Reviewer 1:

Major comments:

1. This manuscript contains a lot of data that a priori looks very good. Good comparison of the different methods can be done only in a particular area, while the rest of the data, mainly the pCO2 and NCP from O2/Ar, would be a good data set. In the section Methods there are a lot of explanation of how to arrive to the NCP calculations that can be found in the literature but very little is explained about replicates, average, and data quality controls or transformations (e.g. PQ to change from O2 units to C units is missed)

   Response: In the revised manuscript, the Methods section has been shortened by 20% in length by citing literatures for established NCP calculations and by removing unnecessary explanations. Relevant information has been added to explain the replicates, average, and data quality controls of the measurements of DIC, TA, DO, pCO_2, O_2/Ar and nutrients when available. The transformations of the NCP units were described in the original manuscript: "To facilitate the comparison, we converted NCP estimates from the different approaches (Eqs. 6, 10, 15 and 16) to the same unit in term of carbon (mmol C m^{-2} d^{-1}) using the Redfield ratio of C:N:O_2 = 106:16:138." (page 9, lines 17-18).

2. The manuscript needs a little bit of more order to be able to read it fluid. Suggestions are made in the pdf attached.

   Response: We sincerely thank the reviewer for the very detailed comments and suggestions which significantly improve our manuscript. Please see our responses to minor comments below.

3. Part of the discussion is based in differences between methods due to different stratified or mixed column states. I think the writer is interchanging "mixed layer" and "mixing column" concepts and making therefore wrong assumptions.

   Response: In order to clearly show the different stratified or mixed water column states (as well as the different turbidity conditions), the vertical profiles at typical stations have been presented in the revised manuscript as the newly added figures in the supplement. In the nGOM, the water column was well-mixed in certain nearshore shallow regions (e.g., the high-turbidity Atchafalaya coastal water, Fig. S2) while stratification was observed in offshore regions (Fig. S3) and in the lower Mississippi river channel (Fig. S4). We also discuss the influences of physical factors on NCP_{O2Ar} estimation under different mixing conditions. As shown in the newly added Figure 8, the NCP_{O2Ar} in the nearshore well-mixed shallow stations contained the contributions from both water column and sediment mechanisms (Fig. 8a). On the contrary, the NCP_{O2Ar} in the offshore stratified system only reflected the NCP of the biological community in the mixed layer (Fig. 8b). Meanwhile, we highlight the impact of the lateral transport of strongly net heterotrophic Mississippi and Atchafalaya river water (please see our responses to reviewer 2, general comment and major comment 5). We have amended the Results and Discussion sections to make these points clear to the readers.

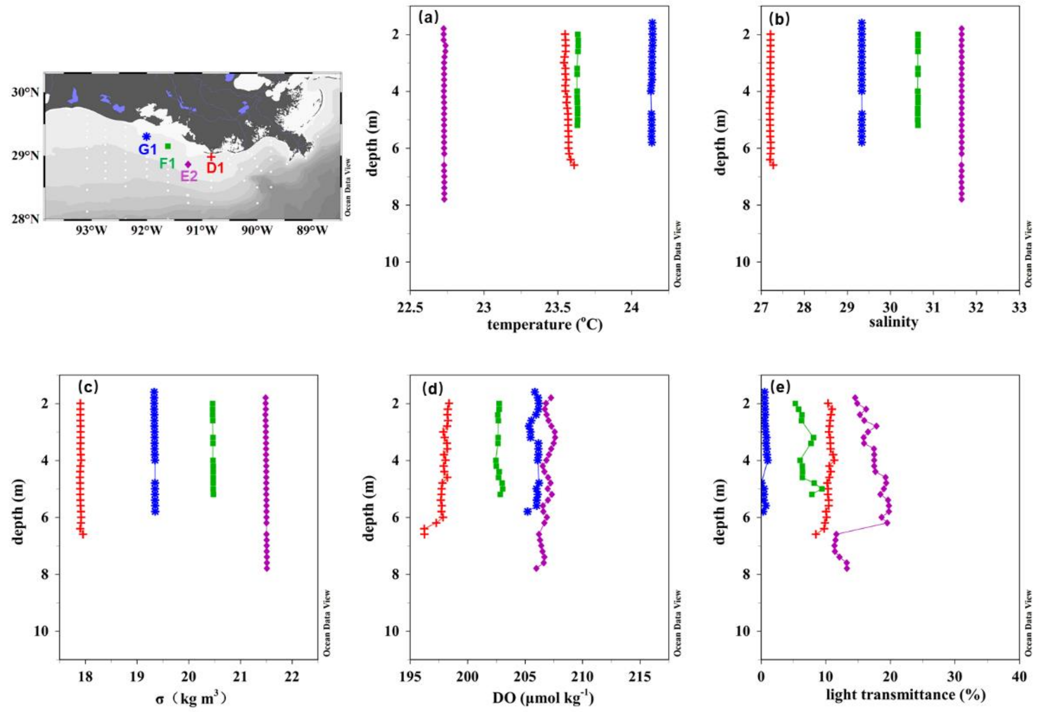


Figure S2 The vertical profiles of (a) temperature, (b) salinity, (c) density anomaly, σ = density (kg m^{-3}) – 1000, (d) DO, and (e) light transmittance at the well-mixed nearshore stations. The different colors correspond to the measurements from different sites shown in the map in the upper left corner.

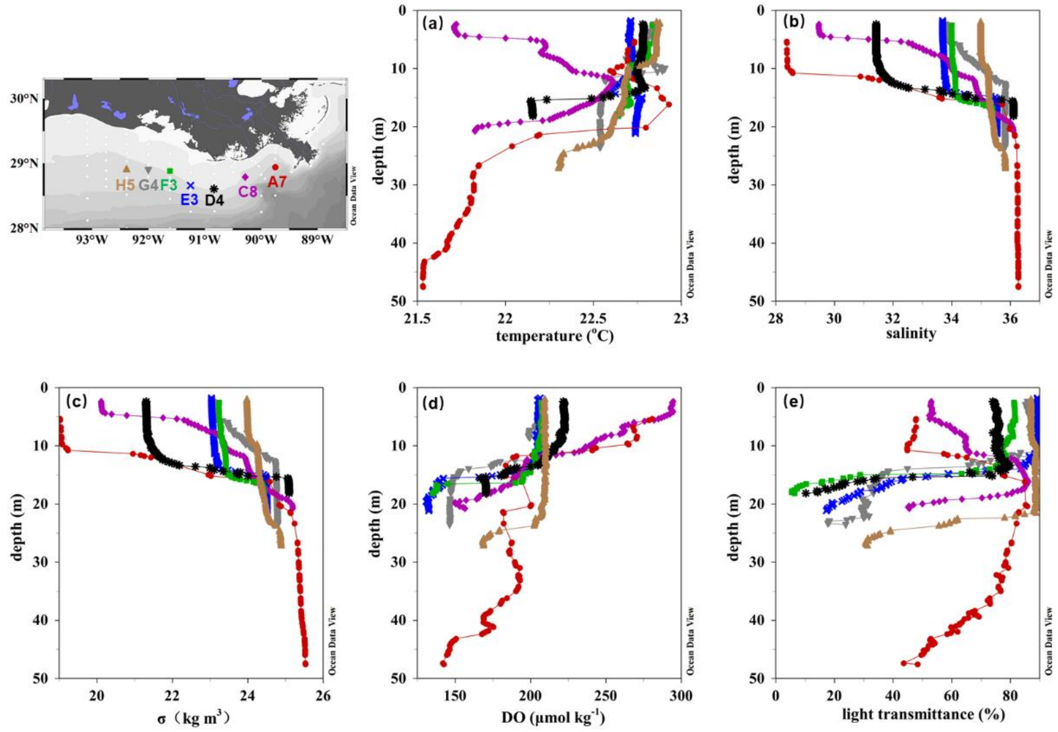


Figure S3 The vertical profiles of (a) temperature, (b) salinity, (c) density anomaly, σ = density (kg m^{-3}) – 1000, (d) DO, and (e) light transmittance at the stratified offshore stations. The different colors correspond to the measurements from different sites shown in the map in the upper left corner.

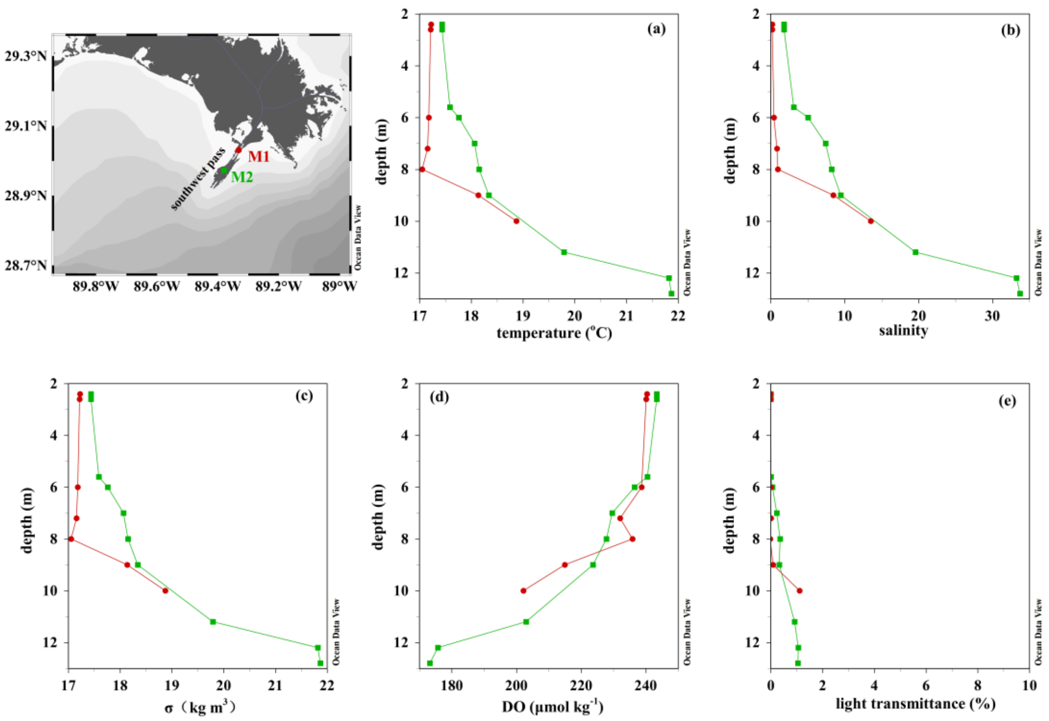


Figure S4 The vertical profiles of (a) temperature, (b) salinity, (c) density anomaly, σ = density (kg m^{-3}) – 1000, (d) DO, and (e) light transmittance at stations M1 and M2 in the lower Mississippi River channel. The different colors correspond to the measurements from different sites shown in the map in the upper left corner.

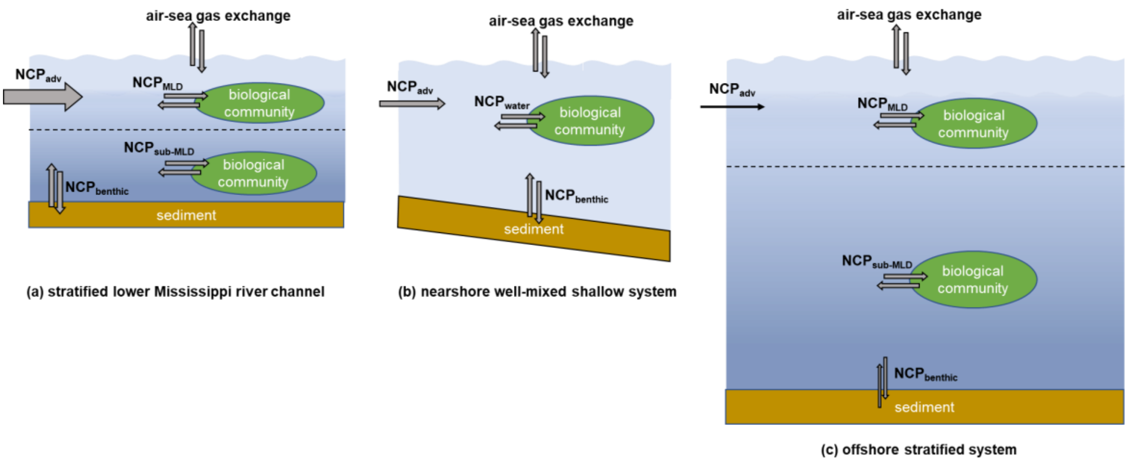


Figure 8 The differences in water column mixing conditions and NCP_{O2Ar} estimation in the nGOM. The dotted lines in panels (a) and (c) indicate the mixed layer depth. In the stratified lower Mississippi river channel (a) and the offshore stratified system (c), NCP_{O2Ar} reflected the combined effects of the biological community in the mixed layer (NCP_{MLD}) and lateral advection (NCP_{adv}). In the nearshore well-mixed shallow system (b), NCP_{O2Ar} reflected the influences of water column and benthic metabolisms (NCP_{water} + $NCP_{benthic}$) and lateral advection (NCP_{adv}).

4. Uncoupled 02 and CO2 fluxes and definition of sink or source of CO2 can be the result of smoothing CO2 values to a mean, but I cannot really tell without knowing the raw data. Also

comments attached.

Response: The O_2 and CO_2 fluxes were both calculated from high-resolution pCO_2 and DO data (1 minute averaged) from the continuous underway measurements. There was no smoothing of CO_2 values when discussing the uncoupled O_2 and CO_2 fluxes. These points have now been clearly stated in the Methods section in the revised manuscript.

5. Supplementary material of Figure S1. Is the data from the webpage only for the river discharge? This graph doesn't have x axis label. It should use dates instead and change consecutive numbers 1-12 to meaningful dates.

   Response: Both the river discharge and NO_x flux data were from the USGS (now explained in the figure caption of Fig. S1). The x axis label "month" has been added and the numbers 1-12 have been replaced by the meaningful dates "Jan, Feb, …Dec" as suggested.

6. Supplementary material of Figure S2 (a,b) shows regresion lines to show tendencies forced by few data points in the river. R2 is not shown but is expected to be very low to use it.

   Response: This figure has now been moved to the main text as suggested by minor comment 44 below. The lines in this figure are conservative mixing lines (not regression lines) which demonstrate the changes of concentrations of DIC and NO_x responding to the conservative mixing between river end member and seawater end member. We have clarified this in the revised manuscript as: "The enhanced biological production resulted in significant deviations of DIC and NO_x from their conservative mixing lines (Fig. 7)."

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2019-88/bg-2019-88-RC1-supplement.pdf

Minor comments:

1. Page 1, line 23 "use the same number of decimals for each number"

   Response: Corrected as suggested.

2. Page 1, lines 27-28: "need to talk in the discussion about the "slow" air-sea gas exchange"

   Response: In the revised manuscript, we now demonstrate the slow air-sea CO_2 exchange rate (compared to that of O_2) in the run-1 simulation by calculating the re-equilibrium time for CO_2 and O_2 after a biological perturbation (see the updated Fig. S5 below). The water is assumed to be in equilibrium with atmosphere at day 0 in the simulation. After the 15-day autotropic production (day 0 to day 15), the re-equilibrium time for O_2 (a few days, Fig. S5b) is significantly shorter than that of the pCO_2 (a few months, Fig. S5c). Because the slow air-sea CO_2 exchange rate due to the buffering effect by the DIC pool is well-known in the carbonate system, we remove the original Figure 10 and add this updated figure to the supplement according to the suggestion from reviewer 2 (minor comment 44).

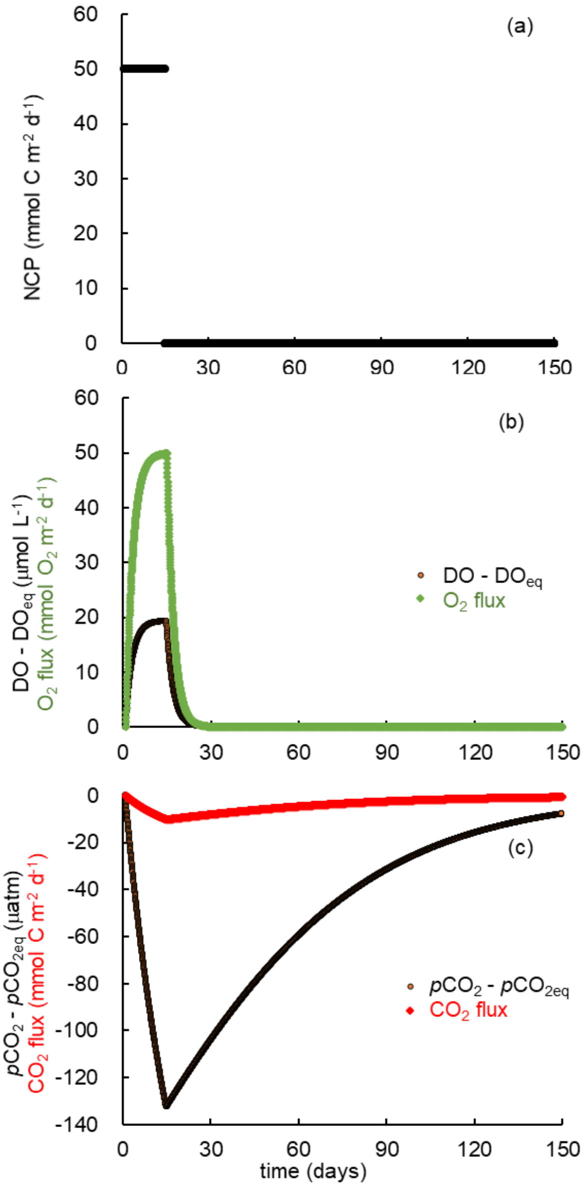


Figure S5 Simulation of CO_2 and O_2 dynamics responding to NCP and gas exchange using a 1-D model. The water is assumed to be in equilibrium with atmosphere at day 0. The NCP is assumed to vary with time, NCP = 50 mmol C m^{-2} d^{-1} from day 0 to day 15 and NCP = 0 from day 16 to day 150. The variations of (b) CO_2 flux and the deviation of pCO_2 from equilibrium (pCO_2 - pCO_{2eq}) and (c) O_2 flux and the deviation of O_2 from equilibrium (DO-DO_{eq}).

3. Page 2, line 2: “reference”
   Response: Corrected.

4. Page 3, line 14: “if it is the first time you use an acronym, define it”
   Response: Corrected.

5. Page 3, line 19: AND SLOW air-sea exchange
   Response: Revised as suggested.

6. Page 3, line 22: “I am struggling to place the sampling site on a map. Give reference context area in the map. show where the river is. From this map it doesn't seem to have a rive but just coast line.”
Response: We have modified Figure 1 and its caption (see below) to better show the sampling sites as well as to highlight the Mississippi River and Atchafalaya River.

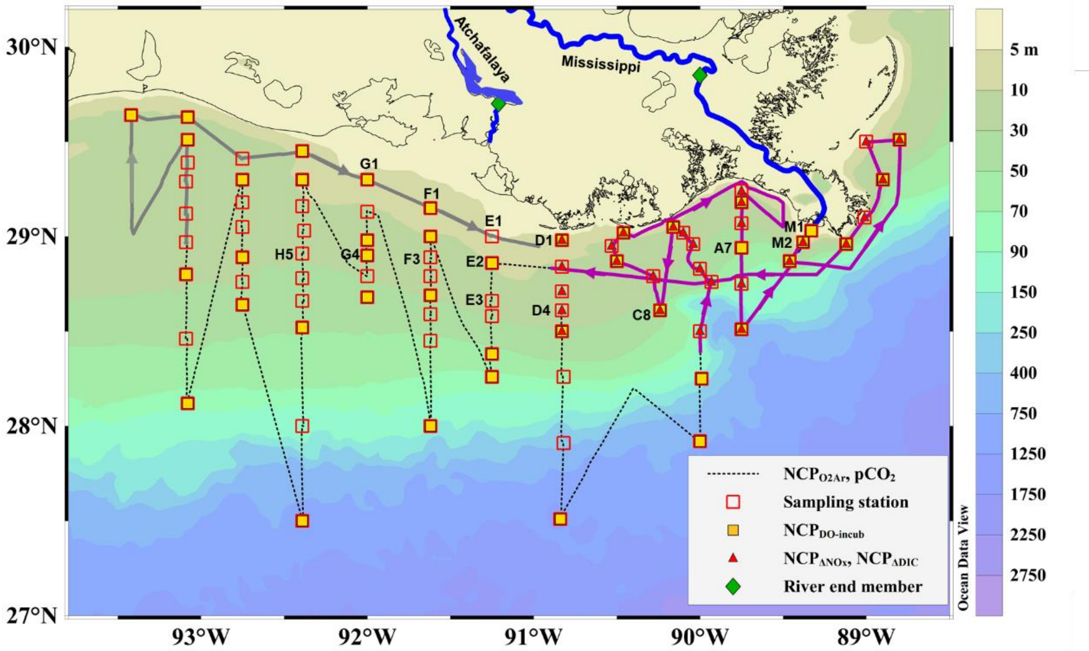


Figure 1 Map and sampling sites in the northern Gulf of Mexico during the April 2017 cruise. The black dotted line is the cruise track where high-resolution underway measurements were made. The track in the Mississippi plume (purple line, Apr. 8-10) and the Atchafalaya coast (gray line, Apr. 15-17) are highlighted. Also shown are the sampling stations (hollow red squares), stations where light/dark bottle DO incubations were conducted (solid yellow squares), stations where non-conservative changes in DIC and NO_x were used to estimate net community production (NCP) rates in the Mississippi plume (solid red triangles), and stations where the properties of river end members were measured (solid green diamonds).

7. Page 3, line 23: “rephrase: "at 83 sampling stations" at the end of the sentence”
Response: Revised as suggested.

8. Page 3, line 24: “active”
Response: Corrected.

9. Page 4, lines 1-2: “of what?”; “change and for a ","”; “profiles or water column profiles”, “remove, you said the brand already in the sentence above”.
Response: This sentence has been revised to now state “Discrete water samples for DIC, TA, DO and nutrients were collected at 3-12 depths per station depending on the bottom depth and vertical profiles of temperature, salinity and O_2.”

10. Page 4, lines 7-8: “precision and accuracy are not the same thing, you can not give one value

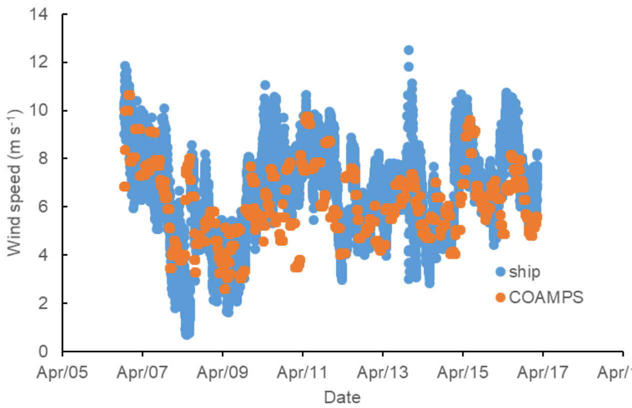
for both", "? why do you cite? did you measure you own accuracy?"
Response: These sentences have been revised to now state "The precision of DIC and TA measurements were better than 0.1%. DIC and TA measurements were calibrated, with accuracy both within 2 μmol kg^{-1}, by the certified reference materials provided by A. G. Dickson, Scripps Institution of Oceanography".

11. Page 4, line 10: "include this two points in the map"
Response: These two sampling points have been included in the updated Fig. 1.

12. Page 5, line 11: "why air and not water equilibrated with air? Give reference of who does calibration with air."; "the instrument precision needs to be calculated for your instrument, as it may differ from Cassar's equipment"
Response: The calibration of the O_2/Ar instrument was carried out according to an established method by measuring air as the atmospheric O_2/Ar is essentially constant (Cassar et al., 2009). We have added the reference for the calibration procedure and described the instrument precision as "The instrument precision, estimated from the repeated measurements (n = 1000) of atmospheric O_2/Ar ratio, was ± 0.3%."

13. Page 5, line 13: "Merge 2.3 and 2.4 and use subheadings for the different calculations."
Response: As the NCP estimates involved several different methods and unit conversion, we think it is better to describe NCP in a separated section.

14. Page 5, line 20, "probably because COAMPS use the buoys data as well. What about the wind measured from the ship?"
Response: The COAMPS wind speed also agreed well with the ship measurement (see the figure below). As wind measured from the ship refers to instantaneous speed while biogeochemistry signals and air-sea flux change on a longer time scale, the daily averaged COAMPS wind speed is preferred in our study. As the O_2 and CO_2 fluxes were calculated from the same wind speed, using COAMPS data or ship measurement won't affect the discussion on the relationship between O_2 and CO_2 fluxes.



Comparison between COAMPS wind speed and ship wind measurements

15. Page 5, line 23: "why? and what happen when sea values are close to 405? what % of the sea values where close to 405?"

Response: Paired pCO_{2sea} and pCO_{2air} are needed for the calculation of air-sea CO_2 flux (Eq. 1). In our study, the underway pCO_2 measuring system switched between the measurements of pCO_{2sea} and pCO_{2air}. pCO_{2air} was only measured every 3 h in order to better capture the variability of pCO_{2sea} and as pCO_{2air} is not expected to change rapidly. "Comparing to the large variations in pCO_{2sea} (110-1800 μatm), the variability of pCO_{2air} (± 4 μatm) was minor and the pCO_{2air} is set at a uniform value of 405 μatm as the mean observed concentration for the flux calculation." This is a common practice for CO_2 flux calculation in the CO_2 community. When pCO_{2sea} value is close to 405 μatm, the seawater is close to be equilibrium with the atmosphere and the CO_2 flux is close to zero.

16. Page 6, line 1: "define the term before saying how it was calculated"
Response: The definition of γ_{DIC} is "the pCO_2 buffer factor response to change in DIC", so it has already been defined in this sentence. However, this variable (shown in the original Fig. 10) is not very useful in our discussion. It has now been removed from the revised manuscript as the original Fig. 10 was replaced without showing this variable (please see the response to minor comment 2 above).

17. Page 6, line 4: "it is not clear if you calculate oxygen fluxes from the optode, incubations?"
Response: The oxygen fluxes were calculated from the high resolution optode measurements (not from incubations). We have clarified this in the revised manuscript as: "$[O_2]_{meas}$ is the seawater DO concentration from the underway optode measurement and $[O_2]_{sat}$ is the saturated DO concentration calculated from the measured sea surface temperature and salinity".

18. Page 6, line 8: "This section can be reduced by removing the majority of the explanatory equation, leaving only the main ones and citing the authors or papers where the equations are explained."
Response: We have shortened this section by 20% in length as suggested.

19. Page 6, line 10: "you can simplify the subindex, no need to say write incub, it has been already explained."
Response: In order to make these variables readily understood in the figure legend, we have chosen to keep the subindexes as they were originally described.

20. Page 6, line 13: "cite Craig and Hayward [1987]"; "you give details bellow. remove this part of the sentece."
Response: Revised as suggested.

21. Page 6, line 14: "dissolved oxygen concentration in the surface water is affected by physical (e.g., changes in temperature, salinity and atmospheric pressure, bubble dissolution and/or injection) and biological processes (photosynthesis and respiration) (Fig 2)."
Response: Revised as suggested.

22. Page 7, line 1: "how did you arrive to the equation? say something."

Response: This sentence has been revised to now state: "Biologically mediation oxygen supersaturation $\Delta(O_2/Ar)$ is defined as in Casser et al., (2011), Jonsson et al., (2013), and Kaiser et al., (2005) to be..."

23. Page 7, line 4: "double check this this equation with Kaiser 2005 and Cassar 2011, I do not think it is totally correct."
Response: We confirmed that this equation is correct (see equations 1 and 3 in Cassar, 2011). According to minor comment 18, this explanatory equation has been removed and only one main equation is now presented in the revised manuscript:

$$\mathrm{NCP_{O2Ar}} = \mathrm{bioflux} = k_{O2}[O_2]_{sat}\Delta(O_2/Ar)$$

24. Page 7, line 17: "you have to explain here how you passed from O2 units to C."
Response: The unit conversions of NCP estimates were described later in the original manuscript: "To facilitate the comparison, we converted NCP estimates from different approaches (Eqs. 6, 10, 15 and 16) to the same units in term of carbon (mmol C m^{-2} d^{-1}) using the Redfield ratio of $C:N:O_2$ = 106:16:138." (page 9, lines 17-18).

25. Page 7, line 23: "what did you do with the dark ones?"
Page 8, line 7: "you do not say how this samples where incubated."
Response: Change text to: "Clear and dark bottles were placed into a deck incubator screened at 50% of ambient sunlight, which was plumbed with flowing seawater from the MIDAS system in order to maintain surface water temperatures."

26. Page 8, line 3: "how many samples? SD of the spectrophotometric method etc?"
Response: Clarified by adding the following "The mean difference between DO obtained by the probe and the spectrophotometric method of ±5% was consistent with previous comparisons of probe measured versus Winkler measured DO based on several hundred comparisons (Murrell et al. 2013)."

27. Page 8, line 7: "Did you do it as somebody else who publish before? cite. GPP=NCP-R, not sure how you calculated GPP"
Response: In our study, gross primary production (mmol O_2 m^{-3} d^{-1}) was calculated as GPP = NCP + |Resp| = $R_{DOlight}$ + $|R_{DOdark}|$, where $R_{DOlight}$ is the change in DO in the light bottles during the 24-h incubation and is equivalent to NCP in the bottles, and R_{DOdark} is the 24-h change in DO in the dark bottles. As respiration-induced DO change is negative, our equation GPP = NCP + |Resp| is equivalent to GPP = NCP - R.

28. Page 8, line 15: "Integration calculation is different than for respiration. Need to justify it better or cite."
Response: In our study, the respiration rate was assumed to be uniform in the mixed layer and the integrated respiration over the MLD ($Resp_{Int}$, mmol O_2 m^{-2} d^{-1}) was calculated as $Resp_{Int}$ = R_{DOdark}*MLD. However, the GPP varied with depth due to the changes in light environment at different depths. In our study, we assumed that GPP was linearly dependent on light up to a

maximum GPP_{max} that occurred when %PAR = 50%. The assumption of a linear response in photosynthesis with increasing light up to some light saturation threshold is a simplification but is based on previous measurements from this shelf that indicate that photosynthesis begins to saturate at light levels of around 200 μmol quanta m^{-2} s^{-1} (Lohrenz et al., 1994), which is roughly 50% of light in the surface mixed layer (Lohrenz et al., 1999). The GPP measured at 50% light in the incubations was thus considered the maximum light-dependent GPP rate (GPP_{max}). To calculate the integrated GPP in the mixed layer (GPP_{Int}, mmol O_2 m^{-2} d^{-1}), the GPP was scaled by the light environment in the MLD:

if %PAR ⩾ 50%, $GPP_{Int} = GPP_{max} * MLD$ (7)

if %PAR < 50%, $GPP_{Int} = 2 * \%PAR * GPP_{max} * MLD$ (8)

The coefficient 2 in Eq. 8 was used so that the product 2*%PAR would scale from 0 to 1, i.e. GPP approaches GPP_{max} at %PAR = 50%."

We have amended this paragraph to better describe our incubation and calculations.

29. Page 8, line 10: "NOT sure about this equation, please CITE does it comes from BEER-LAMBERT? PIERSON 2008?"
Response: Reference (Lohrenz et al., 1999) has been cited.
Lohrenz, S.E., Fahnenstiel, G.L., Redalje, D.G., Lang, G.A., Dagg, M.J., Whitledge, T.E. and Dortch, Q., 1999. Nutrients, irradiance, and mixing as factors regulating primary production in coastal waters impacted by the Mississippi River plume. *Continental Shelf Research*, *19*(9), pp.1113-1141.

30. Page 9, line 17: "I couldn't access to the whole paper but looks like there are extensive studies of this river that may provide tau, for example: Lane, Robert R., et al. "Seasonal and Spatial Water Quality Changes in the Outflow Plume of the Atchafalaya River, Louisiana, USA." Estuaries, vol. 25, no. 1, 2002, pp. 30–42. JSTOR, www.jstor.org/stable/1352905."
Response: Although there were some previous studies on the Atchafalaya River (including the one provided by the reviewer), plume resident time for different salinity ranges were currently not available from the literature.

31. Page 10, line 2: "this figure needs to improve x axis"
Response: The x axis label "month" has been added and the numbers 1-12 have been replaced by the meaningful dates "Jan, Feb, …Dec".

32. Page 10, line 19: "the reader doesn't have to know where is Texas and you already said westward, remove Texas"
Response: Corrected.

33. Page 10, line 19: "from when is this data?"
Response: Zhang et al., (2012) provided a model simulation of the multiple-year (2005–2010) average Mississippi plume distribution. This sentence has been revised to state "which was similar to the modeled multiple-year (2005-2010) average pattern of the Mississippi downcoast freshwater transport in April (see Fig. 4 in Zhang et al., (2012))"

34. Page 11, line 1: “remove this”; “which parameter are you talking about in this sentence? and in Figure 3?”
Response: The repetitive word has been removed. This sentence has been revised to now state “The coastal water outside of Atchafalaya Bay (90.50-92.30° W, Fig. 5a) was affected by Mississippi freshwater, however, the mixing conditions and water chemistry in this region was significantly different from those in the Mississippi plume around the Mississippi Delta (Fig. 3).” These differences were described in the following sentence.

35. Page 11, line 4: “remove brand name”
Response: Corrected.

36. Page 11, line 8: “this should go up much earlier in the text! just before speaking about figure 5”
Response: This sentence has been moved to the beginning of this paragraph.

37. Page 11, line 24: “this can be due to sedimentation, more light than insitu”
Response: We fully agreed with the reviewer. In the revised manuscript, we have added vertical profiles of light transmittance in different regions (see Fig. S2, S3 and S4 above) and discuss the potential bias in light/dark incubation method: “For high turbidity water samples (e.g., samples collected in the Mississippi river channel and along the Atchafalaya coast, Fig. S2, S4), sedimentation of particles in incubation bottles could alleviate light limitation for phytoplankton and thus result in overestimation in $NCP_{DO\text{-}incub}$.”

38. Page 12, line 1: “subplots of figure 7 and 8 show the same information several times. It is confusing for the reader have to look at both. Choose one represent better what you are explaining and cite only one per time.”
Response: Figure 7 is chosen to present the Mississippi results.

39. Page 12, line 1: “instead of calling region 1 2 3 in the text and the graphs, call it by this headaings names, both in the text and in the graphs. remove the regions words
Response: Revised as suggested.

40. Page 12, line 2-3: “Fig 7a”; “give values in brackets”; “ 7b, chla was low but the highest in the area!”; “Fig 8a”;
Response: Revised as suggested.

41. Page 12, line 7: “only 8c”
Response: Revised as suggested.

42. Page 12, line 9: “choose”
Response: Corrected.

43. Page 12, line 8: “minus”
Response: Corrected.

44. Page 12, lines 16-18: "you have to explain this figure first. If it is important it should be a main figure and not as supplementary plot"; "number doesnt agree with figure"
Response: This figure has been moved to the main text and explained. Note that the numbers here are the deviations of DIC and NO_x from the conservative mixing lines but not the absolute concentrations. To clarify this, these sentences have been revised to now state "The enhanced biological production resulted in significant deviations of DIC and NO_x from their conservative mixing lines (Fig. 7). The significant non-conservative removal of DIC (up to 250 µmol kg^{-1} deviation from the conservative mixing, Fig. 7a) and nutrients (up to 35 µmol kg^{-1} in NOx, Fig. 7b) that occurred in the Mississippi plume agreed with findings of previous studies (Cai, 2003; Guo et al., 2012; Huang et al., 2012).".

45. Page 12, line 22: "you said before that chla was low in the Missisipi region. Here you say that Atchafalaya region is chracterized by elevated Chla, but the values are lower than Missisipi region. You have to correct this."
Response: We recognize that the original wording in the text was confusing. We were comparing Chl-a concentrations in the same plume system (either Mississippi or Atchafalaya) in different salinity ranges. To make this point clearer, we have revised this sentence to now state "Within the Atchafalaya plume, elevated Chl-a, DO%, and NCP were observed at intermediate salinities where pCO_2 and the size of the oceanic CO_2 sink were also reduced."

46. Page 13, lines 2-6: "together"; "give numbers"
Response: Revised as suggested.

47. Page 13, line 22: "this graphs shows the MLD but without the sea bed depth we cannot know if there is stratification or not"
Page 13, line 25: "You just said above that the water column was stratified"; "if you said that it is mixed until the bottom, there is no separation between mixed layer and below, do not understand this sentence."
Page 14, line 1: "I do not see why that would be add uncertainty. Mixed layer represent better fluxes because in contact with the atmosphere, but if the whole water column is mixed, then all water column is in contact with the atmosphere and that is not a bias"
Page 14, line 14: "if it is mixed layer what you sampled, everything is mixed, cannot be heterogeneity inside the mixed layer. You can test this from chla profiles for examples"
Response: Please see our responses to major comment 3.

48. Page 14, line 15: "I think one of the bias here may be the turbidity. If the incubated samples are not mixed in the same way as natural water, the majority of the sediment probably sink to the bottom of the bottle and the light availability is bigger than in natural waters overstimating NCP."
Response: Please see our response to minor comment 37.

49. Page 14, line 22: "Why are there errors when calculating MLD?"
Response: This is because MLD is used as an input parameter for the calculations of $NCP_{\Delta DIC}$

and $NCP_{\Delta NOx}$ (Eqs. 15, 16). To clarify this, this sentence has been revised to now read "errors in estimating water residence time and MLD leads to proportional errors in the calculations of $NCP_{\Delta DIC}$ and $NCP_{\Delta NOx}$ (Eqs. 15, 16)."

50. Page 14, line 23: "IS THE co2 flux fast enought to cause this?"
Response: No, the CO_2 flux is generally small compared to NCP (<15% in most reports); but O_2 flux is nearly the same as NCP. To explain this, we revised this sentence as "the influence of air-sea gas exchange was not considered in the calculation of $NCP_{\Delta DIC}$ (Eq. 13) which would cause slight underestimation of $NCP_{\Delta DIC}$."

51. Page 15, line 3: "here is the only place were you have the four methods. Why in figure 4 seems to agree better than in figure 8? why there are more data point in this figures than sampling point? "
Response: The NCP data presented in Fig. 4 and Fig. 8 were the same dataset and the numbers of data points in these two figures were identical to the sampling sites shown in Fig. 1 (n=40 for $NCP_{DO\text{-}incub}$, n = 29 for $NCP_{\Delta DIC}$ and $NCP_{\Delta NOx}$). These data were more evenly distributed along with time in Fig. 4 but were clustered in the salinity range of 25-30 in Fig. 8.

52. Page 15, line 10: "this can be due to the bottle incubation method that may have exclude grazers. Did you do replicates? how was your standar deviation? if big that would be a reason"
Response: Grazers might be a factor which affected the bottle incubation. In our study, standard deviation from triplicate bottle incubations were on average about 16% of the mean.

53. Page 15, line 11: "you are contradicting again yourself here. If the water column is well mixed, there is not stratification and therefore no mixed layer and pycnocline."
Response: This sentence has been revised to now state "These nearshore waters were characterized by well-mixed water column which indicates that the surface O_2/Ar could be affected by both water column and benthic metabolisms".

54. Page 15, line 16: "there can be understimation if euphotic depth is deeper than mixed layer, but if you took all your four methods at the same depths, there is no bias in the comparison of the methods"
Response: We agree with this comment. As the samples for the four methods were all collected in the surface water, the production beneath the pycnocline did not result in bias in the comparison if no upwelling occurred. This sentence has been deleted.

55. Page 15, line 24: "that could be a result of"
Response: Corrected.

56. Page 16, line 7: "Did you explained anywhere that the values has been averaged? and why?"
Response: There is an ecological gradient along the river-ocean mixing continuum in the Mississippi plume: from high nutrient and turbid freshwater to clear, oligotrophic offshore oceanic waters. We now explain this both in the main text and in the figure caption as: "Data

were averaged over increments of two salinity units in order to better investigate the changes in NCP and CO_2 flux along this salinity gradient".

57. Page 16, lines 11-15: "I do not understand this sentence. Do you mean it was heterotrophic in the river and autotrophic at higher salinities? "; "could be. You didnt measured benthic respiration so you can only use hypotheses"
Response: As the water column in the lower Mississippi River channel was stratified (Fig. S4), benthic respiration had minor contribution to affect the NCP_{O2Ar} in the mixed layer. Whereas, the advection could play a significant role influencing the surface NCP_{O2Ar} signal in the Mississippi River channel due to the short water residence times. In the revised manuscript, the heterotrophy in the lower Mississippi River channel (indicated by the negative NCP_{O2Ar} rates of -51 mmol C m^{-2} d^{-1}, Fig. 8c) is attributed to the advection of CO_2-rich water from the upper river channel containing strong respiration signals supported by terrestrial carbon loading and urban wastewater. Please also see our response to major comment 7 by reviewer 2.
Although we didn't measure benthic respiration rates in our study, sediment and lower water column oxygen consumption have been reported on shelf-wide scale in the nGOM by previous studies (Murrell and Lehrter 2011; Murrell et al., 2013). The below-pycnocline respiration rates were reported to show low variability over a large geographic and temporal range (Murrell and Lehrter 2011). In the revised manuscript, we provide references and the reported range of below-pycnocline respiration rates (46.4 to 104.5 mmol O_2 m^{-2} d^{-1}) in the discussion section.

58. Page 16, line 11: "minus"
Response: Corrected.

59. Page 16, line 23: "You can find this explained in Seguro et al 2015 (The presence of a chl a peak in the middle of the inner part, after the salt wedge, is a typical feature of stratified and partially stratified estuaries (Voorhis et al., 1983; Collins and Williams, 1981; Humborg et al., 1997; Cloern et al., 2013). This happens because the decrease in turbidity allows a deeper photic layer than the mixing layer in the middle of the estuary (Sverdrup, 1953) where high concentrations of inorganic nutrients are still available (Figs. 3 and 5).)"
Response: Thanks for pointing out this. These references have been added.

60. Page 17, line 17: "you have to explain this graph first, what is the meaning of each quadrant"
Page 17, lines 17-21: there is a blue shadow in the text...which is coincident with an statement that said that both plumes have opposite patterns. I disagree, opposite patterns would be one plume in quadrant 4 and another in quadrant 2 or 3 and 1"; "this sentence is not clear"; "I think the author was trying to speak about Mississippi plume and HTACW?"
Response: We do agree with the reviewer that quadrant 4 and 2 (or 3 and 1) show the opposite pattern. This figure is explained and discussed in the revised manuscript as: "When plotting CO_2 flux against NCP_{O2Ar} (Fig. 9), the data located in quadrant 2 suggest net heterotrophy associated with CO_2 outgassing to the atmosphere and those in quadrant 4 indicate net

autotrophy associated with CO_2 uptake from the atmosphere. Most NCP_{O2Ar} and CO_2 flux data collected in the Mississippi River channel fall in quadrant 2 with negative NCP rates and positive CO_2 fluxes. On the contrary, the Mississippi plume and Atchafalaya plume waters exhibited opposite patterns with positive NCP rates and negative CO_2 fluxes (most data being in quadrant 4)."

61. Page 18, line 2: "i THINK THIS DISCUSSION would benefit from including the different residence times of CO2 and O2, as degassing would not occur at the same speed because of different solubilities of o2 and co2."
    Response: Please see the response to minor comment 2.

62. Page 18, line 10: "I would add this to the M & M section"
    Response: The model description has been moved to the Methods section as suggested.

63. Page 19, line 17: "As explained abpve, I do not agree with this"
    Response: See the response to major comment 3.

64. Page 25, line 20: "several typos"
    Response: Corrected.

65. Page 27, line 9: "purple line"; "blue line"
    Response: Corrected.

66. Page 31, line 2: "It is not clear how figure a and b has been created. Why region 3 is close to 0 in one plot but high numbers in the other? and the same happen with regions 1 and 2. If there is no data, the regions should be white colour."
    Response: This figure presented the results of the three end-member mixing model showing the composition of the surface water. Panel (a) showed the fractional contribution of the Mississippi River end member and higher values indicated more significant influence of the Mississippi plume. The same applied to panel (b) for the Atchafalaya River end member. In the revised manuscript, we removed the original panels (c) and (d) according to the major comment 8 by reviewer 2. Instead, we presented the fractional contribution of the offshore surface water end member in the updated panel (c) to better show the results of the three end-member mixing model (see the figure below).

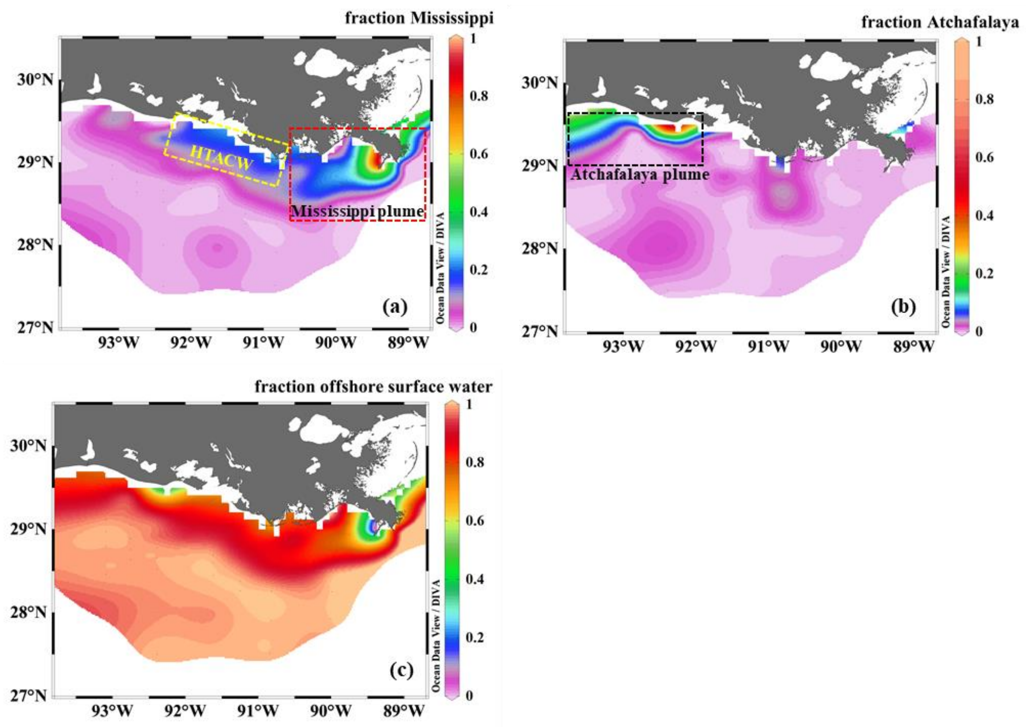


Figure 4 The fractional contribution of (a) Mississippi River, (b) Atchafalaya River, and (c) offshore surface water to the surface water in the nGOM estimated from the three end-member mixing model. The three sub-regions shown in panel (a) and (b) are Mississippi plume, high-turbidity Atchafalaya coastal water (HTACW), and Atchafalaya plume.

67. Page 34, line 2: "would it be posible to put the coloured boxed underneath? if not, just finish the box before covering the labels at the bottom, at the end of the y axis."
    Response: Revised as suggested.

68. Page 34, line 2: ", CO2 flux and NCP O2/Ar"
    Response: Corrected.

69. Page 34, line 4: "why?"
    Response: See the response to minor comment 56.

70. Trying to be consistent in the symbols and colours of Figure 7, seems that you accidentally exchanged the colour and symbol of two regions.
    Response: The figure legend has been corrected.

References Cited:

Cai, W. J. 2003. Riverine inorganic carbon flux and rate of biological uptake in the Mississippi River plume. Geophysical Research Letters **30**(2).

Cassar, N., B. A. Barnett, M. L. Bender, J. Kaiser, R. C. Hamme, and B. Tilbrook. 2009. Continuous high-frequency dissolved O_2/Ar measurements by equilibrator inlet mass spectrometry. Analytical Chemistry **81:** 1855-1864.

Cassar, N., P. J. Difiore, B. A. Barnett, M. L. Bender, A. R. Bowie, B. Tilbrook, K. Petrou, K. J. Westwood,

S. W. Wright, and D. Lefevre. 2011. The influence of iron and light on net community production in the Subantarctic and Polar Frontal Zones. Biogeosciences **8:** 227-237.

Guo, X., W.-J. Cai, W.-J. Huang, Y. Wang, F. Chen, M. C. Murrell, S. E. Lohrenz, L.-Q. Jiang, M. Dai, J. Hartmann, Q. Lin, and R. Culp. 2012. Carbon dynamics and community production in the Mississippi River plume. Limnology and Oceanography **57:** 1-17.

Huang, W. J., W. J. Cai, R. T. Powell, S. E. Lohrenz, Y. Wang, L. Q. Jiang, and C. S. Hopkinson. 2012. The stoichiometry of inorganic carbon and nutrient removal in the Mississippi River plume and adjacent continental shelf. Biogeosciences **9:** 2781-2792.

Jonsson, B. F., S. C. Doney, J. Dunne, and M. Bender. 2013. Evaluation of the Southern Ocean O_2/Ar-based NCP estimates in a model framework. Journal of Geophysical Research-Biogeosciences **118:** 385-399.

Kaiser, J., M. K. Reuer, B. Barnett, and M. L. Bender. 2005. Marine productivity estimates from continuous O_2/Ar ratio measurements by membrane inlet mass spectrometry. Geophysical Research Letters **32**.

Lohrenz, S.E., Fahnenstiel, G.L. and Redalje, D.G., 1994. Spatial and temporal variations of photosynthetic parameters in relation to environmental conditions in coastal waters of the northern Gulf of Mexico. Estuaries, 17(4), pp.779-795.

Lohrenz, S.E., Fahnenstiel, G.L., Redalje, D.G., Lang, G.A., Dagg, M.J., Whitledge, T.E. and Dortch, Q., 1999. Nutrients, irradiance, and mixing as factors regulating primary production in coastal waters impacted by the Mississippi River plume. Continental Shelf Research, 19(9), pp.1113-1141.

Murrell, M. C., and Lehrter, J. C. 2011. Sediment and lower water column oxygen consumption in the seasonally hypoxic region of the Louisiana Continental Shelf. Estuaries and Coasts, 34, 912-924.

Murrell, M. C., Stanley, R. S., Lehrter, J. C., and Hagy, J. D. 2013. Plankton community respiration, net ecosystem metabolism, and oxygen dynamics on the Louisiana continental shelf: Implications for hypoxia, Cont. Shelf Res., 52, 27-38, https://doi.org/10.1016/j.csr.2012.10.010.

Zhang, X. Q., R. D. Hetland, M. Marta-Almeida, and S. F. Dimarco. 2012. A numerical investigation of the Mississippi and Atchafalaya freshwater transport, filling and flushing times on the Texas-Louisiana Shelf. Journal of Geophysical Research-Oceans **117**(C11).