and "much higher" not very quantitative

36) page 13 lines 30-31: "according to Hauss et al (2015)", is this the same eddy?

37) page 17, lines 22-32: In the first paragraph of the conclusions the authors are very generic about their findings regarding the eddy bgc composition discussed in the sections from 3.1 to 3.4. Some of the results are not recalled (eg, particle, POM and DOM distribution). Since many interesting findings are discussed in the paper, I strongly suggest strengthening this part of the conclusion.

38) Figure 2: colorbar and legend seem to contradict each other: if orange points refer to ACME (M105), what do the blue points refer to? This choice of colormap is unhelpful,

the color is mostly constant.

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-23, 2016.