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Biogeosciences Discussions

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

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We deeply thank the reviewer for their constructive suggestions. Below we address point by point their comments.

Responses to Reviewer #1 RC1: Numerous studies during recent years have focused on the central Red Sea, including on the relationship between physical processes and biological activity (e.g. Chen eta al., 2014; Zarokanellos et al., 2017ab; Triantafyllou et al., 2014; Dreano et al., 2016; Wafar et al., 2016). The present study presents

new data, mainly collected by ocean gliders, to investigate the effects of winter mixing, lateral advection and eddies on biological activity. The data is new, but parts of the results are either known or expected (e.g. Zarokanellos et al., 2017ab; Triantafyllou et al., 2014; Dreano et al., 2016; Wafar et al., 2016). As the data is limited to a short period, many of the statements on the relationship between different variables do not seem robust. In addition, there are no sensitivity tests to some of the arbitrary divisions into wind speed and glider sections.

AC: We are grateful to the Reviewer #1 for the constructive comments and suggestions. From our perspective, we believe that the results presented in the manuscript are really new and improve significantly our knowledge for the Central Red Sea. First of all, our analysis contains for first time high-resolutions observations during the winter to spring period for the entire Red Sea. Throughout the manuscript we tried to avoid any kind of "speculation" and we focused on the basic patterns revealed by the available observations. Furthermore, in our opinion, the concluding remarks extracted by the synthesis of the datasets used in the study are carefully substantiated and describe in a clear way the important features and processes that occur in a complicated, previously poorly understood region of the Red Sea. However, the reviewer's comments are very insightful. We address these questions below, hopefully in satisfactory manner of to the reviewer.

RC1: I would suggest the author to try synthesizing the data and results from the above motioned references (and few others) instead of focusing on a limited data set that cannot provide conclusive statements (examples below). Altogether, there is now a bulk of data from different sources and different seasons, that can be used to study physical-biological coupling in the Red Sea.

AC: We agree with the reviewer that a comprehensive synthesis of published and existing data is essential. However, the goal of this paper is to focus on the winter/spring transition in central Red Sea which as not been previously described in the literature. We intend to present a synthesis in a future paper.

RC1: Additional comments: The authors divided the period into three sections: winter mixing, intensified eddy, and strong eastern boundary current. However, no support to this division is provided. Can you demonstrate using satellite data, field observations, or operational numerical simulation the existence of strong eddy and EBC during the corresponding periods? Fig. 8, which is shown only toward the end of the manuscript, provides very limited evidence.

AC: Revised as suggested (lines 283-293). We describe the division of periods based on changes in the physical structure observed with the glider during the observational period. We examined the Absolute Dynamic Topography (AVISO) but did not see a clear signature of the eddy despite the strong signal of the glider data. We also examined MODIS ocean color data. The two best images from the study period are shown in figure 8. The CHL patterns are consistent with the surface velocity field measured by the glider and with subsurface density structure. Real time, operational numerical simulations are not yet available for the period.

RC1: Page 9, line 228: are the results sensitive to the specific division?

AC: We followed up with the reviewer's question, which we had not previously done. In fact, the line segment AB is on average 15% greater than the values from the segment ABC. We have modified the table and figure 7 so for all the six glider lines we compare only the AB segment

RC1: Section 3.1: a. Show the lag correlation between air and sea temperature.

AC: We have included a clarification statement in the revised manuscript in the 3.1 section (lines 264-267).

b. MLD is maximal when Daily mean air temperature is minimal on Jan 11, but similar air temperature appears on Feb 20, and MLD then is rather shallow: : :

AC: We have included clarification statements in the revised manuscript in the 3.1 section (lines 268-280)

c. Overall, weak correlation between wind stress and MLD. How sensitive are the correlations to the division into wind speed categories?

AC: We are grateful for the reviewer recommendation and we removed this part of the analyses as the overall correlation between wind stress and MLD is weak. Revised as suggested (lines 277-280).

Page 10. Line 273: MLD mixing is not monotonic, and there are cases with strong wind and no mixing.

AC: Revised as suggested (lines 269-277).

RC1: Page 10, line 276: Where do we see the existence of CE and AE? (the sections shown in Figure 3 are not enough to demonstrate that there are both CE and AE). Also, we expect the MLD to respond differently in CE and in AE.

AC: We thank the reviewer for his comment and we agree that the overall AE is not well defined from the existing observations. Revised as suggested (lines: 287-290; 303-304, 307, 311-314, 320-323, 326-327, 331-333 338-339, 358-361, 372-375,393-394, 534-535 and 647)

RC1: Section 3.4: can you distinguish between the contribution of the introduction of a new watermass from the Gulf of Aden and uplift of isopycnal by the CE?

Of course all water in the Red Sea originates from the Gulf of Aden. We can distinguish the water mass that has most recently originated from the Gulf of Aden based on thermohaline and optical properties [Churchill et al., 2014; Zarokanellos et al., 2017b]. CDOM is not typically used as water mass tracer but we have found that is consistently indicative of water that has recently originated from Gulf of Aden. Recent studies from other regions have shown the utility of CDOM for tracing and differentiating water with unique characteristics [e.g. Seegers et al., ECSS, 2016].

RC1: Minor comments (partial list) Cetinic et al. (2009) Is missing

AC: Revised as suggested (lines 736-738)

RC1: Figure 1: the blue is hardly seen

AC: Revised as suggested (Figure 1 has been updated)

RC1: Line 127: extra space

AC: Revised as suggested

RC1: Line 142: significantly?

AC: Revised as suggested (line 145)

RC1: Line 207: "study region"?

AC: Revised as suggested (line 221)

RC1: Line 548: there-> that

AC: Revised as suggested (line 570)

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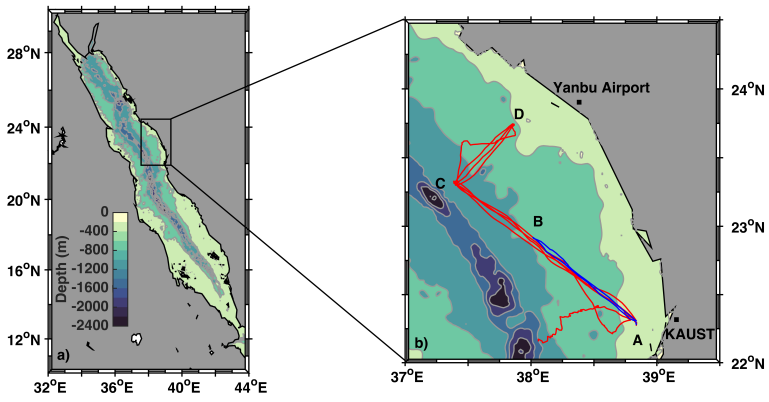

Fig. 1. Revised Figure 1