Title: Causes of uncertainties in the representation of the Arabian Sea oxygen minimum zone in CMIP5 models

General Comments:

The authors present an interesting attempt to answer some of the uncertainties in the OMZ representation using the 10 CMIP5 model outputs. This is an important issue as the future prediction of OMZ shows a large inter-model spread which is a reflection of the current status of model OMZs. The models are first clustered into four bands based on vertical profiles of oxygen and then the models exhibiting most similarity with are observations are compared with other models in their water mass formation process — a representative of ventilation. The authors primarily attribute the uncertainties to a higher oxygen content in the southern ocean and the coarser model vertical resolution. However, quantitative assessment of the model discrepancies is not possible by just looking at model outputs, but I understand this is beyond the scope of the study.

The authors have communicated the scientific evidences with good clarity — outlining the scientific methods and results clearly — which is appreciable. The discussion section can be written a bit more orderly. Scientifically, there is very little discussion of the respiration part other than in the introduction section. Also, the methodology part could be a bit more explanatory to ensure transparency. Overall, the manuscript discusses a very important problem seen in nearly all the earth system models and is promising. I therefore, recommend the study alongside stating my serious concerns in detail below.

Specific Comments:

1. One cause of deoxygenation is solubility which is highly sensitive to the temperature of the oceans — warmer the surface ocean, lesser the solubility of oxygen into the ocean. The representation of ASOMZ in the CMIP5 models will also be a function of ocean temperature in the surface ocean waters. The authors discuss ventilation and respiration to some extent — which I agree are the major causes of deoxygenation. However, I feel the solubility factor might also account to some of the OMZ difference seen among the models. How would the authors justify not looking at the solubility parameter while accounting for the uncertainty in CMIP5 models.

2. Discussion**,** Ln 15-24: The authors conclude that in their study there is no definite linkage found between the model resolution and the representation of OMZ — which to me is surprising. Fundamentally, the ASOMZ is located in what we call the shadow zone where ventilation occurs through mixing processes mainly caused by mesoscale eddies [Resplandy et al., 2012, Lachkar et al., 2016]. Increasing the model horizontal resolution should result in more mesoscale eddy activity hence allowing more ventilation (due to eddy mixing) and hence changing the OMZ. An absence of a linkage between the OMZ and model resolution highlights serious issues in the parameterization of subgrid scale processes in the models or the possibility that the increased model ventilation is being balanced by an increase in model respiration. I am not sure if you can conclude that increasing the horizontal resolution has no effect on the OMZ. Please explain your disagreement to my explanation, if any, and state your reasons for making such a conclusion.

3. Statement: “There is some evidence that these model flaws are related to a deficient representation of ventilation pathways in models. On this basis, it is hardly possible to say whether the models’ biogeochemistry does have deficiencies that are associated with the oxygen representation”

Comment: OMZ is shaped majorly by respiration and ventilation. The study highlights the difference in model mixing of different water masses in the OMZ. However, arising from the fact that OMZ are located in the world’s major upwelling zones — which depicts the importance of respiration in shaping OMZ — it is very possible that the model’s biogeochemical component is highly (if not equally) responsible for the OMZ volume simulated in the models. Previous studies have addressed the weak representation of biophysical processes in the model, which lays a strong possibility of the deficiencies in the biogeochemical component thus, shaping the model’s OMZ. Most of the burden here is placed on the physical parameterizations, whereas the biogeochemical part is a little under looked. Please justify.

4. If you notice the core region of ASOMZ, then you would find almost all the models largely overestimating primary production in the region. This to me, suggests that large respiration should be occurring in the models. I suggest to check whether respiration is well represented in the models. This will confirm that the problem lies largely with the physical processes or biological processes.

5. What clustering method is performed to identify the clusters. Please add some details about the clustering technique in the methods section of the manuscript.

6. There are 10 CMIP5 models used in the study. However, there are ~15-16 ESM models which participated in CMIP5. Please state your choice to choose these 10 models and leave the others.

7. What is the reason to choose the oxygen threshold value of 60 mircomol/litre?

8. Use of T-S diagrams to resolve the water mass characteristics are not quite effective near the shelves. In such a case, how reliable are the estimates taken for the RSW and PGW water masses. Can this be the reason for the models showing large deficiencies in the RSW and PGW masses. What is the author’s opinion?

9. Figure 4a: The authors have shown a vertical line of 50 micromol/litre. Why is this value pointed out when the threshold for hypoxia is considered as 60 in the rest of the study.

10. It is advised to shorten the discussion section slightly. Nonetheless, it can be organized a bit more clearly.

11. Summary, Ln 14-15: The authors suggest improved parameterizations of Persian gulf and Red sea water masses in the models. However, I am not confident if improvement in the parameterization of these water mass overflows into the OMZ region would significantly improve the OMZ. Instead, a more local process improvement would be inclusion of eddies into the parameterizations which would affect the ventilation. Please provide strong evidence supporting your solution put forward to the problem under question.

12. The authors discuss a very important problem using the available model outputs. However, using the model outputs it is very difficult to quantitatively separate the discrepancies in the physical and biogeochemical processes. It would be interesting to have a quantitative estimation of the model discrepancies in between the individual processes using some model experiments. I understand this is beyond the scope of this paper, but this can be mentioned as a future scope of the work undertaken.

13. The authors start the discussion section mentioning that all the models underestimate ASOMZ [as seen in Fig4a]. However, if we look at the vertical profile of oxygen in core region of OMZ [fig 5] we see that almost all the models have higher concentrations of oxygen. Is this possible that the models are overestimating oxygen in core region of OMZ? Please clarify.

14. It is advised to include how the water masses are calculated in the methods section.

15. Technical Corrections:

1. Summary, Ln 10: "overestimate oxygen concentrations…." —> I think it should be underestimate.
2. Introduction, Ln 16-17: Provide references.
3. Discussion, Ln 16: "Recent studies analyzing…." —> Provide the references.
4. It is advised to rephrase a few sentences in the discussion section as they are confusing.
5. Summary: Rephrase the first two sentences