[Reviewer's comments are inserted in regular font and responses are highlighted in blue.]

## Responses to Reviewer #2

GENERAL COMMENTS

**Comment 1 (C1):** The manuscript analyses the output of a coupled physical-biogeochemical model with regard to oxygen distribution and variability at the Louisiana shelf. The strength of the study is the detailed validation of model results against different observations that nicely show the realism of the presented simulations. Main findings of the study are the important role of strong stratification shielding bottom layers from ventilation from above (associated with O2 outgassing from the surface layer), a minor role of primary production below the pycnocline for the development of hypoxia on the shelf and that the hypoxia is determined by a combination of physical processes and sediment oxygen consumption. The last point, however, is in my mind somehow vague and it is not clear to me, how much it depends on the chosen parameterizations.

**Response (R):** We appreciate the positive comments. Regarding how much our conclusion depends on the chosen parameterizations, we have carefully addressed the comments on sediment oxygen consumption and air-sea gas exchange parameterizations as detailed below and hope that the conclusions are better justified as a result.

**C2:** My main concern regards the parameterization of the sediment oxygen consumption (SOC) and the evaluation of the oxygen budget (Eq. 4-6). The used parameterization of SOC depends only on oxygen and temperature. This dependence clearly does not explain very well the observed variability of SOC (Fig. 7). As physical processes, particularly vertical diffusion, are the main oxygen supply to the bottom layer, the SOC parameterized by oxygen concentration depend on the strength of this oxygen supply. This could be an oversimplification of the problem. What could be the role of spatially varying available particulate organic matter on the shelf? Could this be accounted for or why we should not care about it? And is it correct to have a SOC parameterization that depends on oxygen concentration also for relatively high oxygen levels.

**R:** There are three points in this comment that we respond to separately:

1) Regarding "the SOC parameterization does not explain well the observed variability in SOC (Fig. 7)"

This is true and may partly be due to the different measurement techniques used (i.e. limitations in the available data) and partly because instantaneous SOC depends on many other factors (incl. for example sediment porosity and faunal activity), but there is no simple mechanistic formulation explaining these dependencies. The SOC parameterization we used, while simple, does include two parameters known to be key in modulating SOC: temperature, which is well-known to modulate the rate of microbial respiration in sediments, and dissolved oxygen concentration in the overlying water, which regulates the depth of $O_2$ diffusion into the sediments, and hence, the sediment $O_2$ flux into the sediments (Cai and Sayles 1996). The SOC parameterization has been used in previous modeling studies in this region and shown to accurately simulate hypoxia relative to observations. The discrepancies between the parameterization and the observed rates are a subject of our Discussion and will, without a doubt, be the subject of future research.

2) Regarding "What could be the role of spatially varying particulate organic matter [flux]? Could this be accounted for or why should we not care about it?"

We have a model configuration where SOC was determined by the amount of organic matter sinking to the sediment (this was used in several of our previous publications) and we have actually performed all analyses presented in this manuscript for these simulations as well (and they were part of the very first draft of this manuscript). It turned out, however, that our conclusions about the summer oxygen balance are essentially identical when using the spatially and temporally varying organic matter flux to the sediment rather than the SOC parameterization. We decided it would be too tedious and redundant to report results for both cases and since the model's skill in simulating hypoxia is better when using the latter (see e.g. Fennel et al. 2013) we chose to show those. We state this now in section 4.1 on Page 19.

3) Regarding "is it correct to have a SOC parameterization that does depend on SOC for relatively high oxygen levels?"

SOC is dependent on oxygen concentration only for low oxygen levels. We added the following text in the description of our SOC parameterization (Page 10):

"SOC linearly increases with increasing bottom water *DO* for concentrations lower than 50 mmol $O_2$ $m^{-3}$ and saturates when concentrations are higher than 100 mmol $O_2$ $m^{-3}$." This characteristic is shown in Figure 7 where the simulated SOC increases with increasing bottom DO for oxygen concentrations below ~50 mmol $O_2$ $m^{-3}$. The reason the SOC declines above ~80 mmol $O_2$ $m^{-3}$ in Fig. 7 is its temperature dependence (high oxygen concentrations occur in winter when water is colder).

**C3:** The oxygen budget for the three layers described by equation 4 to 6 is not easy to understand and to follow. I would suggest writing down more complete equations. My concerns are the following. If you integrate the time derivative of oxygen over volume and time (first term of equation 4-6, respectively), you obtain the amount of mol O2 within the volume of integration. My understanding from your description is that this volume is not constant for the upper and middle layer. Thus volume changes have to be incorporated into the equations. This also includes the question, what happens if during part of the time only two layers exist and the budget is not evaluated (P14900, L10)? This would change the mol O2 in the regional budgets.

**R:** We replaced all occurrences of 'oxygen budget' with 'oxygen balance', because we feel the latter is a more accurate description of our calculations for the reasons mentioned by the reviewer. We have also removed equations 4-6 that caused confusion; instead, we added the equation of DO conservation in section 2 ('Model description') and clarified how we calculated the summer oxygen balance in section 3.3:

(Page 9) "The equation for the *DO* conservation is given by

$$\frac{\partial DO}{\partial t} = -\left(u\frac{\partial DO}{\partial x} + v\frac{\partial DO}{\partial y} + w\frac{\partial DO}{\partial z}\right) + \frac{\partial}{\partial z}\left(K_v\frac{\partial DO}{\partial z}\right) + PP + WR + F_{bf} \qquad (1)$$

where $x$ and $y$ represent the two horizontal directions and $z$ the vertical direction, $u$, $v$, and $w$ (m $s^{-1}$) represent velocities in $x$-, $y$-, and $z$-directions, respectively, and $K_v$ is the vertical diffusivity (m $s^{-2}$). On the right-hand side of the Eq. (1), the first term represents horizontal and vertical advection of DO, and the second term is the vertical diffusion of DO (horizontal diffusivity $K_H$ is set 0 in the model and hence we neglected horizontal diffusion terms in equation). Here the advection and diffusion terms are computed using the advanced numerical schemes build into the ROMS hydrodynamic model. The term PP is the primary production and WR represents the sum of water column respiration and nitrification. Although not strictly accurate, the use of the terminology WR is consistent with the use of WR in the observational literature where measurements of water column oxygen consumption include the contribution of nitrification. The term $F_{bf}$ represents the boundary oxygen fluxes, namely the air-sea flux of oxygen at the top layer and the sediment oxygen consumption at the bottom layer, the parameterizations of which are detailed below."

(Page 15) "We defined the pycnocline as the depth of maximum Brunt-Vaisala Frequency (Pond and Pickard, 1983) and restricted our analysis to horizontal grid cells where all three layers existed (i.e. a main pycnocline was present and was more than 5 m above the bottom). We then integrated the terms in Eq. (1) vertically over each layer at each desired grid cell on each desired day. The advection and diffusion terms were evaluated as divergences, namely fluxes of DO into or out of the given volume through advection or diffusion. Finally we averaged the integrated results over all grid cells within a selected sub-region and over June to August in order to obtain the summer oxygen balance for the sub-regions."

**C4:** Moreover, it is not clear how the advection and diffusion term in the oxygen budget are evaluated. The calculation of the supply due to horizontal advection (which is part of the advection terms, P14901, L6-7) is not correct, as it is written. The integration must be over vertical planes, e.g. u times O2 integrated over y and z and v times O2 integrated over x and z. Moreover, it is not clear to me if these integrations are done for each grid cell or for the whole region. Thus it would be really helpful to see more detailed equations that also would better explain the other terms. How is the vertical diffusion parameterized? Do you use a horizontal diffusivity in the tracer equation? Why it is not included in the budget? Are you able to close the budget with your calculations? What is the error in budget calculations?

**R:** In the ROMS model, advection and diffusion diagnostics were calculated as divergences, namely fluxes of DO into or out of a given volume (i.e. grid cell), and not as fluxes through vertical planes. (This is why vertical and horizontal advection in Figure 9 cannot be separated.) The ROMS diagnostics were calculated in this way because it is practically impossible to close tracer budgets if fluxes are approximated as fluxes across horizontal and vertical planes. However, when using divergences, tracer budgets close perfectly.

We have added the equation of DO conservation in section 2 ('Model description'):

$$\frac{\partial DO}{\partial t} = -\left(u\frac{\partial DO}{\partial x} + v\frac{\partial DO}{\partial y} + w\frac{\partial DO}{\partial z}\right) + \frac{\partial}{\partial z}\left(K_v\frac{\partial DO}{\partial z}\right) + PP + WR + F_{bf} \qquad (1)$$

To calculate oxygen balance, we integrated the terms in Eq. (1) vertically over each layer at each desired grid cell on each desired day. We then averaged the oxygen balance over the grid cells within a selected sub-region and over June to August to obtain the summer oxygen balance. Please see the response to comment 4 for more details on the calculations.

The vertical mixing is parameterized using the Mellor and Yamada (1982) turbulent closure scheme.

Horizontal diffusivity is set as 0 in the model and hence horizontal diffusion term is not included in the oxygen balance.

We have also added more details in Section 2 Model description (Page 7): "The model uses a fourth-order horizontal advection scheme for tracers and a third-order upwind scheme for the advection term in the momentum equation. Vertical gradients are calculated with conservative parabolic splines, and vertical mixing is parameterized using the Mellor and Yamada (1982) turbulent closure scheme."

**C5:** In general, it would be helpful to see some discussion on the sensitivity of the model results to different parameterizations, e.g. the air-sea gas parameterizations. What is the uncertainty of the model results due to the use of these specific parameterizations? Other specific points are listed below. In summary, I think that this paper could be good contribution to a better understanding of low oxygen regions and particularly to the hypoxia of Louisiana after clarification of the addressed general remarks above and specific points below.

**R:** We have added a discussion of the effect of the air-sea gas exchange parameterization in the revision (Page 20):

"We have carried out sensitivity experiments where we doubled and halved the air-sea gas exchange coefficient (results not presented in the manuscript) and found that the model results are insensitive to the air-sea gas exchange rates, likely because the air-sea oxygen flux is fast."

SPECIFIC POINTS

**C6:** P14890, L14: "autotrophic/heterotrophic water" sounds not ok, please reformulate

**R:** We modified this sentence (Page 2) in the abstract by removing the second part that contained autotrophic and heterotrophic. It now reads: "During this time, efflux of oxygen to the atmosphere, driven by photosynthesis and surface warming, becomes a significant oxygen sink."

**C7:** P14891, L29: Do you have explicit horizontal (isopycnal) diffusion in the tracer equation? How is it parameterized? It would be helpful to see the equations that are evaluated for the budget. See also my points above.

**R:** No, the horizontal diffusion coefficient is zero in the model. Also see the response to comment 4.

**C8:** P14894, L12: You use horizontal uniform T/S boundary conditions, which does not allow baroclinic inflow. What are your conditions for flow into and out of the model domain? How do you account for larger scale advection?

**R:** We added more details on how the physical horizontal boundaries are treated:

(Page 7) "An average profile of temperature and salinity, based on historical hydrographic data (Boyer et al., 2006) and assumed to be horizontally uniform, is used as physical boundary condition. At the three open boundaries, gradient conditions are used for the free surface, radiation conditions for the three-dimensional velocities, and a Flather (1976) condition with no mean barotropic background flow for the two-dimensional velocities."

(Page 7) "The model uses a fourth-order horizontal advection scheme for tracers and a third-order upwind scheme for the advection term in the momentum equation."

**C9:** P14895, L25: Wanninkhof (1992) proposed a parameterization with relatively large air-sea gas exchange. Particularly in high productive upwelling regions, the air-sea gas exchange might be limited by surface films (Tsai and Liu, 2003). Probably the effect on hypoxia would be very minor, when using different parameterizations. Please comment on that.

**R:** We are confident that the effect is negligible. Please see the response to comment 5.

**C10:** P14896, L7: Dependence of SOC on oxygen concentration is typically assumed for much lower oxygen levels only (e.g. Canfield 1993, 1994). Why is there no dependence on organic matter load?

**R:** Please see the response to comment 2.

**C11:** P14901, Eq. 6: Terms for horizontal and vertical advections has to be separated and integrated over different planes.

**R:** Advection terms are calculated from divergences, hence can not be separated into horizontal and vertical. Please see the response to comment 4.

**C12:** P14972, Figure 10: Please explain the term "Net".

**R:** We have added an explanation in the caption of this Figure (Page 47, now Figure 9): "The net rate of oxygen change in each layer (i.e. the sum of all oxygen source and sink terms) is given and denoted as Net."

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Fennel, K., Hu, J., Laurent, A., Marta-Almeida, M., and Hetland, R.D.: Sensitivity of hypoxia predictions for the Northern Gulf of Mexico to sediment oxygen consumption and model nesting, J. Geophys. Res., 118, 990-1002, 2013.

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