**Reviewer 2:**

**General comment:**

The authors present results of Net Community Production (NCP) in waters of the northern Gulf of Mexico system which includes a portion of the downstream Mississippi and Atchafalaya rivers, and the continental shelf where these rivers discharge in the Gulf of Mexico. The NCP was estimated through four different methods: continuous O2/Ar measurements, light/dark bottle incubations, DIC and NOx measurements. The authors also analyzed the relation between the NCP and pCO2 measurements to complete a picture in the metabolic state of the northern Gulf of Mexico (nGOM). The measurements were done during spring and summer in 2017 at an extensive network of stations sampled in vertical profiles and in continuous underway measurements along the ship track. The authors discuss the difference between the results from the different methods to estimate NCP. Their results show that during the sampling period and along the surveyed areas, the river headwaters are heterotrophic, while autotrophy (signaled by the highest measured NCP) characterized the continental shelf. With a 1-D model, the authors demonstrated a temporal mismatch between the estimated gas exchange and biological production, i.e. due to a decoupling between CO2 fluxes and NCP, at the time of the measurements, and this could be related to the presence of pCO2 transported from headwaters identified in areas where local productivity hints to dominant heterotrophy. The results of this work are interesting because the authors combine the traditional pCO2 measurements to NCP values to better understand the metabolism of the Gulf of Mexico shelf system.

Unfortunately, I find that the quality of the presentation of results, as well as the text itself lacks scientific rigor. The authors make a big effort on trying to explain the results and make use of assumptions that were not really proven by their results (such as the presence of benthic respiration to justify NCP-water column integrated heterotrophy) and make no effort to investigate further the role of physical factors. At this stage, I cannot recommend this manuscript for publication in Biogeosciences. I list major and minor comments in a supplementary pdf aiming to provide a more detailed review. I recommend the authors to consider these comments if they think they might be useful to improve their work.

Please also note the supplement to this comment: https://www.biogeosciences-discuss.net/bg-2019-88/bg-2019-88-RC2-supplement.pdf

Response: We sincerely thank the reviewer for pointing out the weaknesses of our original manuscript and for providing constructive comments and suggestions. In the revised manuscript, we now present vertical profiles at typical stations (the newly added figures S2, S3 and S4) to better describe the different stratified or mixed water column states. The water column was well-mixed in 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). The influences of physical factors (both vertical mixing and lateral advection) on the NCP estimation are also discussed in the revised manuscript. In the stratified regions (lower Mississippi River channel and offshore stations, Fig. 8a, c), $NCP_{O2Ar}$ only reflected the NCP of the biological community in the mixed layer. On the contrary, $NCP_{O2Ar}$ in the well-mixed regions (the high-turbidity Atchafalaya coastal water, Fig. 8b) contained the contributions from both water column and sediment mechanisms. Meanwhile, we highlight the impact of the lateral transport of strongly net heterotrophic Mississippi and Atchafalaya river water. Because of the high air-sea $O_2$ exchange rate, it generally takes a few days for $O_2$ to be equilibrium with the atmosphere (please also see the response to reviewer 1, minor comment 2). Therefore, lateral advection of heterotrophic river water played an important role affecting the $NCP_{O2Ar}$ within the river channel where the water transport speed was rapid (Fig. 8a). Whereas, the influence of horizontal transportation of the heterotrophic river water decreased offshore with the increasing water residence time, and the influence of remote source water heterotrophy was negligible in most offshore regions where water residence time was sufficiently long (Fig. 8c). Please also see the response to minor comment 5 below.

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.

In addition, we have revised the manuscript throughout to improve English and scientific rigor according to the detailed suggestions from the two reviewers.

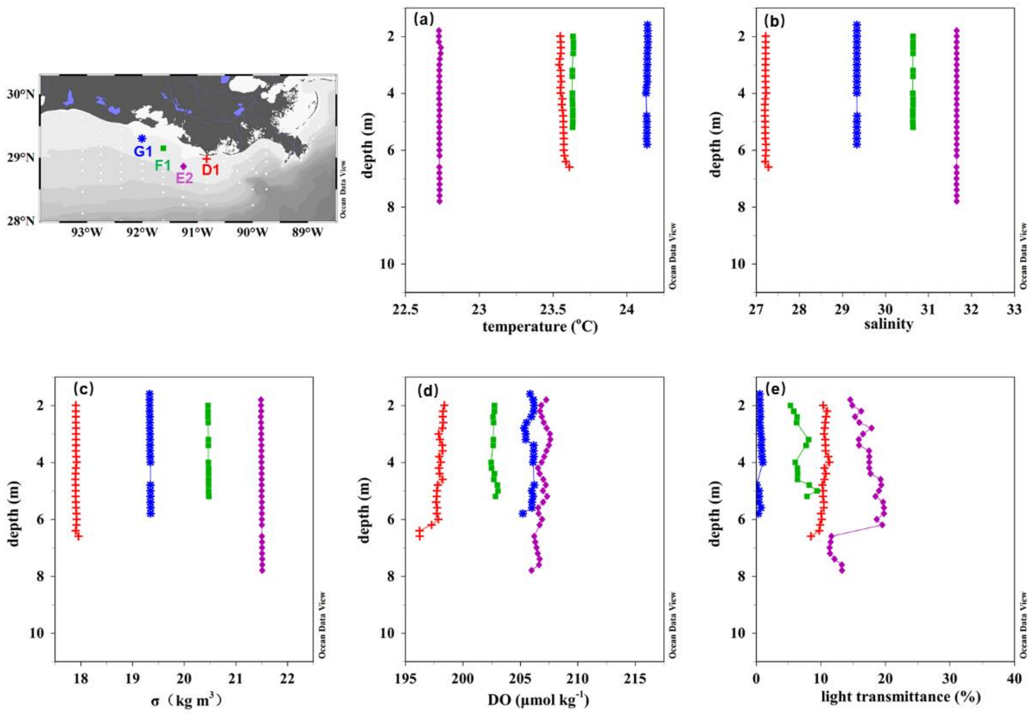


Figure S2 The vertical profiles of (a) temperature, (b) salinity, (c) density anomaly, σ = density (kg $m^{-3}$) – 1000, (d) DO, and (e) light transmittance at the well-mixed nearshore stations. The different colors correspond to the measurements from different sites shown in the map in the upper left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

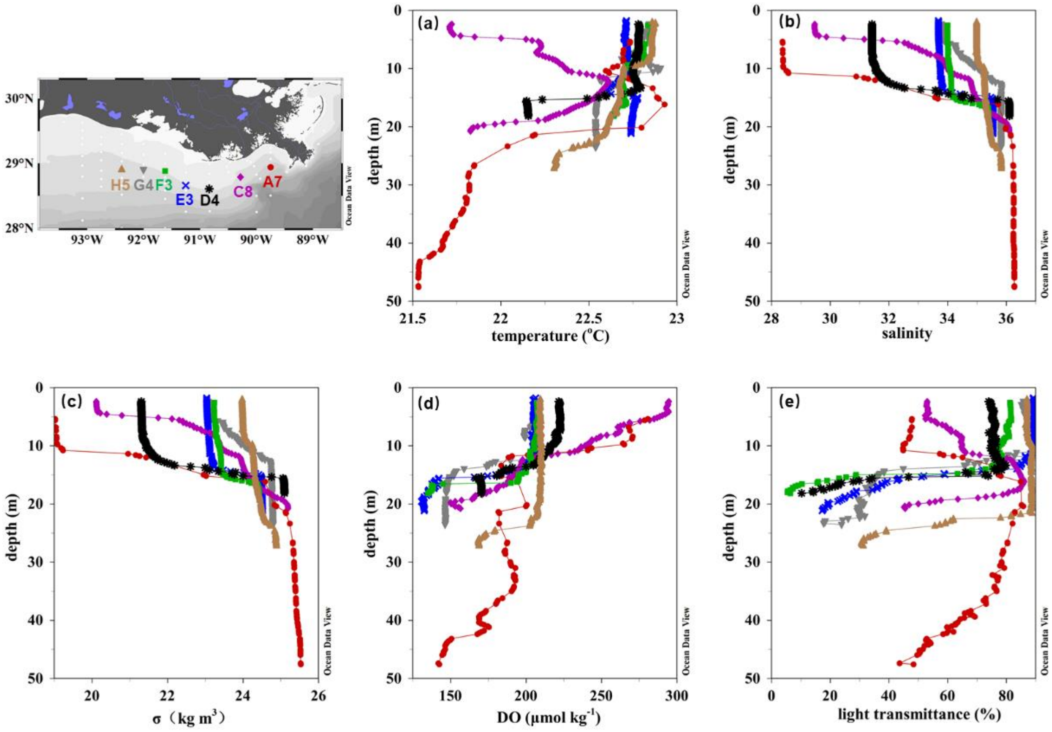


Figure S3 The vertical profiles of (a) temperature, (b) salinity, (c) density anomaly, σ = density (kg m$^{-3}$) – 1000, (d) DO, and (e) light transmittance at the stratified offshore stations. The different colors correspond to the measurements from different sites shown in the map in the upper left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

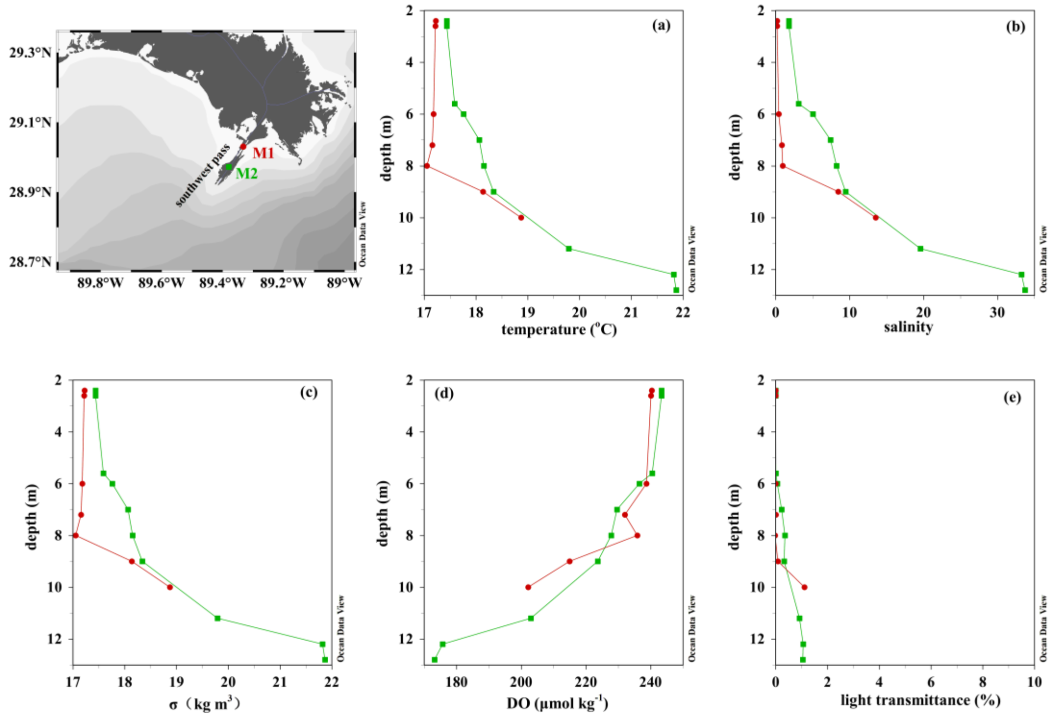


Figure S4 The vertical profiles of (a) temperature, (b) salinity, (c) density anomaly, σ = density (kg m$^{-3}$) – 1000, (d) DO, and (e) light transmittance at stations M1 and M2 in the lower Mississippi River channel. The different colors correspond to the measurements from different sites shown in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

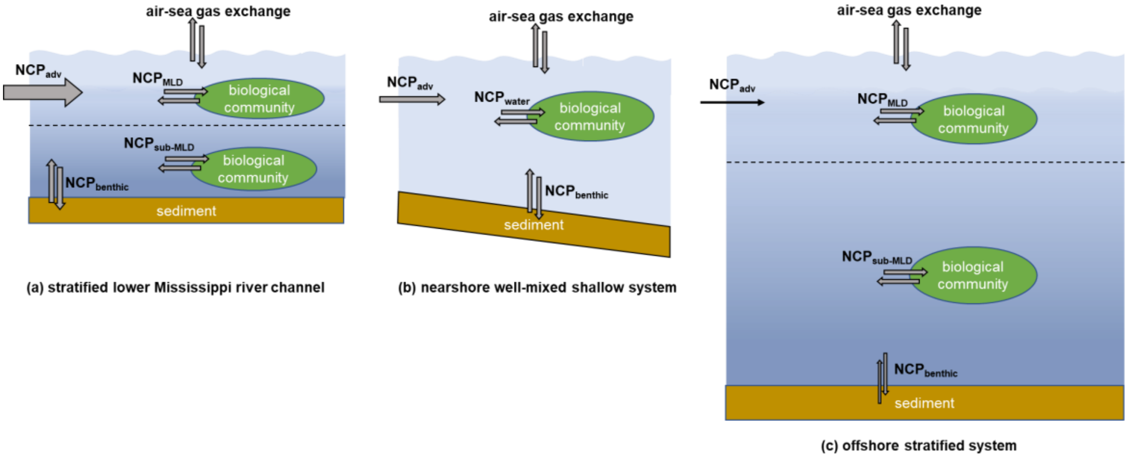


Figure 8 The differences in water column mixing conditions and $NCP_{O2Ar}$ estimation in the nGOM. The dotted lines in panels (a) and (c) indicate the mixed layer depth. In the stratified lower Mississippi river channel (a) and the offshore stratified system (c), $NCP_{O2Ar}$ reflected the combined 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 ($NCP_{adv}$).

**Review for manuscript bg-2019-88**

"Spring net community production and its coupling with the CO2 dynamics in the surface water of the northern Gulf of Mexico" by Jiang et al.

**Major comments:**

1. The manuscript will benefit greatly by going through a thorough revision on the English language. I am myself not a native speaker, but I can still identify many mistakes in the wording, spelling mistakes, punctuation, etc. I list some examples in the minor comments, but the mistakes are so many that it is impossible to correct all through this review. In this context, the authors also make use several times of subjective terms without giving quantities to justify, e.g. "moderate", "rapid", "deeper", "higher". These words should be avoided or accompanied by a quantity to reference the use of the adjective.
   Response: A through revision on the English language has been done by our coauthors who are native English speakers. Subjective terms have been avoided or accompanied by quantities as suggested.

2. The aims of the manuscript are not clearly stated. On the one hand, they aim to compare NCP estimates from four different methods, and on the other hand, they aim also to compare the relation NCP vs. $pCO_2$ in the area of study. But I think before aiming the second, they should clearly state early in the manuscript what is the purpose of comparing NCP from different methods? What is the gain and need of doing so?
   Response: The main purpose of this manuscript is to understand the spatial variability of NCP and $pCO_2$ in the nGOM and to investigate the relation between NCP and $CO_2$ flux. Previous NCP studies in our study region have been mainly based on the light/dark incubation and non-conservative changes in DIC and $NO_x$. To our knowledge, this work is the first attempt to

apply the $O_2$/Ar method in the nGOM. We thus compared the $O_2$/Ar result to those from the existing methods to evaluate the consistency of NCP estimates from various methods. Meanwhile, each of the methods have different advantages and disadvantages. By making NCP estimates using the different methods we can get a more robust understanding of the overall metabolism of the system. In the revised manuscript, the purpose of comparing NCP from different methods has been better described in the introduction section. We have also shortened the length of the discussion on NCP comparison to be more focused on discussing the NCP variability and the NCP-$CO_2$ flux relation.

3. The authors list in the discussion section (Pag. 13-14), mostly the disadvantages of applying the different methods for NCP determination. After reading all these disadvantages and limitations in each method, I see difficult to justify a comparison between these methods at all and making this as one of the main aims for this study. Further, the comparison between the four methods for NCP estimates was also done in only few stations. The authors finally compare the $NCP_{O2Ar}$ vs $pCO_2$ because both have high spatial resolution, hence proving that the methods comparison done in this work does not contribute substantially to the results presented in this work.
Response: As previous estimates of NCP for this shelf have mainly been based on light/dark incubations and non-conservative changes in DIC and nutrient, we think it is important to understand how the high spatial resolution $NCP_{O2Ar}$ and the existing methods compare. Similar comparison of NCP estimated from multiple approaches has been carried out by Ulfsbo et al. (2014) in the central Arctic Ocean. Even if large uncertainties are associated with different approaches and there was a large variability in NCP over our studied area, we found encouraging agreement among these methods showing elevated NCP rates in the plume regions. The comparison of different methods provides us with additional information about how the system works that is greater than the sum of the individual methods, especially for the regions where their results contradicted (e.g., in the lower Mississippi river channel and the high-turbidity Atchafalaya water). However, we agree with the reviewer that the current methods comparison was too lengthy. We have shortened the methods comparison in the revised manuscript to be more focused on the NCP variability and the NCP-$CO_2$ flux relation.

4. The methods section lacks of detail and scientific rigor in many parts:
   a) the authors do not show the vertical resolution of the sampled profiles, why they were not done at the same standard depths within each max. depth of the water column?
   Response: Bottom depth varied significantly in our study region from nearshore (a few meters) to offshore (a few thousand meters). As a result, discrete water samples were taken at 3-12 depths depending on the bottom depth and the vertical structures of temperature, salinity and $O_2$. We sampled standard depths at offshore stations with bottom depth deeper than 200 m. In the nearshore and shelf region, standard depths were preferred but sometime we collected samples at non-standard depths to better characterize the vertical variability. Meanwhile, this study focused on the surface water and the samples for NCP and $pCO_2$ were all collected within the MLD: samples for $NCP_{O2Ar}$ and $pCO_2$ were collected from the underway system at a depth of ~2.5 m, while the discrete samples used to derive $NCP_{DO\text{-}incub}$, $NCP_{\Delta DIC}$ and $NCP_{\Delta NOx}$ were collected

from the Niskin bottles at ~1.5 m.

b) No mention of duplicate or uncertainties analysis.

Response: Information on duplicate and uncertainties analysis has now been added for DIC, TA, DO, $pCO_2$, $O_2$/Ar and nutrients when available.

c) How often were the $pCO_2$ measurements calibrated? (it is only stated regularly)

Response: Clarified as "The $pCO_2$ measurement was calibrated twice daily against 3 certified gas standards (150.62, 404.72, and 992.54 ppm) and has a precision of 0.1 μatm and an accuracy of 2 μatm."

d) Overall the way they are written are all over the place and not rigorously written

Response: We have revised the Methods section thoroughly to improve scientific rigor.

e) There is no sufficient rigor on writing the equations, e.g. one should not include the units in the equation itself but rather in the text when explaining the variables.

Response: Equations have been revised as suggested.

5. The authors did not show vertical profiles to evidence their claim that most of the sampled water columns were well mixed. Also, they mention that there is a strong stratification due to buoyancy of the fresher river water plume above the oceanic shelf water. I find hard to believe that it is justifiable to assume steady state in the $NCP_{O2Ar}$ determination. At least, the contribution of horizontal processes into the shelf $O_2$ budget should have been investigated. I think the authors fall short here by simply assuming steady state, particularly after several works have proven in the past that physical contributions during this method must be considered at best. A great scientific contribution would be for the authors to provide an effort on quantifying the influence of horizontal processes into the NCP by $O_2$/Ar measurements.

Response: We thank the reviewer for pointing out the importance role of physical processes. The typical vertical profiles for well-mixed and stratified systems have now been added as supplemental figures (see Fig. S2, S3, S4 above). The influences of physical processes (lateral advection and vertical mixing) on the NCP estimation have now been discussed in the revised manuscript and are briefly summarized as follow:

The modeling study by Teeter et al., (2018) suggested that $NCP_{O2Ar}$ (estimated as the bioflux of $O_2$) accurately represents the exponentially weighted NCP over the past several residence times of $O_2$, which remains true under non-steady state conditions. As $NCP_{O2Ar}$ is an exponentially weighted moving average over time, it mostly reflects the in situ NCP rate and partly contains the NCP signal of the source water. The lingering memory effect of $O_2$ is generally weak due to the quick $O_2$ gas exchange. As a result, the influence of lateral advection on $NCP_{O2Ar}$ is commonly considered to be minor in most $O_2$/Ar studies and this applied well for the offshore regions in our study. However, the effect of lateral advection cannot be ignored in regions where riverine influence was significant and water residence time was short. For instance, the strong heterotrophic signals of the Mississippi and Atchafalaya river water can be transported to lower river channel and nearshore waters by the rapid lateral water transportation (Fig. 8a, b). In the revised manuscript, the heterotrophy in the lower Mississippi River channel is attributed to the lateral advection of $CO_2$-rich water from the upper river channel containing strong respiration signals. Note that in situ autotrophic production rates in the river channel were expected to be low due to the strong light limitation (Fig. S4e) and the high $NCP_{DO\text{-}incub}$ rates could be a result of overestimation caused by the sedimentation of particles in incubation bottles which alleviated light limitation (see the response to reviewer 1, minor comment 37).

Vertical mixing with low-DO subsurface water (e.g., upwelling) has been highlighted as the most important factor resulting in bias in $NCP_{O2Ar}$ (Castro-Morales et al. 2013; Nicholson et al. 2012; Shadwick et al. 2015; Teeter et al. 2018). In our study, $NCP_{O2Ar}$ well represented the NCP in the MLD in the stratified offshore waters as stratification prevented the occurrence of upwelling and as water residence time is sufficiently long to minimize the influence of lateral advection (Fig. 8c). On the contrary, $NCP_{O2Ar}$ in the well-mixed nearshore shallow waters reflected the combined result of the NCP in the water column, the signal crossed the water-sediment interface and the influence of the advection of heterotrophic river water (Fig. 8b). In the revised manuscript, the heterotrophy in the high turbidity Atchafalaya coastal water is attributed to the combined effects of the lateral advection of $CO_2$-rich Atchafalaya river water and the in situ water column and sediment oxygen consumption.

6. During the preparation of samples for the $NCP_{DO\text{-}incub}$, the authors mention that after initial measurement of DO, there was a compensation of volume in the incubation bottle by adding an extra volume of water. I find this problematic, by doing this there is introduction of DO from the new added water volume to the sample, hence it will change the initial measured DO conditions. By looking at the results of those 3 stations in the Mississippi river channel (results mentioned in P11, L21-23), it looks like while $NCP_{O2Ar}$ resulted in negative values, the $NCP_{DO\text{-}incub}$ showed positive values, and I wonder how much of that difference is rather the influence of the addition of DO by the volume compensation? Are those the same three points shown in Fig. 4c of the Mississippi plume with high $NCP_{DO\text{-}incub}$ values? Also, consistently $NCP_{DO\text{-}incub}$ is higher than $NCP_{O2Ar}$ also for the Atchafalaya plume. Indeed, incubation methods tend to bias the result due to a lack of homogeneity in the collected sample, and the authors should discuss these differences in the context of methods comparison. However, the introduction of a volume of water has another connotation, hence, I have no reason to trust the $NCP_{DO\text{-}incub}$ results and believe in these differences and going further discussing potential heterotrophy and autotrophy.

Response: The addition of DO was not corrected because the replacement volume (~ 3 ml) represents on average about 1% of the incubation volume (300 ml). Even in an extreme case, for example a DO concentration of 0 in the bottle and 200 mmol $m^{-3}$ in the replacement volume, the addition of the replacement water would change the DO in the bottle by 2 mmol $m^{-3}$, which is near the detection limit for this method of ~ 2 mmol $m^{-3}$ $d^{-1}$ (Murrell et al. 2013). The high values of $NCP_{DO\text{-}incub}$ reported in Fig. 4c are much greater than the potential bias introduced by the replacement volume. We have revised the method description with additional information along these lines beginning at P7, L20. The revision states: "After recording the initial saturation value, the probe was removed and the small volume (~ 3 ml) displaced by the probe was replaced with filtered seawater from an offshore, low nutrient site. The addition of any $O_2$ in the replacement water was considered negligible and in the worst case scenario, for example $O_2$ saturation concentration of 0 in the bottle and 200 mmol $m^{-3}$ in the replacement water, would only result in a change of ~ 2 mmol $m^{-3}$, which is approximately the same as the detection limit for this method of 2 mmol $m^{-3}$ $d^{-1}$ (Murrell et al. 2013). Another previous study with this method also found the correction was negligible

(Murrell et al. 2009).

7. Further, they argue that in the Mississippi river channel the $NCP_{O2Ar}$ showed heterotrophy which is dominated by benthic respiration, and results of $NCP_{DO\text{-}incub}$ showed autotrophy. While it is true that the method with $O_2$/Ar measurements integrates the results in the mixed layer, it is based in surface measurements (one point in the vertical column), just as in the $NCP_{DO\text{-}incub}$. Unless benthic respiration is truly proven, the negative NCP values can well be the result of turbulent horizontal or vertical mixing, hence encouraging the method to include physical factors.
Response: Many thanks to the reviewer for pointing out this mistake. A detailed examination of the data in the lower Mississippi river channel suggests that the water column was stratified rather than well-mixed (stations M1 and M2 in Fig. S4 above). Because of the stratification, benthic respiration cannot account for the heterotrophy in the surface water indicated by the negative $NCP_{O2Ar}$. The light/dark incubation experiment at stations M1 and M2 suggested that the low $O_2$ concentration cannot be explained by in situ community mechanisms (low respiration rates and positive $NCP_{DO\text{-}incub}$ values). The Mississippi fresh water in the upper river channel was strongly heterotrophic, which was resulted from the respiration supported by terrestrial carbon input and urban wastewater. In the revised manuscript, the heterotrophy in the lower Mississippi river channel was attributed to the lingering effect of the respiration signals of the heterotrophic river water from the upper river channel combined with short water residence time in the river channel.

8. Figure 5 – Did you plot yourself panels c and d? It looks like those are a plain zoomed copy of panels in a figure published in the work by Zahng et al. (2012), which is referenced correctly. The authors should only cite this reference and refer the reader to that citation, and specifically to that figure, for further details. It is not ok to plainly copy and paste here those previously published figures. This does not mean to reproduce a figure with previous data, which instead would mean that you use the original data and produce the figure again. As those panels seem to be a plain copy this action breaches copyrights and authors must avoid doing this. Hence, panels c and d on this figure should be completely removed. Also, Figure 5 is mentioned before than Figure 4, why not switching the order of these figures?
Response: This comment has been taken and we apologize for this mistake. The panels (c) and (d) in the original Fig. 5 have been removed. Also, we have switched the order of figures as suggested.

9. Figure 6 – The spatial interpolation shown in panels a to d is quite bias. Showing a map with only the transect results of the $NCP_{O2Ar}$, or the spatial interpolation for this result and $NCP_{\Delta NOX}$ and $NCP_{\Delta DIC}$ at best, but not a spatial interpolation for the very scarce $NCP_{DO\text{-}incub}$, where some structures in the spatial distribution of many places seem to be only an artifact of the interpolation, such as the large extent of the high NCP values in the Mississippi plume.
Response: Data in Fig. 6 are now shown as colored dots to avoid bias and artifact from interpolation.

**Minor comments:**

1. Between a quantity and its units there must be always a blank space, please revise this, especially for a number in percentage (e.g. 180 %, 10 m, 40 km, 28.5o N).
   Response: Corrected as suggested.

Abstract (P1)

2. L23 – remove "the spring season" and change to "during spring in 2017"
   Response: Corrected.

3. L23 – use same number of decimals in the degrees
   Response: Corrected.

Pag. 3

4. L1 – how much is "moderate salinities"
   Response: Corrected as "intermediate salinities (15 to 30 during this cruise)".

5. L4 – Photosynthetically Active Radiation
   Response: Corrected.

6. L18-19 this last sentence should be removed from here
   Response: Corrected as suggested.

Pag. 4

7. L8 – Precision AND accuracy?
   Response: Corrected as described in response to reviewer 1, minor comment 10.

8. L9 – mark this location in Fig. 1
   Response: These two sampling stations are now marked and more information is presented in the updated Fig. 1. Please see the response to minor comment 6 by reviewer 1 for the detailed description of the revision.

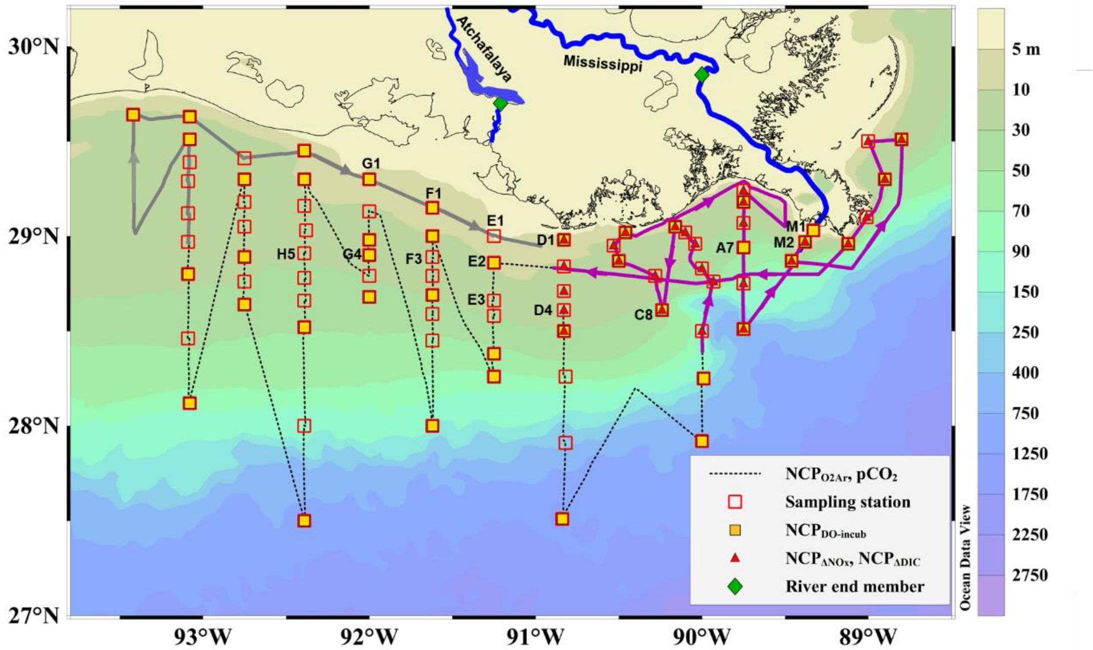


Figure 1 Map and sampling sites in the northern Gulf of Mexico during the April 2017 cruise. The black dotted line is the cruise track where high-resolution underway measurements were made. The track in the Mississippi plume (purple line, Apr. 8-10) and the Atchafalaya coast (gray line, Apr. 15-17) are 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).

9. L10 – DO **in discrete samples** was measured by a ….

   Response: Revised as suggested.

10. L18 – either you use the tilde symbol or explicitly write approximate

    Response: The tilde symbol has been removed.

Pag. 5

11. L5 – against the **surface** discrete

    Response: Revised as suggested.

12. L20 – where is this comparison of wind speeds shown?

Response: This sentence has been revised to now state "The COAMPS daily wind speed agreed well (mean difference = 0.4 m $s^{-1}$, **figure not shown**) with the measurements from the buoys in our study region". Because the $O_2$ and $CO_2$ fluxes were calculated from the same wind speed, using COAMPS data or buoy measurement didn't affect the discussion on the relationship between $O_2$ and $CO_2$ fluxes. Therefore, the comparison of wind speeds was not critical for our discussion and its figure was not shown in the manuscript.

13. L16, L20 and L25 – variables should be consistently written in italics (here and elsewhere)

Response: Corrected as suggested.

Pag. 6

14. L4 – why not referring to $O_{2meas}$ instead of $O_{2sea}$? You also measure in river waters! And also to keep consistency with $O_{2sat}$.

    L20 – in Eq. 3 again there is no consistency on the way the concentration of gases are expressed, while in Eq. 2 it was simply $O_{2sat}$ and $O_{2sea}$, here it is $[O_2]_{sat}$ and $[O_2]$, respectively. Please keep consistency.

    Response: Corrected. $[O_2]_{meas}$ and $[O_2]_{sat}$ are now consistently used in the revised manuscript.

15. L6 – instead of "observed seawater DO" change to "measured surface water DO"

    Response: Revised as suggested.

16. L7 – which T and S were used to calculated $O_{2sat}$?

    Response: This sentence has been revised so that it now states: "$[O_2]_{meas}$ is the seawater DO concentration from the underway optode measurements and $[O_2]_{sat}$ is the saturated DO concentration calculated from the measured sea surface temperature and salinity (Garcia and Gordon 1992)".

17. L20 – Current Eq. 4 should be shifted to be Eq. 3

    Response: Revised as suggested.

Pag. 7

18. L2 – here the authors need to better justify why in this region it is possible to neglect vertical mixing and lateral advection. They are important physical factors and later in the manuscript they claim it should be relevant to consider them. At least an effort should be done on explaining further why they were neglected.

    Response: We now discuss how the physical factors including vertical mixing and lateral advection affect the NCP estimation under different mixing conditions. Pleases see our responses to the general comment and major comments 5 and 7.

19. L4 - Equation 5 is of little use and is also wrong, the first term NCP should be removed because you are calculating NCP with the second term. I will completely remove it from the manuscript and use the term of the left in Eq. 6.

    Response: Although Eq. 5 was correct (NCP in Eq. 6 was calculated from Eq. 5 assuming $\mathrm{MLD}\frac{d[O_2]_{biol}}{dt}$ =0 and $\frac{[Ar]}{[Ar]_{sat}}$ =1) (Cassar et al. 2011; Jonsson et al. 2013; Kaiser et al. 2005), this explanatory equation has been now removed to make the method section more concise as suggested by both reviewers.

20. L15 – Is Eq. 9 correct? If you reduce GPP this equation is rather adding a factor to the high GPP value calculated in Eq. 8. Please check it.

    Response: The percent signs were absent in Equations 8 and 9 and they have been corrected as now read:

if %PAR ≥ 50%, $GPP_{Int}$ = GPP*MLD

if %PAR < 50%, $GPP_{Int}$ = 2*%PAR*GPP*MLD

21. L18 – in which depths the BOD bottles were collected?
    Response: The depth information has been added as: "The surface water samples **(~1.5 m)** were collected from Niskin bottles into triplicate clear and black 300-ml BOD bottles (Wheaton)."

22. L22 – filtered seawater also introduces DO into the sample, see my major comment above.
    Response: Please see the response to major comment 6.

Pag. 8

23. L3 – it is not sufficient to claim that there was no bias between the two methods, some values should be presented here.
    Response: Clarified as "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)."

24. L4 – the units of the DO rate of change are wrong
    Response: The units were correct here as the DO change rates during the incubation experiments (mmol $m^{-3}$ $d^{-1}$). These rates were then integrated over the MLD to produce the NCP rates (mmol $m^{-2}$ $d^{-1}$).

25. L16 – why it was chosen 50 % of light?
    Response: The text has been changed to answer this question: "Clear bottles were placed into boxes screened at 50% of ambient sunlight, which is a light level that permits maximum photosynthetic rates on this shelf (Lehrter et al. 2009). Please also see the response to reviewer 1, minor comment 28.

Pag. 9

26. L18 – "To facilitate **the** comparison, we converted NCP estimates from **the** different…."
    Response: Corrected.

Pag. 10

27. L5 – the lower discharge in 2017 is also observed in previous months, not only in April
    Response: This sentence has been revised to now state "The discharge in **spring** 2017 is slightly lower than the monthly mean value during 1997-2017".

28. L5 – "light"? please complete this to light transmittance
    Response: Corrected.

29. L9 – The authors claim a correlation between MLD and salinity, however by looking at Fig. 5 panels b and c, this is not evident. If the authors define MLD based on a potential density

criterion, they should make a comparison to density (i.e. incl. temperature which has more structure in surface waters) and not only to salinity.
Response: We agree with the reviewer that both temperature and salinity should be considered when discussing stratification. However, the salinity played a more important role in the formation of stratification in the plume regions because the temperature of river waters was lower than or similar with that of seawater. We have revised this sentence to now state "The MLD was generally shallow during our study period (2-11 m), especially **in the plume regions** where the stratification was mainly caused by the buoyancy of freshwater on top of the oceanic water (Fig. 3b, c)."

30. L15 – remove "in"
    Response: Corrected.

31. L17 – 19 this sentence will benefit by adding the correct punctuation
    Response: This sentence has been revised to now state "In spring when river discharge is high and wind is typically downwelling-favorable, the Mississippi River freshwater generally flows westward in a contained nearshore current".

Pag. 11

32. L2-4 too many subjective words without quantities or comparison in reference to something else (lower, deeper, higher?)
    Response: We were comparing the properties of water in the HTACW region to those in the Mississippi plume: "**In comparison to the Mississippi plume,** the HTACW was characterized by well-mixed water column, higher temperature, deeper MLD, lower light transmittance, lower DO% and lower $\Delta O_2/Ar$ (Fig. 3)."

33. L2-4 Whereas I agree in the observations made regarding the HTACW in the Atchafalaya Bay in Fig. 3, I disagree in the MLD which does not look homogeneously deeper in that region. From Fig. 3c, the surface water does not look well mixed as the authors claim in the following sentence (and vertical profiles are not presented). Therefore, this needs more investigation.
    Response: We appreciate the reviewer highlighting the need to display the vertical profiles. The HTACW region was, in fact, well-mixed as can be seen in the vertical profiles now provided in supplemental Fig. S2 in the revised manuscript.

34. L7-8 The authors define three sub-regions in Fig. 5 a and b based on the identified water characteristics, but they define them only by in their longitude limits and they should also include the latitude limits.
    Response: In comparison to latitude limits, we choose to present salinity limits for the two plume sub-regions and light transmittance limits for the high-turbidity Atchafalaya coastal water to better characterize the three sub-regions: "the Mississippi plume (to the east of 90.50° W, **salinity < 32**), (2) the high-turbidity Atchafalaya coastal water (90.50-92.30° W, **light transmittance < 20%**, named as HTACW hereafter), and the Atchafalaya plume (92.30-93.50° W along the coast, **salinity < 32**).".

Pag. 13

35. L9 – "spatial" instead of space

Response: Corrected.

Pag. 15

36. L1- associated with "the" different

Response: Corrected.

37. L7 – what inherent averaging the authors refer here? If they have continuous highly resolved data at least episodic extreme events would be better captured than discrete sampling.

Response: The modeling study by Teeter et al., (2018) suggested that the $NCP_{O2Ar}$ represents the exponentially weighted NCP over the past several residence times of $O_2$ (stated in the Methods section). The original expression of "episodic extreme events" was not appropriate and this sentence has been revised to now state "As $NCP_{O2Ar}$ is an exponentially weighted average rate over the past several residence times of $O_2$, $NCP_{O2Ar}$ is less able to capture **high NCP values** due to the inherent averaging of the $O_2$/Ar approach."

38. L12-14 this assumption of integrating vertically the $NCP_{O2Ar}$ can be avoided by considering vertical processes if the authors suspect this is the case (as mentioned in L17 same page). L12 – also, the authors contradict themselves in the structure of the water column, it is well mixed or not?

Response: The influences of vertical mixing on $NCP_{O2Ar}$ have now been better explained according to different mixing conditions. Please see the response to general comment and major comment 5.

39. L15 – "fraction"

Response: Corrected.

Pag. 16

40. L1 – vertical or horizonal mixing?

Response: As our study focused on the surface water, the mixing here was mainly horizonal mixing between the river water and seawater.

41. L10 – space between "community were"

Response: Corrected.

Pag. 17

42. L17 – before explaining the results in Fig. 9, please explain what it is plotted there.

Response: We agree that this is something that should have been included in the manuscript and have adjusted the manuscript accordingly. Please see our response to minor comment 60 by reviewer 1 for details of this revision.

Pag. 18

43. L3-10 these lines should be part of a methods section where the 1-D model is introduced to

the reader

Response: The model description has now been moved to the Methods section as suggested.

44. L11-25 I am not surprised by the results presented in Fig. 10, they are only showing the wellknown changes in the carbonate system, and this figure can only be seen as a proof of their model performance under standard defined initial conditions. I would place this figure in supplement.

    Response: We agree with the reviewer that the changes shown in this figure are well-known and this figure has been moved to the supplement. According to the suggestion from reviewer 1, we have updated this figure to show the differences in gas exchange rate and equilibrium time of $O_2$ and $CO_2$. Please see our response to minor comment 2 by reviewer 1 for details.

Pag. 19

45. L23 – " has the advantage of being "

    Response: Corrected.

Pag. 20

46. L3 – I miss the results on the distribution of nutrients. From the way the results were presented, this conclusion is not clearly supported

    Response: This sentence has been revised to now state "Along the river-ocean mixing gradient, the availability of light generally determines the onset of the biological growth which was mainly supported by river-borne nutrient loading."

Figures

47. Figure 1 – I miss some labels in the map. For people that is not familiar with this study region, it will be useful to add directly in the map the labels of the location of the main features that are mentioned throughout the manuscript, e.g., Mississippi and Atchafalaya deltas, Mississippi South Pass, Atchafalaya Bay. Also, add numbers to the stations and remove the units to all of the depths in the color bar, and rather add "m" above the bar as in Figure 3. The figure caption needs to be improved.

    Response: We have modified Fig. 1 to add more features according to the suggestions from both reviewers. Please see the response to minor comment 6 by reviewer 1 for a thorough explanation.

48. Figure 2 – This schematic contains extra information that is not a central part of the manuscript. If you are not talking about the actual biological pump and its components, I will remove it from the figure. I would also invert the order with the $CO_2$ and carbonate system on the left and the $O_2$ part on and the $O_2$ part on the right of the figure.

    Response: We agree with the reviewer that this schematic didn't contribute much and it has been removed from the revised manuscript. Instead, we now present a new Fig. 8 to show the differences in $NCP_{O2Ar}$ estimation according to the different mixing conditions. Please see our response to the general comment above.

49. Figure 4 – This figure needs to be georeferenced, or provide more information in Figure 1,

where was the start of the continuous transect? Also, you could add number of stations so it is clearer the geographical position of the points in panel c.

Response: Arrows have been now added in Fig. 1 to better show the direction of the transect and labels have been added for some key stations. Please see the response to minor comment 6 by reviewer 1 for details.

50. Figure S1 – add labels to the x-axis

Response: The label "month" has been added to the x-axis.

References

51. - The authors should carefully revise the guidelines for authors before submitting a manuscript to a journal. In this case, for the presentation of references, in the text they are always lacking of a comma between the authors and the year of the publication. Also, the format of the presentation of references at the end of the manuscript, should be also carefully checked (e.g. the year of the publication must precede the doi).

Response: Commas between the authors and the year of the publication were added in the in-text citations. However, placing the publication year at the end of the references is the requirement of the new format of the bibliography of BG.

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