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Review of Karstensen et al. : Upwelling and isolation in oxygen-depleted anticyclonic modewater eddies and implications for nitrate cycling.

Special issue: Hydrography, biogeochemistry, and biology of "dead-zone eddies" in the eastern tropical North Atlantic.

GENERAL COMMENTS

This work is a contribution to a special issue about "dead-zone eddies" in the Eastern North Atlantic (ETNA) where 6 manuscripts are currently available, 3 already reviewed and published in BG and the rest in discussion form.

To be concise I consider Karstensen et al. (BGD, 2016) needs MAJOR REVISION, the reasons are exposed below. My main concern about this work is the lack of a clear focus on the hypothesis, results and discussion, is it about chemical or physical oceanography?. Another important consideration is that I needed to read carefully four manuscripts within the special issue to deeply understand the results and the discussion, the manuscript (ms) is full of typos or miss-references to the figures. It seems that the authors did not check the ms coherence before submitting, this is a very bad point for their reputation. Considering the amount of coauthors an effort should have been done to ease the reading of the ms and make it a stand- alone work.

Despite this I think the ms merits to be published after some improvements both in content and layout. I understand that it is somehow difficult to organize the wealth amount of data recorded by the different surveys and observing platforms deployed to characterize this intriguing new dead zones in the ETNA.

In addition this paper is mostly about physical oceanography, and I am a chemical oceanographer, maybe the ms needs a third opinion.

A fundamental issue is the prime hypothesis of this ms which is finally resolved in Fig.7, the authors propose a physical mechanism to explain the isolation of the eddy core but also another one (near inertial waves, NIW, breaking) to explain the flux of nutrients to the upper mixed layer. As the authors say in the text the evidences to support the physical mechanisms suffer from "not having concurrent hydrography and currents data and limited options for estimating balances" (P14, L3-4). On the biogeochemical side, the authors only support their " nitrogen cycling" hypothesis with

nitrate and oxygen data from the glider surveys, but other measurements are available from typical CTD casts as described in Fiedler et al. (BGD 2016).

SPECIFIC COMMENTS

1. Introduction

Although the intro is rather long, just the last three lines contain some references to the other ms related to the studied Anticyclonic Mode Water Eddies (ACME) within the same project and using the same observing platforms. I think a comprehensive summary of the different genomic, biological and biogeochemical aspects of the ACMEs should be given, also highlighting the contribution of the current ms.

2. Data and methods

2.1. Glider survey

Maybe a word or reference about the interpolation method for the glider data would be interesting.

2.2 Glider sensor calibration

Page5, line 16. I would like to see some number about oxygen precision and accuracy, as done for nitrate (P6, L7-8). Although more details about this are surely given in Hahn et al 2014, please consider my demand.

2.3. Ship survey

I do not understand why not using the biogeochemical data gathered during M105, at least NO3, PO4, O2, particulate and dissolved organic matter, to sustain your biogeochemical interpretation of the results. More comments about this issue will be given in the corresponding section of the ms.

3. Results and Discussion

3.1 Vertical Eddy Structure

Biogeosciences is not "Journal of Physical Oceanography" so my excuses for not understanding all the difficult terms in this section. As the aim of the ms is explaining the "fluxes of nitrate" into the mixed layer supporting the high primary production in the ACME, my opinion is that an effort should be done to make the ms more readable for the ocean biogeochemical community.

P9L5-9. I checked (I read) Fiedler et al 2016 and I did not find any explanation about the translational velocity of the ACME, I found this information in Karstensen et al (BG 2015).

3.2 Eddy core isolation and vertical fluxes.

Please check the figure references in this section, it is a mess!!

It was very hard to follow the result description and the final message to be conveyed.

P9-L13 no reference to limnic systems is given in Karstensen et al (2015).

P9-L19: the nonlinearity parameter is not defined or commented previously in this work but in Karstensen et al (2015). Please explain why alpha is important for the coherence of the eddy but it does not matter to explain isolation.

P10-L2-3. Weird phrase.

P11. A mess with the figure references. Please just for the biogeochemist summarize where would NIW brake and induce mixing / fluxes in the eddy structure.

P11-L8-9. "no concurrent velocity and stratification section data exists" I do not understand, you have velocity and CTD casts from the ship so at least you have 8 stations.

3.3 Nutrient budget.

This section should be entitled "nitrate budget"... but not even so... as no budget is estimated, a better title would be "nitrate cycling" .

My main concern about this section the rejection of using other biogeochemical data from the ship surveys within the ACMEs. For example why not using the M105 NO3 and AOU data in Fig 6c?, they crossed the eddy center as showed in Fig 2b.

An evidence of denitrification would be a differential NO3:PO4 ratio.

After reading several times this section, the main question is how are the nutrients inyected into the mixed layer to support primary production?. However no profile of chlorophyll is given (I found some info about this in Loscher et al. BG 2015) , I wonder if the gliders have at least a backscattering or fluorometer sensor.

The biogeochemical info in Fiedler et al BGD 2016 in the shelf, CVOO and the eddies may help to explain the high primary production (PP), if eddies are formed in the shelf, they contain nutrients that are used and converted into organic matter (particulate and dissolved ) that sinks and is remineralized in the eddy creating the O2 minimum. Is it enough the initial NO3 in the shelf to sustain PP in the eddy when it moves into the ETNA?. Does it really need an extra NO3 input?.

It is very hard to understand a decoupled O2 and NO3 cycle if denitrification is not important. Please check the NO3:PO4 ratio. An anomalous O2:NO3 ratio could be related to the stochiometry of the organic matter remineralized both particulate and dissolved, please check the available data.

4. Summary and conclusions

I suppose it would need to be rewritten depending on the results from section 3.3.

I hope to have been helpful.