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# *Interactive comment on* "Winter mixing, mesoscale eddies and eastern boundary current: Engines for biogeochemical variability of the central Red Sea during winter/early spring period" *by* Nikolaos D. Zarokanellos and Burton H. Jones

**Anonymous Referee #2**

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This paper deals with physical processes impacting the biogeochemical variability in the central Red Sea using underwater gliders data. These specific platforms are adapted to the studied processes in the study (i.e. meso and sub-mesoscale processes) making relevant data available. Thus, some results of this study could be publishable and valuable for the scientific community in the future. However, the paper needs a lot of work before publication. There are some major issues, especially with the processing of the bio-optical data and their transformation in biogeochemical properties. Moreover, the draft is unfinished with many errors especially in the legends of



the figures, which is unpleasant for the reader and makes the paper very hard to follow. Thus, from my point of view, the current version of the paper is not publishable in the journal Biogeosciences. More details are provided below:

GENERAL COMMENTS

-As stated above, further work on the processing of data has to be done. Major issues on the transformation of raw data in biogeochemical parameters need to be fixed.

-The different sections used to discuss some physical processes (e.g. winter mixing, eddy) are chosen arbitrary from the author. This needs to be supported by more scientific arguments. Moreover, the authors have to provide evidences of the eddy existence (maybe using satellite data?) but also details of the position of the glider from the eddy core during the data acquisition.

-I think that the text has to be re-ordered. The discussion is more a "Results" part (see details in specific comments). The authors could merge the discussion and the results parts in a "Results and Discussion" part.

-This is very hard for the reader to understand the figures of this paper. Some figures are difficult to read, or are even unreadable (e.g. too tiny labels, see specific comments). Moreover, there are too much errors in the legends of the figures for a submitted paper (e.g. parameters not plotted)!

-Authors have to harmonize the abbreviations in the document.

SPECIFIC COMMENTS

-Abstract

l. 21: I think that this statement has to be moderated as the nitrate concentration is not directly measured from the glider.

l. 23: CHL instead of chlorophyll

l. 30: ML instead of mixed layer

-Introduction

l. 68: Please add in the Red Sea.

l. 70-71: Satellites do not sample, I would re-formulate this sentence.

l. 77: CHL has not been introduced before in the text (except in the abstract).

l. 77-78: I do not really agree this sentence. . . Or at least add references.

l. 91: The authors should do a link between primary production, particle concentration (which one?) and bio-optical properties.

l. 96: Some references should be added here.

l. 100: bbp is the particulate backscattering coefficient and not backscatter.

l. 99-100: Authors do not provide in this study relationships between CHL and CDOM or bbp and CDOM.

l. 102-103: Where this is shown in the paper?

l. 104-105: I would remove this paragraph, that is not essential.

-Data Sources and methodology – Glider observations

l. 119: Where is located the reef system Shi'b Nazar?

l. 131-132: Did the authors remove both negative and positive spikes? If yes, why? Positive spikes have a biological meaning.

l. 146: Even if they not present any measurement for the Red Sea, I think that it is important to mention the paper of Roesler et al. 2017 (Limnology and Oceanography: Methods).

l. 147: CHL measurements: where, when and how these measurements were ac-



quired?

Did the CHL profiles were corrected from the instrumental dark signal?

l. 147-148: I think Morel and Maritorena reference is not well placed because it refers to the calculation of Zeu from Chl?

l. 150 – 183: Are there some differences with the processing of bbp data from profiling floats (see Schmechtig et al. 2014)? And if yes, what are these differences?

l. 173: bbp is commonly referred as b[bp] (with bp as an index). Moreover, in the equation bbp is well referred but not in the text.

l. 185-186: From my point of view, there are some important issues here.

1) no explications are given for the conversion of bbp(532) in bbp(700), unless it is the reference to Tiwari and Shanmugam, 2013? If yes, the reference is not well placed, the authors should move this reference to the first point of the sentence. As the glider measures the bbp at 3 wavelengths, I would suggest to compute the spectral dependency of the bbp to retrieve the bbp(700). See Loisel et al. 2006 for instance.

2) "and then to POC concentrations using the various empirical relationships". The word "various" is misleading, which one is used here? Or maybe this is an average of all relationships presented in the Table 2 of the paper of Cetinic et al., 2012? Unless I make a mistake, Boss et al. 2013 do not present any measurements of bbp and estimate the POC from the relationship of Gardner et al. 2006. If the authors use the relationship of Cetinic et al. 2012 (which I think is the case because bbp(700) is used) this is a linear relationship so it is not necessary to present both parameters in the figures.

In any case, authors have to moderate the use of POC in their study because many uncertainties are associated with the conversion of bbp in POC, even more in semi-confined environments with specific physical and biogeochemical properties associated, such as the Red Sea.

l. 196: How AOU data were calibrated?

l.199-200: “Because the regression intercepts the oxygen axis between ..”, where this statement is shown?

-Data Sources and methodology – Satellite data

Authors have to provide the resolution (spatial + temporal) of the satellite data used in this study.

-Data Sources and methodology – Statistical Analysis

L 225 + l. 228 + L. 231 + l. 238: Tables have to be introduced in the “Results” part.

l. 227: integrated in which layer?

l. 232: The first optical depth has not been introduced before and it is never explained how this is computed?

l. 236-237: Abbreviations are not the same throughout the document.

-Results

As I mentioned above, I think that a lot of work is needed upstream (transformation of bio-optical data in biogeochemical parameters) before reviewing all this "results" part. Thus, here are some comments but it is not an exhaustive list:

l. 256: what means 5m? The authors should show the lag in Figure 2.

l. 258: In Figure 2, the reader cannot see the correlations that the authors mention in the text. Some statistics are needed here.

l. 270 – 280: The authors have to better explain with more details this part of the study. Some data have to support the arbitrary choices of the different sections.

l. 282: How is the NOx computed? It is not well explained in the Material and Methods. Is this a linear relationships based on the AOU? If yes, this plot does not bring any



information to the reader. This is the same issue for the POC with bbp data.

l. 322: How to be sure that this is the core of the eddy?

l. 334: I think that the values for the integrated Chl are false; this is not the range of values expected for the integrated Chl. Do the authors mean the Chl in the whole water-column?

The authors should discuss the ratio Chl:bbp and photoacclimation process.

The Figure 9 does not show any relationships. Where are the statistics associated? See Barbieux et al. 2018 for the red sea. The authors should transform their axis as log/log in order to discuss their results by comparison with the results presented in Barbieux et al., 2018.

-Tale 1

The abbreviations have not been defined before (e.g. what C refers to?).

-Tale 2

It would be useful to add the number of points used to compute each correlation. I am not convinced that the relationships shown here are really robust because of the few data used.

-Table 4

What the grey lines indicate?

-Table 5

Exceed 50% of what?

-Figure 2

The authors have to harmonize sea and air temperature and to add the sea temp in the legend.

-Figure 3

Define the horizontal axis in the legend (latitude?). In the last sentence of the legend, detail in which panel is represented the white line. I would suggest to harmonize the colorbars, except maybe for the BVF, there is no need to have different colorbars for the others parameters. I would put the a), b), etc.. . . in black. The line is blue and not magenta. Labels are too small, it is difficult to read. Is it really necessary to plot the parameters up to 400 m in Figs 2 and 3? I would suggest only 250 m to better see the variability of biogeochemical parameters.

-Figure 4

Same comments as for Figure 3. Moreover, there is no plot with NOx. . ... The min and max values in the colorbars are not well defined, the reader cannot see correctly the parameters dynamics. The authors shouls limit the plot to 0-250 m as suggested above. I would not put POC panels (linear relationship with bbp).

-Figure 5

Colorbar of panel c not really adapted as every point seem to be green or blue

-Figure 6

POC and bbp: same issue as in Figure 4

-Figure 7

Labels are too small, it makes the plot unreadable. Moreover, the legend does not help the reader as nothing is well explained for guiding the reader (e.g. no reference to panels a), b) . . .)! There is no panel for POC!

-Figure 8

The panels a), b) and c) are already presented in Figure 4. The velocity is not visible in the plot, it is much too small.



-Figure 9

The upper ocean (0-200 m) is in the 2 other categories, do the author mean 100-200 m? If the 2 categories DCM region and Surface layer are right, the blue points represent the 100-200 m layer.

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