Review by Jay McCreary of manuscript no. bg-2012-138, entitled "Controlling factors of the OMZ in the Arabian Sea," by Laure Resplandy and coauthors

SUMMARY: This paper discusses the biodynamics of two aspects of the Arabian Sea oxygen minimum zone (OMZ): its seasonal cycle, and its eastward shift away from the highly productive region in the western Arabian Sea. Despite the length of my review, I like this paper a lot, as it contains new and useful information about Arabian Sea OMZ dynamics. I am sure that I will find a revised version of the manuscript that takes into account the following comments acceptable for publication in Biogeosciences.

Many of the comments are editorial in nature, and are intended to help the readability of the text. The authors can accept or reject them at their discretion. Some comments express scientific concerns, and I would like to know how the authors respond to these issues, either by modifying their text or by rebuttal. They fall into three categories. 1) The length of the two runs (10 and 33 years) are not long enough for solutions to reach equilibrium (*e.g.*, Comment 12). I think it is important for the authors to justify this aspect of their experimental design more carefully. 2) The Arabian Sea OMZ extends from 200–1000 m, and there is confusion about just what depth range the authors are discussing at various parts of the text (*e.g.*, Comments 15 and 38). For example, in some cases the discussion seems to concern the depth of the near-surface oxycline rather than the OMZ "core." 3) Some of the statements about the impact of physical dynamics seem incorrect or are unsubstantiated (*e.g.*, Comments 16, 25, 29, 34, and 40–43).

Page numbers and line numbers refer to the version of the manuscript that I downloaded from the Biogeosciences website.

1) page 5510, line 8: In the abstract you wrote: "We find that the oxygen concentration in the OMZ displays a seasonal cycle with an amplitude of 5–15% of the annual mean oxygen concentration." I think you should state here that the 15% occurs at the very top of the OMZ, when the oxycline is shifted up and down by ocean dynamics. The 5% occurs everywhere else in the OMZ. Right? That is, it is not the OMZ itself that varies by 15%, but only the depth of the oxycline.

Furthermore, is it really correct to say that the OMZ is "ventilated"? To me that process means that oxygen is injected into the OMZ and therefore increases oxygen throughout its vertical extent. In fact, there is no (or little) ventilation at all, with only the oxycline shifting up and down.

2) page 5510, lines 23–24: I don't think you ever showed in the manuscript that low oxygen waters from the western AS actually advect eastward to cause the ASOMZ there. If not, then this last sentence should be replaced.

3) page 5513, lines 1–15: I found this paragraph a bit confusing. The opening sentence is the first place where you note the eastward shift. I expected some further discussion of its particular aspects. The remainder of the paragraph, however, is mostly a discussion of denitrification, which seems off the main topic. You might try to expand the first sentence into a small paragraph. Then, have the discussion of denitrification in a separate one.

4) page 5514, lines 20–21: What about horizontal mixing? This process should be very important in the dynamics of the deeper OMZ. Is it the same as in the biological model?

5) page 5515: Laure, equations should be viewed as part of sentences, and so punctuated accordingly. So, in the above equations, I added a period at the end of them.

6) page 5515: I thought that $f(O_2)$ was long and complicated enough to be displayed in its own line. I am still not sure, though, about whether it is written correctly. For example, I am not sure what "$0, 6 - O_2$" means. I guess it means that $\max(0, 6 - O_2) = 0$ if $O_2 > 6$ and is zero otherwise. Right? Please check.

7) page 5515: From our own work on this topic, I know that the detrital sinking (w_s) and remineralization (e) rates are very important for setting the depth scale of the OMZ, $\delta z = w_s/e$. So, at this point in the text, I wanted (expected) you to tell me what those rates are. Should you do that? Maybe not, as most of your readers are likely not aware of the OMZ sensitivity to δz.

8) page 5515: How does the model differentiate between new and regenerated production? Should you tell your readers here? Or is the biomodeling community generally aware of this point?

9) page 5516: I replaced "were damped" with "are relaxed to WOA05 values" in the above. Okay?

10) page 5516: Does the Kone et al. (2009) simulation develop an eastward shift? If so, to what degree was the shift initiated simply by the initial conditions of that run, which are oxygen from WOA05.

11) page 5516, description of mixing: What about vertical mixing in the biological model? This process should be very important in the dynamics of the deeper OMZ. Is it the same as for the physical model?

12) pages 5516–17: My key concerns about this paper stems from the length of integration of the two experiments: main run for only 10 years, and perturbation for only 33 years. Is a 10-year integration long enough for the model to adjust at all away from its initial conditions? Certainly not at depth. I expect the only changes that can occur are in the upper ocean, in the precise nature of the oxycline variability and perhaps somewhat below that depth. Regarding the perturbation run, is it problematic that oxygen only decreases at depth by 20 μmolO_2/kg. There is no indication in the oxygen field itself that an eastward shift will ever develop. It may be true that "the experiment is useful for illustrating the mechanisms controlling the OMZ formation," but it can't really say anything about the biodynamics of the eastward shift.

I think you need to defend your experimental design in more detail here. I note that you state in a comment that it would take 500 years for the perturbation run to reach equilibrium (longer than in our model). That is likely true for your main run as well. So, how does this lack of equilibrium affect your conclusions? Just what can you actually conclude with certainty?

13) page 5517: What do you mean by "surface layer"? Is that the top-most level of the OGCM? Or is it a surface mixed layer? Please explain.

14) page 5518: In the above you refer to the "western coast." Do you mean the west coast of India or the west coast of the Arabian Sea. Please clarify. This potential confusion occurs elsewhere as well.

15) page 5519: I was confused by your used of the term “core” here and elsewhere. For me, the core of the ASOMZ lies at depth, in a depth range centered about the depth where oxygen attains its minimum value. I am not sure how you define that term, but it often seems like you use the term to refer to a region just beneath the oxycline. I think you need to define up-front the various depth ranges in the OMZ that you refer to. Perhaps useful labels are “oxycline,” “upper OMZ,” and “lower OMZ.”

16) page 5519, lines 12–14: Coastal upwelling extends only to about 150–200 m. So, this process cannot impact the “core” of the OMZ. (But that depends on just what depth range you mean by “core.”) Fix the text.

17) page 5519, lines 17–18: What feature are you referring to here? The slight shift of the isoline? Please tell your readers.

18) page 5519, lines 24–26: What feature are you referring to here? The observed and modelled distributions have a very different structure. So, it is hard to know in what way the model is more intense.

19) page 5520: In the above, I replaced “dynamical transport” with “oxygen advection” and “advection.” That seemed clearer to me. Okay?

20) page 5521, line 3: In the first sentence of the paragraph, you refer to the “core” of the OMZ. Not sure what you mean here, but it appears to refer to the oxygen levels in the vicinity of the oxycline. To me, that is not the core.

21) page 5521, lines 14–16: I thought the Red Sea was closed. So, how can advection from the Red Sea impact anything. Also how can *southeastward* advection from the Red Sea impact the CAS region, which lies north of the Gulf of Aden? In either case, from what you have presented in this paper how have you demonstrated that horizontal advection “ventilates” (if that is actually the correct word, see above comment) the offshore regions?

22) page 5521, Figure 6: What you mean by the “2” in Figure 6(2) is not defined. That nomenclature needs to be defined somewhere. Likewise for 1 and 3.

23) page 5521, line 21: Again “core” seems actually to refer to the very top of the OMZ, in the depth range of the oxycline itself.

24) page 5521, lines 28–29: Are there any westward currents that actually carry low oxygen values from offshore to the coast during the NEM? I don’t know of any? Again, just what level does “core” refer to.

25) page 5522, lines 1–3: Here and elsewhere, it is important to differentiate between horizontal and vertical advection. In this instance, the oxygen tendencies in Figure 6 are surely due to *vertical* advection below 400 m, and only the near-surface changes result from horizontal advection. By the way, the deep vertical advection cannot be due to coastal upwelling (which is shallow), but rather to Rossby-wave propagation.

26) page 5522, lines 13–17, coastal undercurrent: What feature are you referring to here. It is hard for me to see a clear indication of a Coastal Undercurrent.

27) page 5522, line 20: As in the previous comment, it is not clear what you are referring to here by surface confined and subsurface currents. I made a small change here, but can you clarify what you mean more.

28) page 5522, Figure 6: the 1000 m marks on the y-axis in panel c extend into the panel b. Panel b is mislabelled panel c.

29) page 5523, lines 15–19; discussion of eddy terms: What have you presented in the paper that allows you to conclude that eddy-driven ventilation is mostly associated with the *vertical* advection of surface ventilated waters during the SIM and SWM and with *horizontal* advection during the FIM and NEM period? None of your figures or discussion seems to allow this conclusion. At the OMZ base is anything really advected *into* the OMZ itself? I doubt it. It is just the bottom of the OMZ shifting up and down slightly seasonally.

30) pages 5523–24: Is your general statement about OMZs in fact true? Certainly, oxygen attains a minimum value in the water column in many regions other than just beneath areas of maximum production. A key factor in the distribution of all OMZs is that they exist in a region of weak subsurface currents, which is therefore poorly ventilated. The extent of that poorly ventilated region determines the areal extend of the OMZ. Probably what you wrote is okay, but it may not be precise.

31) page 5524: The reference here (and elsewhere?) to Figure 5, should be Figure 8.

32) page 5524, lines 5–6: I disagree with this sentence. In our paper on the ASOMZ, we discuss the dynamics of the eastward shift completely in terms of equilibrium solutions, by obtaining a large suite of test solutions. It is likely true, though, that the processes that account for the eastward shift cannot be deduced only from an analysis of (2). So, perhaps modify the first sentence to state your meaning more clearly.

33) page 5524, lines 11–14: It is hard to tell in Figure 8 just how close balance $\overline{(\partial O2/\partial t)_{\mathrm{Bio}}} = -\overline{(\partial O2/\partial t)_{\mathrm{Dyn}}}$ is attained. Visually, the two fields do not seem to be close at all. It would be good to plot the difference of the two fields as well. You should argue that the difference map is "small" in some way with respect to the individual fields. Is that the case? Is there in fact a significant model drift? If so, the model is still not near equilibrium, which is a problem for drawing solid conclusions about the biodynamics of the seasonal cycle.

34) page 5524, lines 15–16: It really is difficult for me to understand just what the perturbation experiment allows you to conclude. In particular, can the tendencies revealed by the experiment explain anything about the causes of the eastward shift? Perhaps, but it really is a stretch, since your final state is so far from equilibrium. You really need a more careful discussion of this point than you present in the current text. Just what can you, and can you *not*, conclude from this experiment?

35) page 5525, Figure 10: I did not find Figure 10 to be useful at all. It just demonstrates that your solution isn't near equilibrium, which we already knew. I recommend that you delete it.

36) page 5525, lines 5–7: The perturbation run does not produce an eastward shift at all. So, just how is the oxygen distribution in the solution spatially consistent with that in the observations?

37) page 5526, lines 11–13: The claim in the last sentence is really just a hope. There is no solid reason to expect it to be correct. You need to rephrase this unsubstantiated claim.

38) page 5526, lines 22–25: Not sure if "upper part" is the correct terminology. The 15% change occurs where the oxycline is advected vertically, so occurs only at the top-most part of the OMZ. A more useful definition of "upper" OMZ might be the depth range from 200–500 m, which lies below the oxycline. The "deep" OMZ is the part that extends to 800–1000 m.

39) page 5527, lines 3–6: Your meaning is unclear. The "compensation" of oxygen variability between the upper (not near-surface) and deep OMZs must happen because of vertical advection (a consequence of the actual minimum of oxygen occurring near 500–600 m). This signal is likely due to a first-baroclinic Rossby wave. The near-surface signal is due to the swift monsoon currents and it *must* average out seasonally.

40) page 5527, lines 11–29; page 5528, lines 1–2: There are a number of imprecise or unsubstantiated statements in this paragraph. (line 14) How did you demonstrate that "low oxygen waters [in the central Arabian Sea] were sustained by their transport offshore [from the Somali and Omani coasts0"? I can't find anything in the paper that shows this property. Delete or clarify. (lines 16–17) Since there is no connection to the Red Sea, how can this conclusion be valid? (lines 18 and 19) Coastal downwelling can extend at most to 200–300 meters. I don't think it reaches deep enough to impact the "core" of the OMZ. A similar problem exists for coastal upwelling during the SWM. There are deep signals in your model: They must indicate the presence of first-baroclinic-mode (low-order-mode) Rossby waves generated by offshore Ekman pumping. (lines 26-29) The lower OMZ (the meaning of "lower OMZ" should be defined somewhere) cannot be influenced by coastal upwelling or downwelling, but it can be affected by offshore Ekman pumping. (page 5529, lines 1–2) Not sure about just what is the Coastal Undercurrent in your figure. Such a feature is actually hard to define because of the offshore propagation of Rossby waves, which occurs so efficiently away from the west coast of Indian, particularly the southwest coast where the Rossby radius is large.

41) page 5528, lines 17–25; lines 25–29: There is nothing in this paper to support either of these conclusions. Please clarify or delete them.

42) page 5529, first paragraph: Nothing in this paper to support these conclusions either. Maybe that is okay since this is a discussion section. On the other hand, in the main text you concluded that eddies were not really important. So, it seems odd that so much discussion is devoted to that topic here. Should some of this text be moved to the subsection where you discuss eddies?

43) page 5529, line 25, – page 5530, line 2: There is nothing in the paper that supports the conclusions in this text. In particular, how is it shown that low oxygen values in the interior of the Arabian Sea are influenced by low oxygen from the western basin. Furthermore, if that is actually case, at which depth range does it occur. It is not likely to occur at great depth since currents are weak there.

44) page 5530, lines 14–27; discussion of McCreary et al. (2011): In the first sentence, I thought you concluded that mesoscale eddies had little effect on the OMZ in your solutions, which would seem to contradict the McCreary et al. (2011) results. I am also not sure that the last sentence is correct, as different sinking or remineralization rates have a large impact

on our OMZ. If, however, w_s and e are changed by the same factor, such that their ratio δz is unchanged, then the impact on the OMZ is weaker.

45) page 5531, section 6: Is this section needed? Didn't you just state all of your conclusions in Section 5? I would combine and shorten these two sections. (Or maybe the journal asked you to write Section 6?)