

Review on "pCO₂ variability in the surface waters of the eastern Gulf of Cádiz (SW Iberian Peninsula)" by Jiménez-López et al.

General comments

The authors use the approach by Olsen et. al. (2008) that allows to quantify different contributions to the total observed change in pCO₂, yet the authors fail to implement this in their discussion. First, complex local interactions are described without using the evidence found in the data (Line 316-373), before the quantification of different contributions appears - more as an after-thought (Line 374-391). Moreover, the authors now describe two different ways to estimate the pCO₂ decomposition in their method section, the original Takahashi approach and the more elaborate decomposition as in e.g. Olsen et. al. (2008). There is no fundamental difference between Equation (1) and (7), but the authors treat it as such in the method and discussion section. The authors should restructure and revise both sections accordingly.

The authors should explain, how the uncertainty of the measurements is determined - is it a standard deviation in space or time? Many reported values lack statistical significance to support the statements made by the authors (probably a consequence of averaging over the entire study area and all seasons), which is not acknowledged or discussed or explained throughout the manuscript.

The manuscript has improved since the first submission, but there are still issues that need to be addressed before the manuscript is ready for publication.

Minor comments

Line 152-168: Again, paraphrase the method by Takahashi et al. 2002. At the moment, many sentences are directly copied from the original paper without citation.

Line 162-165: You sample 4 times a year and according to Figure 3, you do not sample the maximum SST in summer, therefore you do not capture the true seasonal amplitude of SST (or pCO₂). You should explain that you estimate here the differences between summer and winter cruises.

Line 177: superscript SW needs to be explained

Line 186-188: The residual may be dominated by mixing and biological activity, but it also includes salinity-driven and freshwater-induced changes in pCO₂ and other minor processes that impact surface pCO₂. In any case, I do not understand, why the salinity-driven and freshwater-driven changes of pCO₂ are not

calculated as through the presence of large river system this may not be negligible. Moreover, two parameters of the CO₂ system in seawater are measured, that is sufficient to estimate DIC and alkalinity. Follow e.g. Sarmiento and Gruber (2006).

Line 236-238: The reported values do not support this statement; the values are not statistically significant different from zero, except for the winter value.

Line 242-247: Again, why is Figure 4 helpful: there is no statistical difference in both SST and pCO₂ with bottom depth range; there is no general trend to be observed here. There is neither a decrease nor a progressive increase to be observed in the mean values, but there might be, if you look at seasonal values.

Line 281-283: The reported values do not support this statement; only CO₂ fluxes during ST3 and ST5 are statistically significant different from zero; only autumn is statistically significant different from zero.

Line 324: Is C dissolved inorganic or organic carbon? abbreviation without explanation.

Line 351-352: same sentence.

Line 355-357: The present Figure 4 does not support this statement.

Line 374: Figure 7? please recheck all Figure references in the manuscript!

Line 375: Again, the residual also represents salinity-driven and freshwater-induced changes in pCO₂

Line 377: "[...] presents practically the same temporal trend in deep and coastal areas, but with a global behaviour different [...]" - what do you mean by that?

Line 379: The reported values do not support this statement; neither is the distal zone a sink nor the coastal area a source of CO₂ as both values are not statistically significant different from zero.

Line 374-391: Consider discussing dpCO₂/dt instead. Between cruises not the same amount of time has passed and therefore Figure 7 includes a temporal bias that has no physical reason.

Line 449: The reported values do not support this statement; both values are not statistically significant different from each other.

Line 459-460: The reported values do not support this statement; both values are not statistically significant different from zero.

Line 465: The reported value does not support this statement; it is not statistically significant different from zero.

Line 467: Please re-check your estimate of the uptake capacity and correct - if necessary - in the entire manuscript:

$$0.07 \text{ mol C m}^{-2} \text{ yr}^{-1} * 5.28 * 10^9 \text{ m}^2 * 12.01 \text{ g mol}^{-1} = 4.44 * 10^9 \text{ g C yr}^{-1}$$

Line 473: The reported values does not support this statement; both values are not statistically significant different from each other.

Line 493-493: The statement made in the last sentence has not been discussed throughout the paper and needs further explanation.

Figure 10 and 11: Please indicate: air-sea or sea-air CO₂ flux

References:

Sarmiento, J. L. and Gruber, N.: Ocean Biogeochemical Dynamics, Princeton University Press, Princeton, New Jersey, USA, 2006.