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Discussions

Interactive comment on "Factors controlling pCO_2 variability in the eastern Gulf of Cádiz (SW Iberian Península)" *by* Dolores Jiménez-López et al.

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Reviewer 1:

The authors are very grateful for your constructive comments and suggestions on the previous version. We think that the manuscript has been significantly improved thanks to all the contributions made. Below you will find the comments you made and our comments as authors (marked AC) on each point. In response to all the comments the manuscript has been modified, resulting in changes to line numbers. Therefore, we have included the new line numbers (whenever applicable) so that you can refer to either the current or (former) version if you wish.

Thank you very much for your consideration. Sincerely, Dolores Jiménez-López, on

behalf of all co-authors.

Jiménez-López et al. discuss spatio-temporal variability of pCO2 in the Gulf of Cádiz based on a new dataset collected between March 2014 and February 2016. These results will eventually help to better understand carbon cycle processes on continental shelves and their contribution to the global carbon cycle. And although the authors discuss many different and interesting local features, the submitted manuscript lacks clarity and should be revised and restructured before publication.

*General comments

The separation of the driving mechanisms of pCO2 into temperature and biological effects follows the line of argumentation of Takahashi et al. (2002), however, the authors actually calculate thermal and non-thermal components of pCO2 (e.g. Landschützer et al. (2015)). This is also stated by the authors themselves (Line 161 or 372), but not implemented or followed in their discussion, which is a consequence of the fact that the wording in the method section 2.3 is almost identical to the description of the method in Takahashi et al. (2002) (but not cited as such). Especially in the continental shelves, complex interactions of air-sea gas exchange, primary production, lateral and vertical transport, entrainment of high-DIC waters from below, anthropogenic runoff and freshwater addition lead to changes in salinity, DIC and alkalinity and thereby affect the non-thermal trend in pCO2. Moreover, the authors show seasonality, which they attribute to temperature and biological effects only, while at the same time, they discuss how, e.g., river runoff changes in magnitude over the year and thereby affects pCO2. Although the authors present many different drivers of pCO2 variability, they go back in the temperature-biology framework, which is inconsistent and difficult to follow.

The discussion section needs to be restructured accordingly. First, only results of the 8 cruises should be interpreted without repeating the results. Second, the results should be put in context with previous studies that took place in the same study area and its vicinity; here, it is crucial to include the reference years and seasons. And last, the

findings for the Gulf of Cádiz may be compared to other continental shelf areas in the North Atlantic and globally. At the moment, the authors list many results, in the result and discussion section, with little interpretation and no clear line of argumentation that leads to the presented conclusion.

AC: Thank you very much for your suggestions. We have changed the terms "temperature and biological effects" to "thermal and non-thermal" components of pCO2. In addition, we have determined the contribution of SST, air-sea CO2 exchange and mixing plus biological processes to the changes of pCO2 using the Olsen et al. (2008) method (suggested by the reviewer 3). This quantification appears in the Material and methods (Line 174-191) and Discussion sections (Line 374-391) and a new figure has been added (Fig. 7). The Discussion section has been modified following your suggestions. Firstly, some Discussion paragraphs are moved to the Results section (Line 223-229 and Line 244-249). Second, in the Discussion, the results are considered in the context of previous studies of the same area (including references to years and seasons) and then in the context of studies of other continental shelf areas. And last, Table 4 has been removed, and Table 5 is now Table 4, in which only studies of the Gulf of Cádiz are included.

*Specific comments

-Line 74: If previous studies have already determined the sink strength of the Gulf of Cádiz, seasonally driven by temperature and biology, what is the added value of your study? I am missing a clear motivation for this manuscript in the introduction.

AC: Thank you very much for your suggestion. We have included the following in the text to explain the added value that our study provides: Line 73-77: "It has also been possible to estimate the influence that various sea surface currents have on pCO2 variability, since this study considers deeper areas than previous works. Therefore, we can analyse the change that has occurred in relation to the CO2 uptake capacity in the Gulf of Cádiz in the last 10 years, in comparison with other studies that analyse the

seasonal variation underway of pCO2 in this area (Ribas-Ribas et al., 2011)".

-Line 84 / Figure 1: Add bathymetry. Add position of river Guadalete and the tidal creeks and the position of cape San Vicente. You may want to add a circulation scheme here that would help to visualise the surface circulation here.

AC: We followed your suggestions and edited Fig. 1. However the tidal creek named "River San Pedro" could not be added due to its small dimension compared to the rivers Guadalquivir and Guadalete.

-Line 110: Do the transects cover different water masses or circulation features? AC: Yes, the shallower stations of the different transects are influenced by the Gulf of Cádiz Current and the deeper stations (about 300 m approximately) by the Azores Current. In Fig. 1 the circulation scheme of the study area is illustrated. Line 133: How do you correct the temperature difference?

AC: Corrections between the equilibrator and SST were made following the method of Takahashi et al. (1993). The sentence has been edited in the text. Line 127-129: "The xCO2 was converted into pCO2 according to the protocol described in DOE (2007). Corrections between the equilibrator and SST were made following Takahashi et al. (1993). The temperature difference between the ship's sea inlet and the equilibrator was less than 1.5 °C".

-Line 144: How was the oxygen sensor calibrated? Confusing to first explain how AOU is derived, without a detailed description of how oxygen values were determined.

AC: This point is now clarified and added in the text. Line 134-138: "Dissolved oxygen values were obtained with the sensor of the rosette (SeaBird 63) pre-calibrated using Winkler titration ($\pm$0.1 μmol L-1) of samples collected from several water depths at selected stations (Parsons et al., 1984). Apparent Oxygen Utilization (AOU) was determined as the difference between the solubility calculated applying the expression proposed by Weiss (1974) and the experimental values of dissolved oxygen".

-Line 150: Why unit "mile"? Why is exactly 0.5 mile chosen? What is the distance between two stations? Particularly in the SP section, could there be an overlap in pCO2 data for calculating the mean? If there is a CTD coupled to the rosette-sampler, why are not discrete SST and SSS data used for each station and compared / evaluated to the underway SST / SSS measurements?

AC: We have used the unit “mile” to facilitate our estimation of transit times between the sampling stations. The mean distance between stations was set at 5 miles from the beginning, and then 0.5 mile constitutes approximately 10% of this distance. The mean pCO2 of the study was not calculated using the discrete values, but by the underway measurements. In any case, we have reviewed the manuscript and the data and we have observed that the SP7 station represented in the previous Fig. 1 is not included in the results presented in the Table 2. This station was removed in the new Fig. 1.

SST and SSS data for each station and for the underway measurements were compared and they do not show differences greater than 0.04 °C and 0.01 units, respectively. This point was clarified and added in the text: Line 146-148: “SST and SSS data were compared with the values collected with the CTD coupled to the rosette-sampler and they do not show differences greater than 0.04 °C and 0.01 units, respectively”. Moreover, the discrete values obtained through the underway measurements of SST and SSS were used because they are necessary in subsequent calculations.

-Line 200: How can there be no spatial and seasonal variation in SSS, when there are freshwater inputs through storms and rivers?

AC: Spatial variation was only observed in the area of the Guadalquivir River related with a storm period that led to very heavy freshwater discharges during December 2014 (Line 230-231).

-Line 212: In which zones exactly? If sharp pCO2 variations are observed that coincide with discrete sampling stations, could that be related to the sampling strategy (e.g. potential sampling of ship exhaust) and not be a real signal? Do you correct for this?

How can the sampling time be depth-dependent if only discrete samples were taken at 5m depth (Line 135)?

AC: The sampling time in each station was variable because water samples were taken at different depths of the water column with Niskin bottles, which were mounted on a rosette-sampler, although in this study we use only the “surface” sample at 5 m. In addition, as these cruises were multidisciplinary, the sampling time was dependent on the various activities carried out in each station. For example, at some stations, this activity could take up to 8 hours due to the sampling of zooplankton where a bongo and neuston net and/or multinet was used. Studies such as those of Sierra et al. (2017a, 2017b), González-García et al. (2018) are examples of these cruises and the other activities carried out here. This sentence is now clarified in the text: Line 239-241: “In Fig. 2 a sharp variation of SST and pCO2 can be observed in some zones that coincides with the stations where discrete water samples were taken. This may be due to the different sampling time at these stations, which varied between 2 and 8 hours in function of the depth of the system”. Additionally, we have observed some instability of the underway measurements of pCO2 in the areas that coincide with the position of the discrete water samples (Fig. 1), due to changes in the flow pump of the ship when its dynamic positioning was functioning.

-Line 220-228: Are there no spatial differences in pH and AOU?

AC: There are some spatial differences in pH and AOU, although a general trend was not observed. They seem related more to the intensity of local processes, such as continental inputs through the River Guadalquivir, increase of the primary production in coastal areas, influence of the upwelling in Trafalgar and relative change in the intensity of the surface currents. A short sentence is now added in the text: Line 259: “No general trend in the spatial variations of pH and AOU was found”.

-Line 258: Is it truly equivalent? 17.4 μatm C-1 divided by 400 μatm results in 0.0435C-1.

AC: Our apologies, the correct value is 16.9 μatm °C-1. This has been rectified in the manuscript (Line 289). Thank you for drawing this to our attention.

-Line 265: It is not clear to me, why table 4 is useful. Clearly, local effects and seasonality impact pCO2-SST relationships, but they are not discussed or put in perspective with the results of the Gulf of Cádiz.

AC: Yes, you are right. We have removed Table 4, and we have discussed in the text certain relationships and seasonal variations found in other studies.

-Line 290: The larger trend in pCO2 in the ocean than in the atmosphere can be driven by

AC: We are sorry, but we think that this suggestion is not complete. In any case, this sentence has been clarified in the manuscript: Line 311-314: "This suggests a possible increase of the anthropogenic nutrient and C inputs from land (Mackenzie et al., 2004) since the direction and magnitude of estuarine and continental shelf CO2 exchange with the atmosphere is highly dependent on the terrestrial organic budget and nutrient supplies to the coastal ocean (Borges and Abril, 2011; Cai, 2011)".

-Line 300-305: There is no statistical difference in pCO2 or temperature with bottom depth, which might be because Figure 5 shows data from all seasons and years.

AC: Yes, you are correct. With this figure, we want to show only the general trend of pCO2 and SST at different intervals of depth of the water column through offshore areas. Fig. 5 has been modified to Fig. 4, and this paragraph moved to Results, now Line 242-247.

-Line 362: You only show the relationship between AOU or pH and pCO2 but there is no discussion of it. Why is almost the entire study area over different seasons oversaturated in oxygen?

AC: This point is now explained and discussed better in the following paragraph of the text (Line 361-373). The oversaturation in oxygen may be due to the influence

of two factors: the first is, greater photosynthetic activity in the area throughout the year (González-García et al., 2018) and the air injection processes responsible for an mean increase of 7μmol L-1 in the surface waters of the ocean (Sarmiento and Gruber, 2006).

-Line 377: total or mean T/B. The T/B ratios by Ribas-Ribas et al. (2011) and de la Paz et al. (2009) have been estimated for which years or seasons?

AC: It is total T/B. Years and seasons of these studies are included in Table 4, and in the text we have included a notification to refer to this table.

-Line 382: How does the DIC flux from the sediment affect T/B?

AC: Benthic DIC flux is another source of inorganic carbon that increases the CO2 concentration in the water column, which would affect the increase of CO2 non-thermal.

-Line 385: What is the cause for ΔpCO2bio variations over depth?

AC: The variations of ΔpCO2bio (now modified to ΔpCO2 non-thermal) observed with respect to the system depth are due to the influence of several processes. In areas close to the coast there is an increase of ΔpCO2non-thermal due to continental inputs, greater primary production and the remineralization of the organic matter in the sediment. In the central area, there is a decrease of these 3 processes. And in the deepest areas, there is an increase of ΔpCO2 non-thermal with the change in the origin of the surface currents. This point clarified in the text, Line 404-419.

-Line 389: If ΔpCO2temp and bio are calculated as a seasonal amplitude, what temperature and chlorophyll values are used to establish the dependency here? Are these annual means (same for Figure 8 A and B). In any case, I do not understand how the thermal component in relation to temperature and the non-thermal component in relation to chlorophyll confirms the importance of different processes on pCO2 variation.

AC: Temperature and chlorophyll values used to establish the dependency are the mean values of the 8 cruises for each of the discrete sampling stations. Following

the suggestions of other reviewers, Fig. 8 was removed and the importance of different processes on pCO2 variation was calculated by a different method (Olsen et al., 2008), (Line 374-391 and Fig. 7).

-Line 397: How can the surface chlorophyll and nutrients be constant, when there is a large gradient with distance to the coast (Line 395)?

AC: Thank you for this question. There was a mistake in the text and it has been corrected (Line 410). We wanted say that chlorophyll-a and nutrients concentrations decrease exponentially with the depth system, but their values are relatively constant in waters with bottom-depth higher to 200 m.

-Line 405: Are the T/B ratios for the different transects significantly different from each other?

AC: T/B ratios for the different transects are not associated with a standard deviation since they are calculated as total ratios, so is not possible to determine significant differences between transects.

-Line 422: Again, why is table 5 helpful? I understand that there are many studies that evaluate shelf area processes in the North Atlantic, but this is not discussed in the manuscript. It appears more as a list of literature than it helps to put you own results in perspective.

AC: Yes, you are right. Table 5 is now Table 4 and following your suggestion it has been modified. In this table only the studies carried out in the Gulf of Cádiz are included, and these are also discussed in the manuscript. Other general studies are also discussed in the text where relevant.

-Figure 3: Panels should have the same size; panel B should next to or below panel A. Add linear correlation equation including units for both panels.

AC: Corrected.

-Figure 6: Why are there 2 regression lines plotted for AOA-pCO2?

AC: Corrected.

-Figure 7 / 9: What are the uncertainties of the thermal and non-thermal components? Are they significantly different from each other?

AC: T/B is a total ratio, so it is not associated with a standard deviation; nor is it possible to determine significant differences between components either. Figure 9 is now Figure 8.

-Figure 10: You could change the colourbar; it is not clear where the border between outgassing and uptake is located.

AC: The border between outgassing and uptake is shown in yellow now (Fig. 10).

-You could simply state in section 2.5. that all reported linear correlations are statistical significant with p-values smaller than 0.05 in the entire manuscript unless indicated otherwise. With that, you do not have to report the p-value again. There a numerous linear relationship equations in the manuscript without units. The correlation equations could be plotted within the according figures to increase readability.

AC: Corrected, thank you for your suggestions. Section 2.5 modified, Line 207-209: “The threshold value for statistical significance was taken as $p < 0.05$. Moreover, all reported linear correlations are type I and they are statistically significant with p-values smaller than 0.05 in the entire manuscript unless indicated otherwise”. Units in the linear relationship are included in the text. Correlation equations have been plotted within the figures but without units, since there is insufficient space (Fig. 5, 6 and 11).

-Please have colour blind people in mind for all figures. It is not possible to differentiate between years with the presently used lighter / darker colours; you could use different symbols as well.

AC: Fig. 5, 6 and 11 have were modified using different symbols.

-Abbreviations that are only used in one paragraph only are superfluous; for example EBUS. Consider abbreviating T by SST and and S by SSS for readability.

AC: These suggestions have been considered in the text.

-The manuscript will benefit from the input of a native speaker. There is a need to check for incomplete sentences and the use of correct tenses. There should be fewer, but longer paragraphs that consist of more than one or two sentences; while covering the same topic. This will make it easier to follow clear arguments.

AC: A native speaker with experience of scientific papers has revised the manuscript again.

References:

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Please also note the supplement to this comment:
https://www.ocean-sci-discuss.net/os-2019-6/os-2019-6-AC1-supplement.zip

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