

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

Anonymous Referee #1

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GENERAL COMMENTS

This manuscript investigates the mechanism controlling the development of hypoxia in the northern Gulf of Mexico with a modelling approach. The topic fits perfectly with the scope of Biogeosciences. The main question, i.e. to quantify the respective importance of the various oxygen sinks and sources in mitigating/enhancing hypoxia in the northern Gulf of Mexico, is well stated and modelling experiments well constructed to answer the question. The fact that stratification and sediment oxygen consumption are the main driver of hypoxia in this area has already been suggested but this manuscript specifically tests and confirms this hypothesis (by comparing hypoxic area obtained with and without considering water column biological processes affecting oxygen). The most problematic issue is the strong emphasis on benthic oxygen consumption in the

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discussion, and the large approximations in its representation in the model. Once the impact of the latter on the conclusions are discussed accordingly, this manuscript would constitute a valuable publication and contribution to the understanding of hypoxia in the northern gulf of Mexico

In general the manuscript is slightly redundant. There are many figures and all the information contained in the figure is not always exploited in the discussion. Either some figures could be removed, either these should be better integrated in the discussion.

SPECIFIC COMMENTS

[1] The main weakness of the modelling set-up lies in the empirical relationship used to estimate the sediment oxygen consumption (SOC). SOC is expressed as a direct function of bottom oxygen consumption, calibrated empirically on the basis of a set of in-situ benthic chambers measurements. While it is recognized (P14898 L15-21) that others SOC data sets indicate lower values than those obtained in the simulation (ie. Lehrter et al. 2012, Murrell and Lehrter, 2011) the manuscript states that "Observations from Rowe et al. (2002) and McCarthy et al. (2013) mostly fall within the range of the variability of simulated SOC". This has to be described more accurately since on Figure 7 only 4 points out of a total of 12 (Rowe et al. (2002)) lie between the depicted range of 25th-75th model percentile, and 5/18 for McCarthy et al. (2013).

[2] More generally, there is a paradox in that the validation procedure indicates simultaneously (1) an overestimation of bottom oxygen concentration and (2) an overestimation in sediment oxygen consumption (SOC). Moreover if we take into account the manuscript's main conclusion which is that oxygen dynamics in the bottom layer is driven by sediment oxygen consumption. The direct (empirical) dependence of SOC on DO makes it difficult to interpret this behavior. The authors justify this (P14903,L22-P14904,L5) by suggesting that measured SOC could underestimate the true sediment oxygen demand, ie. that the accumulation of reduced metabolite resulting from benthic respiration could lead to further oxygen consumption not accounted for by SOC

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measurement. In order to be so, the oxidation of these metabolites should occur in the water column, which suppose those are released to the water column, which suppose quasi-anoxic bottom conditions, but Fig 7. indicates overestimated SOC over a large DO range. Could some physical aspects explain this apparent paradox ? Can the accuracy of vertical diffusion at the bottom pycnocline and/or horizontal advection be checked independently, i.e. on the basis of physical aspects (probably this has been done already, and a referenced discussion will do). In general, because this is central to the main conclusion these aspects have to be discussed more completely.

[3] I wonder why nitrification is listed in the oxygen sinks (P14895,L13) but is not considered in the budget (Section 3.3). Nitrification of ammonium originated from the sediments could be a significant oxygen sinks in bottom waters not accounted for by SOC measurements. If nitrification happens to be a significant term in the budget and if in-situ estimates are available, a validation would greatly complete the present picture. For instance Lehrter et al. 2012 mention that "Realistic models of sediment O2 dynamics for this shelf will need to include the accumulation of oxygen debt from reduced nitrogen, iron, manganese, and sulfur." In the present manuscript the list of "reduced metabolite" given P14903 excludes ammonium.

[4] In general the effect of temperature on Oxygen saturation concentration should be acknowledged when discussing air-sea oxygen fluxes and community respiration/production (e.g. P 14900, L 1-3 ; P14901, L 15 ; P14905 L 4). For instance, which part of the oxygen flux to the atmosphere is due to the autotrophic condition of surface water, and which part is due to the fact that warming surface waters become naturally oversaturated in oxygen, as oxygen solubility decreases and exchanges rates at the surface are kinetically limited.

[Table 3] The SOC bias is estimated by comparing model values to observations according to the DO ranges. This approach is strongly dependant on the assumption of a close relationship between SOC and DO, an assumption that is questioned by the large dispersion of in-situ measurement depicted in Fig 7. Wouldn't it be better to com-

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pare model and in-situ SOC values according to the spatial distribution (e.g. using the four areas used in the present manuscript or the zones of similarity from Lehrter et al. 2012) ? This could eventually lead to a discussion on the adequacy of using such an relationship over the important environmental gradient covered by the model domain. The validation procedure has to establish that the model approximation does not jeopardize the conclusions presented on the basis of the sensitivity experiment (i.e. with an without water column terms).

[Fig. 5] Fig. 5 is not really exploited in the discussion. Why is this figure essential ?

[Fig 7.] SOC is a function DO, modulated by temperature. As the same relationships is used in the two simulations (Model and MODEL + CCR), how comes that they depict different curves ? Is that due to a different DO/Temperature distribution ? Please clarify.

TECHNICAL COMMENTS

[P14893 , L11] Boyer et al , 2005 or 2006 ?

[P14894 L 10] "Climatological boundary conditions were initialized using an average profile of temperature and salinity based on historical hydrographic data (Boyer et al., 2005) and assumed to be horizontally uniform" : It is not clear with this sentence whether physical boundary conditions vary seasonally.

[P14899 L 18] Please provide the exact time frame of integration.

[P14905 L 29] The reference Kemp et al. 1992 does not appear in the bibliography.

[FIG 6.] Split the y-label: PP for the upper part ; Water community respiration for the lower part.

[Fig. 9] : Should be introduced in section 3.1

[References] Refs Dagg et al., 2004 ; Green et al., 2006 ; Trefry et al., 1994 appears in the bibliography but not in the text

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