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Interactive comment on "Ventilation dynamics of the Oxygen Minimum Zone in the Arabian Sea" *by* Henrike Schmidt et al.

Anonymous Referee #1

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The paper by Schmidt et al. investigates the dynamics of the ventilation of the Arabian Sea and its role in shaping the intensity and variability of the oxygen minimum zone (OMZ) located there. To this end, the study uses Lagrangian trajectories based on a velocity field derived from a reanalysis by the HyCOM model. The authors claim that the eastern part of the OMZ is ventilated mostly from the north by the PGW in winter whereas the western OMZ is ventilated essentially from the southeast during the summer season. The study investigates the ventilation seasonality and timescales as well as the potential role the Arabian marginal seas (the Red Sea and the Persian Gulf) play in this regard.

I) General comment:

The subject of the paper is highly relevant in the general context of understanding the pathways and timescales of the ventilation of the Arabian Sea and how they impact the OMZ. However, I have several major concerns that prevent me from recommending this manuscript for publication. First, the paper is poorly written. The objectives are not stated clearly and key details of the Lagrangian experiments are missing. Moreover, the explanations given by the authors are sometimes vague or difficult to understand. More importantly, the experimental design does not seem to be appropriate to draw conclusions in a quantitative manner. Below, I develop these points with more specific comments.

II) Specific comments:

1) The study is focused on two sites at 19N (one at 62N and the other at 66.6N). It is not clear what motivates this choice or why are they supposed to represent the dynamics of the whole OMZ?

2) Another point related to the design of the experiment and the robustness of the results is the focus on one particular layer (sigma=27). Why restrict the analysis to this layer if the focus is on the entire OMZ? (especially given the fact that the World's thickest OMZ in the Arabian Sea extends vertically over a wide range of densities from 26 to 27.2)?

3) A related issue is the use of two-dimensional trajectories along one isopycnal surface, failing to take into account upwelling and diapycnal mixing, while these processes may contribute strongly to the ventilation of the OMZ. In particular, we know that the winter convection and water mixing in the north is an important source of ventilation in the northern Arabian Sea. Not being able to take this into account, appears to me to be a major weakness of the study.

4) The details of the particle release experiments are not well described. Are all 50000 particles for each site (ER vs WR) released the same day at the same lat/long point? How is this supposed to capture the spatiotemporal variability around each site?

5) From Table 1, there seem to be large differences in the ventilation sources depending on the duration of integration and the date of particle release (for instance runs 1-5 vs run 7 or run 17). This suggests that the results are not robust with respect to the time of release, and hence they may not necessarily represent the large-scale picture. I suspect the results to be affected by the mesoscale variability around the two sites, which prevents drawing any solid conclusions regarding the ventilation of the Arabian Sea at large.

6) The study focuses on the suboxic core of the OMZ (here defined as O2<10mmol/m3) and uses the World Ocean Atlas (2013) for analysis. Yet, it is known that this dataset strongly overestimates oxygen at low concentrations and hence underestimates the size of the suboxic core of the OMZ and its intensity (see Bianchi et al., 2012 and Banse et al., 2014). Empirical corrections have been proposed to minimize this problem by Bianchi et al (2012) and other studies.

7) The questions of the seasonal maintenance of the OMZ and its eastward shift have been explored by several studies in the past and several drivers have been proposed to explain these observations (e.g., Resplandy et al., 2012, McCreary et al., 2013, Acharya and Panigrahi, 2016). It is not clear what this study adds to what has been proposed before.

8) The model resolution, although in the eddy-resolving range, may not be fine enough to resolve the outflow of the RSW and PGW as these depend on the geometry of narrow straits (especially the Strait of Bab el Mandeb) that requires very high-resolution to be properly represented.

9) Authors claim that the results were insensitive to the choice of the diffusion coefficient. Yet, previous works (see for instance Gnanadesikan et al, 2012, 2013) clearly show that the volume and intensity of OMZs can be very sensitive to the choice of the mixing coefficient. Authors need to explain this.

10) The estimation of ventilation timescales is very confusing. Authors use several

sections very close to the sites of particle release (Fig 8) and focus on short timescales (1year backward and forward experiments, Fig 10 and Fig 11). How can this help to understand the dynamics of the large-scale ventilation of the whole OMZ?

11) Finally, several paragraphs and sections are vague and poorly written. For instance sections 3.2, 3.3 and 4 are not easy to understand.

III) Additional comments:

Fig 7:

The sections are not really located where they should. Not all particles in the Gulf of Aden are originating from the RS nor are all particles in the Gulf of Oman coming from the PG!!!

Fig 8:

What motivated the choice of these sections?

Fig 9:

What is shown in Fig9a and Fig9b? This is not mentioned in the caption.

Figs 10 and Fig 11:

Why restrict the trajectories to 1 year forward/backward?

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