**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:

- 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.

- 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?
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.
- 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?
  b) No mention of duplicate or uncertainties analysis.
  c) How often were the $pCO_2$ measurements calibrated? (it is only stated regularly)
  d) Overall the way they are written are all over the place and not rigorously written
  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.

- 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.

- 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.

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.

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?

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.

**Minor comments:**

- 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, 28.5° N).

Abstract (P1)
L23 – remove "the spring season" and change to "during spring in 2017"
L23 – use same number of decimals in the degrees

Pag. 3
L1 – how much is "moderate salinities"
L4 – Photosynthetically Active Radiation
L18-19 this last sentence should be removed from here

Pag. 4
L8 – Precision AND accuracy?
L9 – mark this location in Fig. 1

L10 – DO **in discrete samples** was measured by a ….
L18 – either you use the tilde symbol or explicitly write approximate

Pag. 5
L5 – against **the surface** discrete
L20 – where is this comparison of wind speeds shown?
L16, L20 and L25 – variables should be consistently written in italics (here and elsewhere)

Pag. 6
L4 – why not referring to $O_{2meas}$ instead of $O_{2sea}$? You also measure in river waters! And also to keep consistency with $O_{2sat}$.
L6 – instead of "observed seawater DO" change to "measured surface water DO"
L7 – which T and S were used to calculated $O_{2sat}$?
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.
L20 – Current Eq. 4 should be shifted to be Eq. 3

Pag. 7
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.
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.
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.

L18 – in which depths the BOD bottles were collected?
L22 – filtered seawater also introduces DO into the sample, see my major comment above.

Pag. 8
L3 – it is not sufficient to claim that there was no bias between the two methods, some values should be presented here.
L4 – the units of the DO rate of change are wrong
L16 – why it was chosen 50 % of light?

Pag. 9
L18 – "To facilitate **the** comparison, we converted NCP estimates from **the** different…."

Pag. 10
L5 – the lower discharge in 2017 is also observed in previous months, not only in April
L5 – "light"? please complete this to light transmittance
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.
L15 – remove "in"
L17 – 19 this sentence will benefit by adding the correct punctuation

Pag. 11

L2-4 too many subjective words without quantities or comparison in reference to something else (lower, deeper, higher?)
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.
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.

Pag. 13
L9 – "spatial" instead of space

Pag. 15
L1- associated with "the" different
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.
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?

L15 – "fraction"

Pag. 16
L1 – vertical or horizonal mixing?
L10 – space between "community were"
Pag. 17
L17 – before explaining the results in Fig. 9, please explain what it is plotted there.

Pag. 18
L3-10 these lines should be part of a methods section where the 1-D model is introduced to the reader
L11-25 I am not surprised by the results presented in Fig. 10, they are only showing the well-known 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.

Pag. 19
L23 – " has the advantage **of** being "

Pag. 20
L3 – I miss the results on the distribution of nutrients. From the way the results were presented, this conclusion is not clearly supported.

**Figures**

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.

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 the right of the figure.

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.

Figure S1 – add labels to the x-axis

**References**

- 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).