Review of the manuscript "Ventilation dynamics of the oxygen minimum zone in the Arabian Sea" by Schmidt et al., submitted to *Biogeosciences*

This paper claims to investigate the ventilation dynamics of the Arabian Sea oxygen minimum zone (ASOMZ) using a combination of observations and reanalysis velocity fields from HYCOM (Hybrid Coordinate Ocean Model). They perform Lagrangian trajectory analysis based on those velocity fields to understand the pathways of Persian Gulf (PGW), Red Sea (RSW) and Indian Central (ICW) water masses.

The ASOMZ ventilation by the PGW, RSW and ICW have extensively been discussed previously in several studies (the authors themselves cite those papers). It is not clear to me how this paper contributes in terms of new scientific insight and knowledge. I feel that the paper lacks novelty and does not have any new result. I find many inconsistencies throughout the paper and the manuscript is poorly written. Hence I am unable to recommend it for publication in Biogeosciences. Some of the major issues are as follows:

1) I find that the title and abstract of the manuscript are somewhat misleading as the paper is mainly about the Lagrangian pathways of water masses and remains completely decoupled from the oxygen dynamics in the ASOMZ.

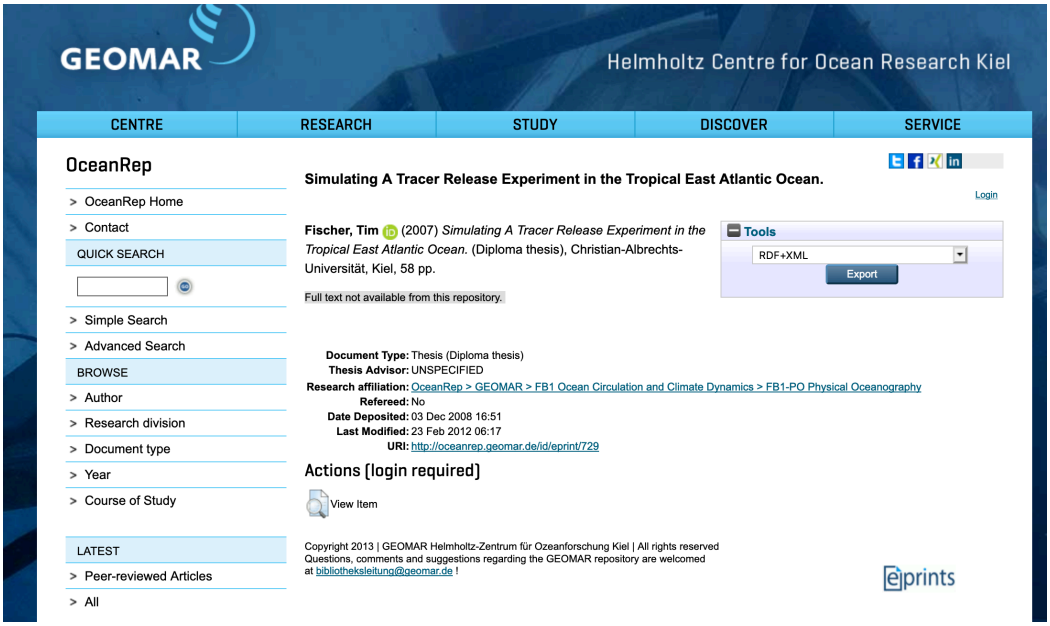
2) The paper uses velocity fields from HYCOM, which is a dynamical ocean model. How would you ascertain that the "model dynamics" is actually responsible for the "observed" characteristics of the ASOMZ? It doesn't make sense to me to draw inferences based solely on a physical model to understand the ASOMZ ventilation dynamics. A better approach would be to perform trajectory analysis on the velocity fields from a coupled physical-biogeochemical model that realistically simulates the observed features of the ASOMZ.

3) The velocity fields from HYCOM is the base of their trajectory analysis. Yet the authors provide no validation of these velocity fields. How good are the velocity fields simulated by HYCOM? Are they good enough to capture the ventilation dynamics of the ASOMZ?

4) The entire manuscript is poorly written. Most of the figure captions are not clear and so are the color (Figure 8, for example) legends.

5) The data and method section, which is very difficult for me to follow, does not detail the Lagrangian trajectory computation and experiments. The authors refer to Fischer (2007), which is a Master's thesis and is not readily downloadable as you can see below. How good is this trajectory method compared to the more advanced recent techniques (see Sebille et al.,

Lagrangian ocean analysis: Fundamentals and practices, Ocean Modelling, 121, 2018, p. 49-75, https://doi.org/10.1016/j.ocemod.2017.11.008).



There are jargons used – for e.g. “Spreading distance” - that are not defined in the method section.

6) The justification for the choice of isopycnal layer and the east-west release points are not convincing.

Additional comments:

1. Figure 1: What is the basis for the sketch of ASOMZ? What are those green curves within the green shade?

2. Figure 4: What is the meaning of standard deviation over 1 deg box when the grid size of WOA13 itself is 1 deg? Panel a, comparison of WR (blue) and ER (red) shows higher oxygen in west than in east during June – July (summer). How do you conclude that the ventilation is weaker in west than in the east during summer (Page 7; line 12)? Also, given the error bars in panel a, the ER and WR are not significantly different, I think. Moreover, the prominent seasonal difference in the thickness of suboxic layer, which is 1000 m in the east and 900 m in the west (panel b), is not significantly distinguishable given the error bar range. The text (Page 7, line 8) states that maximum thickness occurs during the winter monsoon with a depth of 1000 m (Fig. 4b) and nearly total oxygen depletion in the core (Fig. 4a). This is not really the case when we look at the figure! Indeed, fall season (Sep-Nov) shows those characteristics.

3. Figure 8: The discussion in Section 3.3 on this figure is very vague. Wrong reference to Fig. 8e (Page 13, line 7; also for Fig. 9e)

4. Figure 11: for April and October. But it is discussed in the text for summer and winter (page 10, line 32).

5. Page 12: line 17-19: The oxygen concentration is not high during winter, and the statements contradicts seasonality discussed in Section 3.1 and Figure 4a.

6. Inconsistent statements: For example in Page 14, line 2-4: “The analysis of a point to point transit time of particles that reach the marginal seas shows that particles travel at least 2 years from the PG into the eastern AS (Fig. 8d) along the perimeter.” In Section 3.3, it is mentioned as 4 years.

7. Loose statements: For example in Page 8, line 21: “The more direct interior pathway is less important”. How could you make such statements when there is no quantification of the contribution from pathways.

8. The authors sensitize this study by highlighting the decreasing oxygen trend and an expansion of ASOMZ under climate change (in abstract as well as in the introduction). The two studies that they cite do not specifically illustrate any significant deoxygenation in the entire ASOMZ. Also, some recent studies based on CMIP analysis indicate oxygenation of the southern part of the ASOMZ (Bopp et al., 2013).