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Interactive comment on "Seasonal variability of the circulation in the Arabian Sea at intermediate depth and its link to the Oxygen Minimum Zone" *by* Henrike Schmidt et al.

Anonymous Referee #1

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This study uses a Lagrangian approach to better understand the pathways into the Arabian Sea OMZ using HYCOM velocity fields. This is an extremely important topic and the results have the potential to be very valuable for the millions of people that depend on fish catches in the Arabian Sea as their primary form of sustenance, among others. The analysis has potential to produce a useful contribution to the current literature and has very clearly stated objectives, but I have many concerns, listed below. I cannot recommend this article for publication in its current form, but would be willing to review another version pending major revisions.

Some temporal discrepancies: The WOA13 monthly dissolved oxygen climatology cov-

ers 1955-2012, the HYCOM velocities are daily from 2000-2012, and the NCEP CFSR forcing is used from 1995-2012. Are the years in the actual analysis consistent? How can this version of HYCOM have any forcing from 1995-2000 if it starts in 2000? NCEP CFSR only exists until March 2011, at which point most systems switch their forcing to CFSv2. The authors need to make section 2.1 more clear.

A major issue I have is that HYCOM is not a biophysical model. A comparison with a biophyscial model might be more appropriate. Comparison between model currents and real dissolved oxygen measurements is not necessarily realistic. Also, the temporal sampling between currents and dissolved oxygen should be the same. I understand that there is a limited number of dissolved oxygen observations, in which case climatological currents should be used as well.

The authors validate their HYCOM velocity data with YoMaHa'07, which is based on observational Argo data. Why use HYCOM at all if the authors have observational YoMaHa currents that they can compare with the observation-based climatology of WOA13? It does not make sense to compare model currents to explain observations when observational currents are available, especially if the authors are using a Lagrangian approach. Why not use observed Lagrangian data, e.g. drifters? There is plenty of drifter data in this region (see various papers by R. Lumpkin).

YoMaHa currents in figure S1 are from 1997-2007 and HYCOM is implied to be from 2000-2012 based on the text. If they are not the same time period, please correct this.

There is no discussion of non-advective processes that influence changes in oxygen. A full budget analysis might be outside of the scope of this paper, but there is sufficient velocity data to quantify influence of upwelling on biological productivity, particularly because it drives a lot of the biophysical dynamics in this region, particularly during the summer monsoon season and should not be ignored.

Page 2: "the south eastern parts of the tropical ocean poor ventilation south of the subtropical gyre circulation (Luyten et al., 1983)" Check grammar. Also, how is the tropical ocean south of the subtropical gyre?

Page 4, line 12: “it’s” to “its”

Page 6, line 1: I’m not sure why this is a good thing. Vertical advection is an important part of the flow in this region and would be realistic.

First paragraph of page 6: There are a lot of assumptions being made and not many sources. I am not convinced that these are realistic assumptions.

Figure 1: Is this schematic applicable year-round? I imagine the circulation pathways would be very different during the summer and winter monsoon.

With all the discussion of calculating pathways and trajectories, it would be nice to have a figure explicitly showing some of this, as is promised in the beginning of section 3.2, rather than only particle position probability maps. Many of the conclusions that the authors are making are very difficult to obtain from the figures.

Page 9: Why discuss Figure 2 after Figure 4? Throughout the paper there is an unusual amount of jumping between figures, giving the impression that it is poorly organized.

Section 3.1: None of this seems new, especially the second half. Monsoon circulation has been well-studied for decades.

The entire results section is very hard to read, partly due to the writing and partly due to the lack of clarity of the figures. I recommend that the authors read some papers on studies that use drifters for reference to show clear oceanic pathways. A good example is: Drouin, K. L., & Lozier, M. S. (2019). The surface pathways of the South Atlantic: Revisiting the cold and warm water routes using observational data. Journal of Geophysical Research: Oceans, 124(10), 7082-7103.

Add references: Lachkar, Z., Lévy, M., & Smith, K. S. (2019). Strong intensification of the Arabian Sea oxygen minimum zone in response to Arabian Gulf warming. Geophysical Research Letters, 46(10), 5420-5429.

Shenoy, D. M., Suresh, I., Uskaikar, H., Kurian, S., Vidya, P. J., Shirodkar, G., ... & Naqvi, S. W. A. (2020). Variability of dissolved oxygen in the Arabian Sea Oxygen Minimum Zone and its driving mechanisms. Journal of Marine Systems, 103310.

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