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Interactive comment on "Numerical analysis of the primary processes controlling oxygen dynamics on the Louisiana Shelf" *by* L. Yu et al.

Anonymous Referee #2

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General comments

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.

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Interactive Discussion
Discussion Paper

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.

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. 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

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included in the budget? Are you able to close the budget with your calculations? What is the error in budget calculations?

In general, it would be helpful the 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.

Specific points P14890, L14: "autotrophic/heterotrophic water" sounds not ok, please reformulate 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. 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? 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. 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? P14901, Eq. 6: Terms for horizontal and vertical advections has to be separated and integrated over different planes. P14972, Figure 10: Please explain the term "Net".

Canfield, D. E.: Organic matter oxidation in marine sediments, in: Interactions of C, N, P and S Biogeochemical Cycles and Global Change, edited by: Wollast, R., Mackenzie, F. T., and Chou, L., NATO ASI Ser. I, 4, 333–364, Springer, Berlin, 1993. Canfield, D. E.: Factors influencing organic carbon preservation in marine sediments, Chem. Geol.,

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