

## ***Interactive comment on “Factors controlling pCO<sub>2</sub> variability in the eastern Gulf of Cádiz (SW Iberian Península)” by Dolores Jiménez-López et al.***

### **Anonymous Referee #1**

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Jiménez-López et al. discuss spatio-temporal variability of pCO<sub>2</sub> 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 pCO<sub>2</sub> 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 pCO<sub>2</sub> (e.g. Landschützer et al. (2015)). This is also stated by the authors themselves (Line 161 or 372), but

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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  $p\text{CO}_2$ . 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  $p\text{CO}_2$ . Although the authors present many different drivers of  $p\text{CO}_2$  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.

#### 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.

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.

Line 110: Do the transects cover different water masses or circulation features?

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Line 133: How do you correct the temperature difference?

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.

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 pCO<sub>2</sub> 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?

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

Line 212: In which zones exactly? If sharp pCO<sub>2</sub> 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)?

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

Line 258: Is it truly equivalent?  $17.4 \mu\text{atm C}^{-1}$  divided by  $400 \mu\text{atm}$  results in  $0.0435\text{C}^{-1}$ .

Line 265: It is not clear to me, why table 4 is useful. Clearly, local effects and seasonality impact pCO<sub>2</sub>-SST relationships, but they are not discussed or put in perspective with the results of the Gulf of Cádiz.

Line 290: The larger trend in pCO<sub>2</sub> in the ocean than in the atmosphere can be driven by

Line 300-305: There is no statistical difference in pCO<sub>2</sub> or temperature with bottom depth, which might be because Figure 5 shows data from all seasons and years.

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Line 362: You only show the relationship between AOU or pH and  $p\text{CO}_2$  but there is no discussion of it. Why is almost the entire study area over different seasons oversaturated in oxygen?

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?

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

Line 385: What is the cause for  $\Delta p\text{CO}_2$  bio variations over depth?

Line 389: If  $\Delta p\text{CO}_2$  temp 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  $p\text{CO}_2$  variation.

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

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

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.

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.

Figure 6: Why are there 2 regression lines plotted for AOA- $p\text{CO}_2$ ?

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

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

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 are numerous linear relationship equations in the manuscript without units. The correlation equations could be plotted within the corresponding figures to increase readability.

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.

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

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.

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