Interactive comment on "Implementation and assessment of a carbonate system model (Eco3M-CarbOxv1.1) in a highly-dynamic Mediterranean coastal site (Bay of Marseille, France)" by Katixa Lajaunie-Salla et al.

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Lajaunie-Salla and coworkers present an extension of an existing food-web model with a carbonate chemistry balance. They subsequently use this model to look at the carbonate dynamics in the Bay of Marseille, and use sensitivity test to find under what circumstances the coastal waters of the BoM could be a source or sink for CO_2. Overall I found this an interesting study, even though its focus is very local. Depending on the flexibility, this model could likely also be used on different coastal sites.

I do have a few issues with the manuscript in its current state that I feel need to be resolved before it can be published. First and foremost, I had troubles to understand the model set-up. Given that this is a model development journal, the model should be clearly articulated in the main text, and this is not the case.

Secondly, I am not convinced by the authors discussion of the disagreement between model and data. Because they are using the model to look at dynamics in carbonate chemistry, they should either be able to reproduce the data in a better way, or at the very least discuss in more detail why there is a disagreement, and why that is not a problem. I do however think these issues can be dealt with in a revised version, and find the study itself valuable.

Reply: We thank the reviewer for the positive evaluation of our work to be published in GMD journal. We thank too him for the detailed and useful comments that contributed to greatly improve the manuscript.

This study is focused on the Bay of Marseille (BoM) that harbors the big metropolis of the aforementioned town hosting more than 800 000 inhabitants, with in summer an increasing of tourism activities. Moreover, the BoM is impacted by many harbor activities. In this context, we think so that the main results of our study could be transposed to other coastal sites that are also impacted by urban and anthropic pressures.

We modify the conclusion section of manuscript L429-441 as following:

"*The BoM biogeochemical functioning is mainly forced by wind-driven hydrodynamics (upwelling and downwelling), urban rivers, wastewater treatment plants, and atmospheric deposition (Fraysse et al., 2013). In addition, Northern Current and Rhone River plume intrusions frequently occurred (Fraysse et al., 2014; Ross et al., 2016).* ***Moreover the Bay of Marseille harbors the second bigger metropolis of France (Marseille) that is impacted by many harbor activities. The next step of this study will be to couple the Eco3M-CarbOx biogeochemical model with a 3D hydrodynamic model that*** *will mirror the complexity of the BoM functioning. In this way, the contributions of hydrodynamic, atmospheric, anthropic, and biogeochemical processes to the DIC variability could be determined, and an overview of the air-sea CO_2 exchange could be made at the scale of the Bay of Marseille.* ***The main results of our study could be transposed to other coastal sites that are also impacted by urban and anthropic pressures.*** *Moreover, in this paper we highlighted that fast and strong variations of pCO_2 values occur, so thus it is essential to acquire more in situ values at high frequency (at least with an hourly resolution) to understand the rapid variations of the marine carbon system at these short spatial and temporal scales.*

As mentioned by the reviewer we give more details about the model set-up and the modifications made from the previous version. Moreover, we propose to change the figure 3 of the previous manuscript by the figure that compares model and *in situ* values without any mean. This figure shows better the good reproduction of the in situ data by the model.

Comments:

1. Model Description:

I do not think the model development or set-up has been well described in the text. All equations and parameters are to be found in a number of tables of the appendix, and the readers are expected to either know the plankton model used, or go to other papers to find it. This might be fine if it was an established model and in a different journal, but I do not think it is good for GMD. The reasoning behind model set-up and parameterization is not explaining in the text, so it is difficult to understand why the model was set-up as it was. After reading the methods I still had a number of basic questions;

Reply: The Eco3m-CarbOx model is based on a pre-existing model of the plankton ecosystem developed by Fraysse et al. (2013). The model presented in our study includes a set of new developments and improvements in the realism of the plankton web structure and process formulations. In order to improve the representation of chlorophyll concentration in the Bay of Marseille, two types of phytoplankton were added: the *Synechococcus* cyanobacteria, which is one of the major constitutive members of picophytoplankton in Mediterranean Sea (Mella-Flores et al., 2011), and the large diatoms, which are generally observed during spring blooms at mid-latitudes (Margalef, 1978). The functional response of primary production was modified using another formulation of temperature limitation function which takes into account the optimal temperature of growth for each phytoplankton (diatoms and picoplankton). The exudation of phytoplankton was modified taking into account the intracellular phytoplankton ratio of C, N and P. For the assimilation of matter (inorganic and organic) by bacteria and the remineralization processes the dependence on intracellular bacteria ratio was added. A temperature dependence of all biogeochemical processes was added. Also certain parameters in some formulations were modified. The Eco3m-CarbOx model also hosts now a carbonate system module. For this three processes were added (i) the precipitation, (ii) dissolution of carbonate and (iii) the gas exchange with the atmosphere. We agree with the reviewer on this point and we added some important elements on the approach of alteration of the pre-existing model. However, we think that, for a sake of clarity, it is important to keep in the appendix section the tables including source-sink equations and the long lists of model parameters.

We modify a part of the "Numerical model description" section L121-143 as following:
*"**The model presented in this study includes a set of new developments and improvements in the realism of the plankton web structure and process formulations. In order to improve the representation of chlorophyll concentration in the Bay of Marseille The phytoplankton is divided in two groups:** one with traits of the Synechococcus cyanobacteria, which is one of the major constitutive members of pico-autotrophs in Mediterranean Sea (Mella-Flores et al., 2011), and another with traits of large diatoms, which are generally observed during spring blooms at mid-latitudes (Margalef, 1978). For both of the phytoplankton, there is a diagnostic chlorophyll-a variable related to the phytoplankton biomass in carbon, the phytoplankton N-to-C ratio, and the limiting internal ratio f_Q^N (Faure et al., 2010; Smith and Tett, 2000; Tab. A2). **The functional response of primary production was modified using another formulation of temperature limitation function which takes into account the optimal temperature of growth for each phytoplankton. The exudation of phytoplankton was***

modified taking into account the intracellular phytoplankton ratio. For the uptake of matter by bacteria and remineralization processes the dependence on intracellular bacteria ratio was added. A temperature dependence of all biogeochemical processes was added to take into account the effects of rapid and strong variations of seawater temperatures on plankton during episodes of upwelling for instance that are usually often observed in the BoM. Also certain parameters in some formulations were modified owing to the alterations of some formulations.

*Additionally****, a carbonate system module was developed and*** *three state variables have been added: dissolved inorganic carbon (DIC), total alkalinity (TA) and the calcium carbonate ($CaCO_3$) implicitly representing calcifying organisms. The knowledge of DIC and TA allows the calculation of the pCO_2 and pH diagnostic variables, necessary for resolving all the equations of the carbonate system. These equations use apparent equilibrium constants, which depend on temperature, pressure, and salinity (Dickson, 1990a, 1990b; Dickson and Riley, 1979; Lueker et al., 2000; Millero, 1995; Morris and Riley, 1966; Mucci, 1983; Riley, 1965; Riley and Tongudai, 1967; Uppström, 1974; Weiss, 1974).* ***For this module three processes were also added: the precipitation and dissolution of calcium carbonate and the gas exchange of pCO_2 with the atmosphere.****"*

(i) what are the dimensions of the model (1 box, 3D, : : :)?

Reply: The spatial dimension of the model is 0 in this study. The state variables of the model only change along time. We add this information in L113-115, as following:
"*In this study, the state variables of the Eco3M-CarbOx model only change along time (i.e. usually termed "model 0D")*, are representative of the time evolution of a sea surface water cell *but this biogeochemical plankton model is not coupled with a hydrodynamic model."*

(ii) It is stated at L153 that the variables were initialized at winter conditions and forced with measured temperature etc. Does it not require a spin-up for the circulation – that is, presuming it has circulation?

Reply: We agree that this information was not clear in our previous version of the MS. As in this work the biogeochemical model is not coupled with a hydrodynamic model, the circulation is not taking into account. We add this information in L113-115, as following:
"*In this study, the state variables of the Eco3M-CarbOx model only change along time (i.e. usually termed "model 0D")*, are representative of the time evolution of a sea surface water cell *but this biogeochemical plankton model is not coupled with a hydrodynamic model*."

(iii) Why do you choose two three-day periods of wind speed? Why 7 m s^{-1}? What are the boundary conditions? How does the BoM connect to the rest of the Mediterranean?

Reply: As we can see in the figure below, which represents the time series of wind speed at the SOLEMIO station located in the BoM, the studied area is highly impacted by windy periods with speed values above 20 m s^{-1}. In 2017 the average wind speed was 7 m s^{-1}. In order to analyze the impact of these windy periods on the carbonate system we run the model with wind speed of 20 m s^{-1} during three days. We choose three-day periods because the BoM is often impacted by short bursts of Mistral. This wind usually blows for a few days with high speeds ranging from 14 to 28 m s^{-1}.

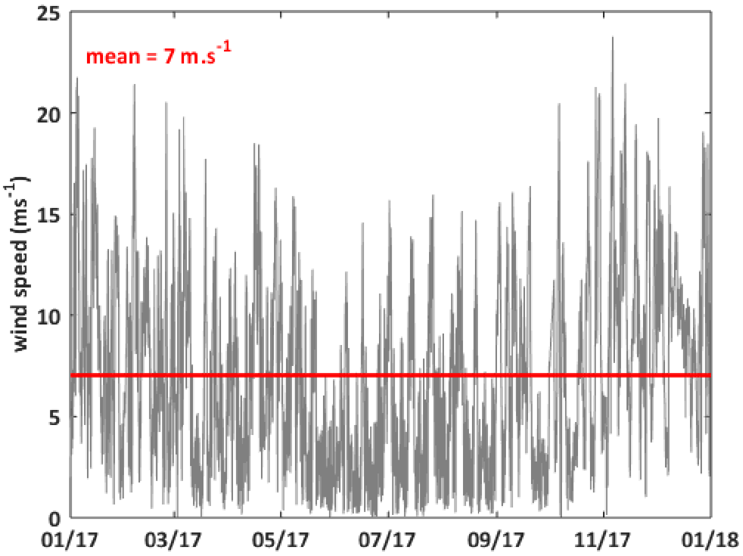

Figure A: Time series of wind velocity (m s^{-1}) at SOMLIT station between July 1st and August 1st, 2017 (gray line) and the average of wind velocity (red line).

We modify the manuscript L182-185 as following:
"Impact of wind events: a first simulation S2 was run with a constant wind intensity of 7 m s^{-1} (2017 annual average wind speed) throughout the year and a second one (S3) with two three-day periods of strong wind speed (20 m s^{-1}) representative of short bursts of Mistral (data not shown) starting on May 15th and August 15th, and a constant value of 7 m s^{-1} the rest of the year."

Furthermore, there seems to be a number of inconsistencies between tables and between tables and text, for example;
(i) In Table A3 you use 'POM' for the bacterial grazing term, but that does not show up in your state variable list (it is likely detritus, but then it should be called that, otherwise it causes confusion).

Reply: We agreed with the reviewers to modify in the manuscript the term "detritus" by "DPOM" to define the detrital particulate organic matter. In same way we modify the term dissolved organic matter "DOM", by the labile and semi labile dissolved organic carbon "LDOM". In the manuscript these changes were made in table A1-A2 and A3 and in L19 and– L118.

(ii) (O/N)nit in Table A2 -> (O/C)nit in Table A5, (O/N)uptNO3 in Table A2 -> (O/C)uptNO3 in Table A5 :

Reply: We agreed with the reviewer that there are several mistakes on the tables A2 and A5. In this study, the ratio $\frac{O}{C}$ and $\frac{O}{N}$ are equal to 1 and 2, respectively. There is not need to differentiate the name of this ratio for each process. We choose then to use only the ratio $\frac{O}{C}$ and $\frac{O}{N}$ without sub-index, we then modify the tables A2 and A5 considering these changes:

$$\left(\frac{O}{C}\right) = \left(\frac{O}{C}\right)_{PP} = \left(\frac{O}{C}\right)_{resp} = \left(\frac{O}{C}\right)_{respZ} = \left(\frac{O}{C}\right)_{respBa} = 1$$

$$\left(\frac{O}{N}\right) = \left(\frac{O}{N}\right)_{uptNO_3} = \left(\frac{O}{N}\right)_{nit} = 2$$

It is stated at L138 there is closure term, but I did not find where the grazing term closes the balance?

Reply: In the model, all the matter grazed by the zooplankton, (i.e. bacteria, phytoplankton, and DPOM) return as either organic or inorganic matter by excretion, egestion and mortality processes (Fig. B).

- The excretion of zooplankton returns dissolved inorganic matter with a flux of :

$$\boldsymbol{R_{excr}^{DIM}} = \varepsilon_{DIM} \cdot d_X \cdot (1 - k_{X,zoo}) \cdot (R_{Gr}^{Phy} + R_{Gr}^{DPOM} + R_{Gr}^{Ba})$$

- The excretion of zooplankton returns labile dissolved organic matter with a flux of :

$$\boldsymbol{R_{excr}^{LDOM}} = (1 - \varepsilon_{DIM}) \cdot d_X \cdot (1 - k_{X,zoo}) \cdot (R_{Gr}^{Phy} + R_{Gr}^{DPOM} + R_{Gr}^{Ba})$$

- The egestion of zooplankton returns detrital particulate organic matter with a flux of :

$$\boldsymbol{R_{pf}} = (1 - d_X) \cdot (R_{Gr}^{Phy} + R_{Gr}^{DPOM} + R_{Gr}^{Ba})$$

- The mortality of zooplankton returns detrital particulate organic matter with a flux of :

$$\boldsymbol{R_m} = d_X \cdot k_{X,zoo} \cdot (R_{Gr}^{Phy} + R_{Gr}^{DPOM} + R_{Gr}^{Ba})$$

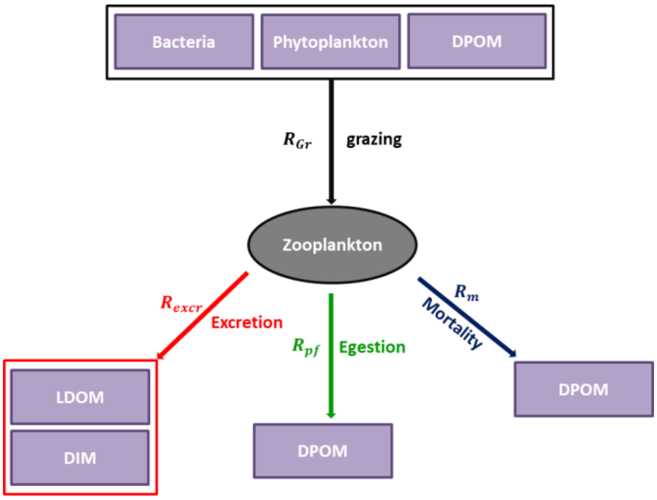

Figure B: Repartition of matter grazed by zooplankton

The sum of these four processes is equal to the flux of matter grazed by zooplankton:

$$\boldsymbol{R_{excr}^{DIM}} + \boldsymbol{R_{excr}^{LDOM}} + \boldsymbol{R_{pf}} + \boldsymbol{R_m} = R_{Gr}^{Phy} + R_{Gr}^{DPOM} + R_{Gr}^{Ba}$$

This is the way whom the biogeochemical model is closed in this study.

2. Model-data agreement:

You discuss the model-data comparison in section 3.1, but seem to brush over some of the misfits quite easily. For example, the alkalinity in Fig.3

- you say the model results are within the range of the data, but they are only barely within the range, and most of the data plots above the model values.

Reply: We agreed with the reviewers and modify the sentence L246-248 as following: "*The biogeochemical model provides almost constant values around 2570 µmol kg^{-1} all along the year, which is lower than in situ data.*"

- At L208 you say that the model shows the same trends for pH and pCO_2, but you have a consistent offset in the first half of your pCO_2 graph? And the trends seem to be inverse in the first half (blue dots going up and orange down) and last part (orange up and blue down) of the graph. Same for the pH (which is what you would expect as they are coupled) I would think that if you want to use the model to investigate carbonate dynamics under future climate change scenarios, you would want to be able to reproduce (or explain) what happens with the alkalinity and pH? Those model-data comparison does not give much confidence to the model (or parametrization) if you specifically want to address carbonate related questions.

Reply: We agree with the reviewer that in the figure 3 we cannot see well the variability of pCO_2 and pH, because the plotted data from model is a mean value at ±5 days around the sampling date. Looking the figure C below, that represents the simulated data and *in situ* data, we can see that the model reproduces the dynamic of seasonal variations of seawater pCO_2 and pH.

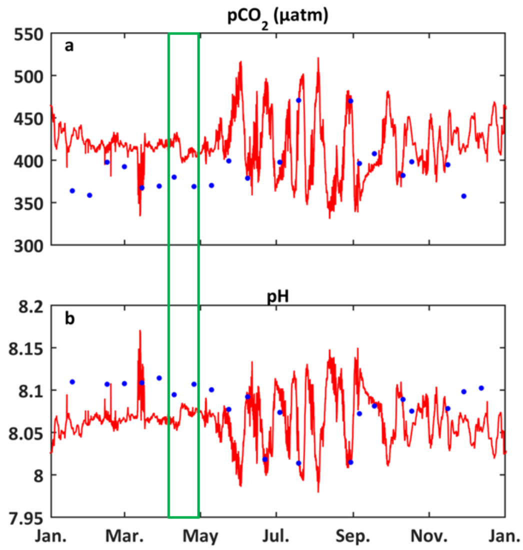

Figure C: Comparison of model results (red) and in situ data (blue) at the SOLEMIO station. (a) pCO_2 (µatm) and (b) pH

From January to February, the model reproduces the observed decrease of pCO_2 and from February to March the increase of pCO_2. In mid-April the observed drop of pCO_2 and increase of pH are also spotted in the model (Fig.C green box). During summer, the dynamics linked to the temperature variations are also well reproduced by the model. We propose to change the Figure 3 of the previous manuscript by the figure that compares model and *in situ* values without any mean.

We modify the manuscript L223-233 as following:
"On the whole, the seasonal ***variations of the seawater pCO_2 are correctly simulated by the biogeochemical model (Fig. 3B), even if the values are slightly overestimated during MWC period. From January to February, the model reproduces the observed decrease of pCO_2 and from February to March the increase of pCO_2. In mid-April, during the simulated spring bloom period, the observed drop of pCO_2 and increase of pH are also spotted in the model (Fig.3C & E).*** *The model especially succeeds in reproducing the observed increase in relation to high temperatures during the SWC period. The reduction of the CO_2 solubility due to thermal effects mostly explains the increase in pCO_2 during the SWC period. The strong standard deviation of modeled values during the SWC period can be explained by the rapid changes in temperature due to upwelling occurring at this time of the year. The range of modeled pCO_2 values (345 - 503 µatm) encompasses the range of observation values (358 - 471 µatm; Tab. 2).* ***The statistical analysis calculated*** *a mean bias of +23 µatm, and a WSS value of 0.69 (Tab. 2)."*

3. General readability:

In general, the text reads a bit awkward, and seems to have a strong French influence (I mean no offense, but that is just the way it felt when I was reading it). In particular oddly placed articles, and plural forms where it should be singular. The manuscript could probably benefit from proofreading by a native speaker. Then again, I might be wrong as I am also not a native speaker, and this is merely a suggestion.

Reply: Before submitting the new revised version, we will give it to a native speaker for proofreading and English improvements.

Minor comments:

L29: ‘strong atmospheric CO2’ -> do you mean high concentrations?

Reply: We agree with the reviewer and changed “strong” by “high” in L29.

L41: are you considering the biological pump to be a physical process?

Reply: In fact the sentence no was clear, we modified as following L41:
“In the ocean, the main processes regulating CO_2 exchanges between the atmosphere and sea are the solubility pump and the biological pump at different time scales”

L47: ‘organic matters’ -> organic matter

Reply: We corrected as suggested L47.

L143: just ‘dataset’ suffices

Reply: We corrected as suggested L158.

L149: Why did you not plot the temporal trend versus the datapoints?

Reply: As the *in situ* data are recorded every 15 days, and in order to do a statistical analysis and have the same number of data we used the mean value at ±5 days around the sampling date. In the figure D below, we plot the temporal trend versus the datapoints without any mean.

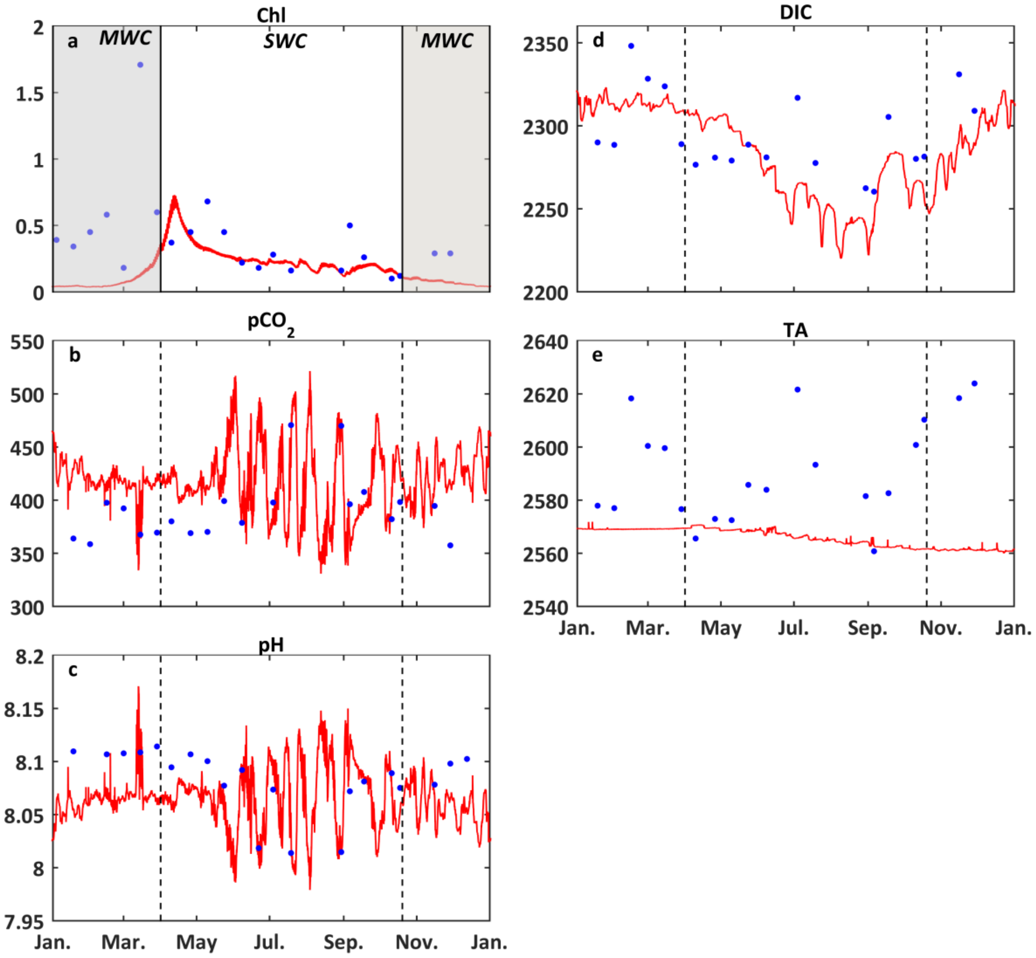

Figure D: Comparison of model results (orange) and in situ data (blue) at the SOLEMIO station. (a) Chlorophyll-a concentrations (mg m^{-3}), (b) pCO_2 (µatm), (c) pH, (d) DIC (µmol kg^{-1}) and (e) TA (µmol kg^{-1})

As mentioned before, we propose to change the Figure 3 of the previous manuscript by this figure that compares model and *in situ* values without any mean.

L236: Why is the flux not expressed in a mol per unit area value?

Reply: In our study as the model run only along time there is not an area dimension, then the flux is expressed in mmol m^{-3} d^{-1}. All biogeochemical processes are expressed in mmol m^{-3} d^{-1}.

L248: There is not really a decrease of seawater pCO_2, it just becomes much more variable

Reply: Here we wanted to highlight the impact of primary production on the seawater pCO_2. Looking at the figure E below (Fig. EC, black line), we can see net decreases of DIC (-10 mmol kg^{-1}) and seawater pCO_2 (-20 µatm) correlated with the increase in chlorophyll concentration. Then we prefer to do not change the sentence and to keep as written in the previous version of the manuscript.

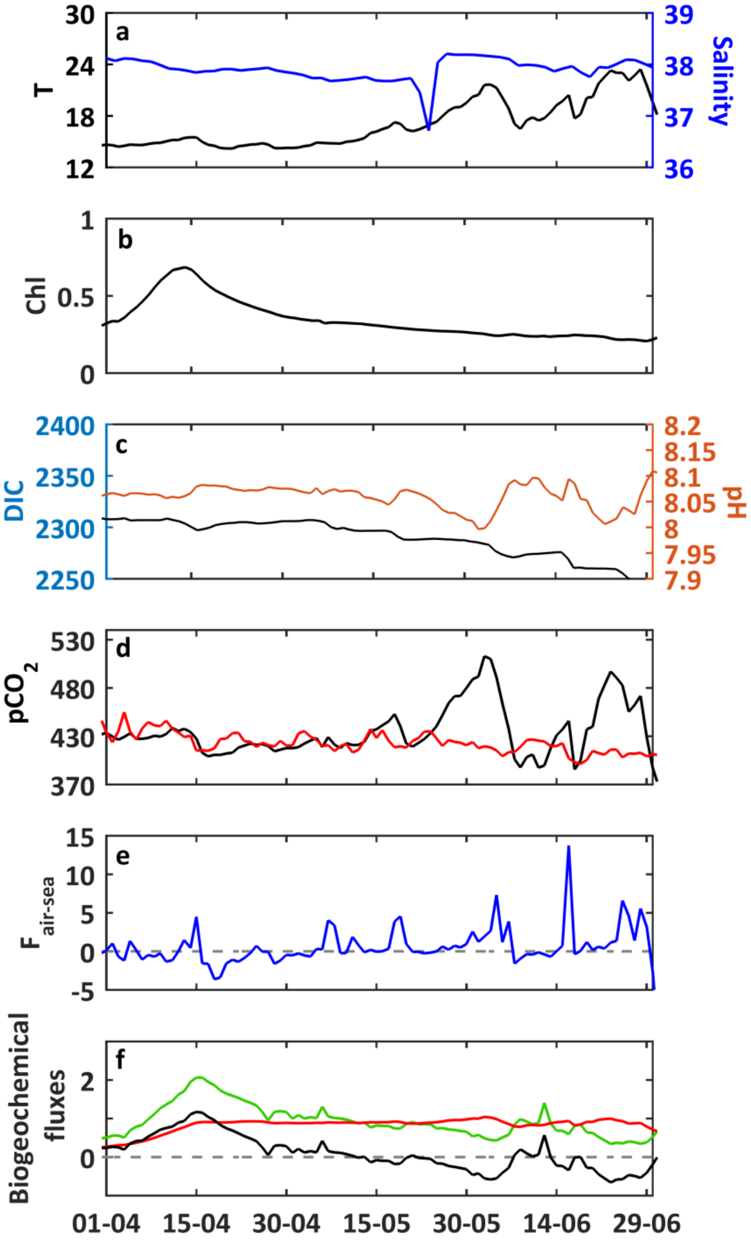

Figure E: Temporal focus between April 1st and July 1st, 2017. In situ daily average of (a) temperature (°C, black line) and salinity (blue line) at the SOLEMIO station. Modeled daily average, (b) chlorophyll concentrations (mg m^{-3}, black line), (c) DIC (µmol kg^{-1}, black line) and pH (orange line),(d) seawater pCO_2 (µatm, black line) and atmosphere pCO_2 from OHP (µatm, red line), (e) air-sea CO_2 fluxes mmol m^{-3} d^{-1}), (f) Gross Primary Production (mmol m^{-3} d^{-1}, green line), total respiration (mmol m^{-3} d^{-1}, red line) and Net Ecosystem Production (mmol m^{-3} d^{-1}, black line).

For sake of clarity, this figure could be added in the annex of the manuscript or in Figure 4 (on the right) of the manuscript.

L282: 'farer' -> further ?

Reply: Yes, we agree with the reviewer we modified the sentence as suggested L301.

L304: how can it affect the spring bloom before the nutrients are supplied?

Reply: In fact, this sentence needs to be corrected. Here we wanted to highlight, that the input of nutrients from river favors the primary production, and then the bloom of phytoplankton will occurs earlier than the reference simulation, which does not take into account rivers inputs. We modified the sentence as following L322-324:

"It can be noted that with the strongest river supply at mid-March (Figs. 7A & 7B), the occurrence of the spring bloom is earlier (Fig. 7C) than that occurring in the reference simulation (S0)."

L359: It sounds contradictory to say the 1.5°C rise affects the carbonate system little, but at L350 that the system is mostly driven by temperature variations

Reply: In winter the seawater temperature is around 13°C and in summer is around 20°C. However, in summer the upwelling events drop temperature by more than 5°C which have a strong impact on the carbonate system. In case of the scenario of a temperature rise of 1.5°C, we increase the time series of temperature by 1.5°C all along the year. This increase being smaller than the variations of temperature that occurs during upwelling events, the impact of +1.5°C over the year on carbonate system is less significant.

L363: double set of citations

Reply: We corrected this mistake.

Figures: give the legend in the figure panels instead of in the caption

Reply: We think that to insert the legend in the caption will overload the figure and will be unreadable. We decide then to keep the legend figures in the caption.

Fig. 6: what are those weird spikes in the air-sea gas exchange curve?

In figure 6, the two spikes in the air-sea gas exchange occur when the wind reaches the maximum velocity (20 m s^{-1}). Between these two spikes, the pCO_2 of seawater and air are balanced then the air-sea gas exchange is null.