

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

Responses to Reviewer #1

GENERAL COMMENTS

Comment 1 (C1): This manuscript investigates the mechanism controlling the development of hypoxia in the northern Gulf of Mexico with a modelling approach. The topics fit 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 discussion, and the large approximations in its representation in the model. Once the impact of the latter on the conclusions is discussed accordingly, this manuscript would constitute a valuable publication and contribution to the understanding of hypoxia in the northern Gulf of Mexico.

Response (R): We thank the reviewer for the encouraging comments. The emphasis on benthic oxygen consumption in the discussion is due to its critical importance as an oxygen sink in driving hypoxia on the LA shelf. We carefully addressed all specific comments, detailed below, which helped strengthen and improve the discussion.

C2: 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.

R: We removed Figure 9, which was not fully exploited in the discussion, and further edited the manuscript to avoid redundancy.

SPECIFIC COMMENTS

C3: [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 other SOC data sets indicate lower value 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 over 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).

R: The sentence in question was removed and instead we now say (Page 13):

“Simulated SOC is at the upper range of the available observations.”

C4: [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 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.

R: We agree that the simultaneous overestimation of bottom oxygen concentration and SOC would be a paradox if SOC was the only process determining bottom oxygen dynamics and if our SOC parameterization was perfect. But, as indicated in the second half of the reviewer's comment, of course other factors matter. A physical explanation would be that the bottom boundary layer in the model is too thick, which means that higher SOC rates are required to draw down bottom oxygen, than in reality. We added the following on page 19:

“Another explanation could be that the thickness of the simulated bottom boundary layer is overestimated. If this is the case, SOC would have to be larger than in reality in order to produce hypoxic bottom water. Future work on validating the expression and dynamics of the bottom boundary layer and its effect on hypoxia dynamics will address this question.”

C5: [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 sink 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, managanese, and sulfur." In the present manuscript the list of "reduced metabolite" given P14903 excludes ammonium.

R: Unfortunately we didn't make it clear enough in the text, but nitrification is explicitly included in the calculation of the dissolved oxygen balance (as a component of water column respiration). We clarified this in the text (Page 14) as follow: “For simplicity, we are considering that oxygen consumption due to nitrification to be included in the

respiration term, and not as a separate process for deriving the oxygen balance. Though we are referring to the sum of respiration and nitrification as WR, we recognize that nitrification is a chemoautotrophic process. While not strictly accurate, this is consistent with the use of WR in the observational literature where measurements of water column oxygen consumption include the contribution of nitrification.”

We also clarified it in the caption of Figure 9 for the oxygen balance (Page 47) by writing explicitly “respiration+nitrification.”

We also followed the suggestion of adding ammonium to the sentence on reduced metabolites (Page 18).

C6: [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.

R: Agreed. In section 3.3 on page 14, we now say:

“In terms of air-sea exchange, oxygen is outgassing during summer and taken up during the rest of the year in all sub-regions, corresponding to the seasonal pattern in water column metabolism (more heterotrophic in winter and less heterotrophic or autotrophic in summer) and the seasonal cycle of surface water temperatures, which affect oxygen solubility contributing to outgassing in summer and uptake in winter.”

On Page 16 we now say:

“The positive net community production and decreasing oxygen solubility associated with the increasing water temperature in summer lead to oxygen outgassing to the atmosphere and net transport of oxygen downward to deeper waters.”

Also in section 4.2 on page 20 we now say:

“The decreased oxygen solubility of warmer waters typical of summer conditions also promotes outgassing, but the effect is relatively small compared to the autotrophy in surface waters (oxygen gas-exchange is fast and the summer change in water temperature is relatively small on the LA shelf). ”

We would also like to note that oxygen gas-exchange is fast and kinetic limitation negligible in this context. We have carried out sensitivity experiments where we doubled and halved the gas exchange coefficient and found only negligible changes in the results.

C7: [Table 3] The SOC bias is estimated by comparing model values to observations according to the DO ranges. This approach is strongly dependent 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 compare 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 a 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).

R: We have not found an along-shelf gradient in SOC. However, there is an across-shelf gradient with SOC increasing from inshore to offshore, which is driven by changes in bottom water oxygen concentrations such that offshore sites (in 50 m water depth) have higher SOC because of higher bottom water oxygen than inshore sites with lower bottom water oxygen. The SOC spread among the available different data sources is not due to a spatial pattern. Also, SOC measurements from all sources, except those from McCarthy et al. (2013), which were collected with a different method, show a dependence on oxygen concentration. Therefore we choose to compare model values with observations according to the DO ranges in Figure 7.

C8: [Fig. 5] Fig. 5 is not really exploited in the discussion. Why is this figure essential?

R: We feel that the vertical profiles of model bias in Figure 5 provide a good illustration of how model and data agree throughout the water column, and how the two simulations differ. Hence we chose to retain the figure to illustrate that point.

C9: [Fig 7.] SOC is a function of 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.

R: The physical model configuration is identical in the two simulations (i.e., the temperature distribution). At the same location and time, both model simulations have the same temperature, but the “Model+CCR” has lower DO concentrations. In Figure 7, at the same DO range, the SOC curve in “Model+CCR” corresponds to locations and times with lower temperatures than those in the “Model” and hence lower SOC values.

TECHNICAL COMMENTS

C10: [P14893, L11] Boyer et al, 2005 or 2006?

R: Corrected, it should be Boyer et al., 2006.

C11: [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.

R: They don't vary seasonally. We have changed the sentence (page 7) as follows:

“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.”

C12: [P14899 L 18] Please provide the exact time frame of integration.

R: The sentence (Page 14) has been changed as follows: “In this section, we evaluate the DO balance for the summer period (June to August) for different regions of the LA shelf to identify the key processes controlling hypoxia.”

C13: [P14905 L 29] The reference Kemp et al. 1992 does not appear in the bibliography.

R: The reference is now added to the bibliography.

C14: [FIG 6.] Split the y-label: PP for the upper part; Water community respiration for the lower part.

R: Figure 6 was redone as suggested.

C15: [Fig. 9]: Should be introduced in section 3.1

R: In response to the reviewer’s concern that there are too many figures, we removed Figure 9 (‘Vertical distribution of hypoxia probability’) and instead refer to Figure 6 in Fennel et al. (2013), which also shows that hypoxia most frequently occurs within a thin bottom water layer on the Louisiana shelf.

C16: [References] Refs Dagg et al., 2004; Green et al., 2006 ; Trefry et al., 1994 appears in the bibliography but not in the text

R: Removed.

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.

McCarthy, M.J., Carini, S.A., Liu, Z., Ostrom, N.E., and Gardner, W.S.: Oxygen consumption in the water column and sediments of the northern Gulf of Mexico hypoxic zone., *Estuar. Coast. Shelf. Sci.*, 123, 46-53, 2013.