

Interactive comment on “Tidal and seasonal carbon and nutrients dynamics of the Guadalquivir Estuary and the Bay of Cádiz (SW Iberian Peninsula)” by M. Ribas-Ribas et al.

Anonymous Referee #2

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GENERAL COMMENTS:

The present paper under review for the journal *Biogeosciences* of Ribas-Ribas et al. describes temporal (tidal and seasonal) dynamics of carbon and nutrients, linked to environmental parameters, across the mouth of two coastal systems in the South West Iberian Peninsula, i.e. the Guadalquivir estuary and the bay of Cádiz. It also quantifies, at the annual scale, carbon and nutrients fluxes from/to the adjacent continental shelf at three different seasons and stations. Even if the paper gives interesting results, their quantification is lacking to bring more value to the objectives of the paper. Indeed, my first general comment is that all along the manuscript, nearly no quanti-

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tative analysis (statistical tests and correlations between parameters) are given, especially in the results and discussion part (sections 3.1 and 3.2). Conclusions of the authors only rely on qualitative points of view and are even often imprecise (i.e., influence of tide and current, relationships between salinity and pCO₂). My second general comment is the non-integrative approach used in this study. Comparisons with other studies on carbon/nutrient dynamics over similar coastal systems (there are mostly no references cited in the discussion part), results from previous works done by the authors (CO₂ fluxes at the air-water interface, Ribas-Ribas et al., 2011b for instance) and data (if they exist) from samplings done directly inside the estuary and the bay, would bring more consistency to the paper. In fact, it is promising, when one considers carbon dynamics over coastal zones, to link the carbon behavior of such heterogeneous systems (metabolic status with NEP and NEE measurements) with the lateral carbon transport with adjacent systems (inland waters and open ocean) (Yan et al., 2008, doi:10.1111/j.1365-2486.2008.01589.x; Guo et al., 2009, *Agr. Forest Meteorol.*, 149, 1820-1828; Cai, 2011, doi:10.1146/annurev-marine-120709-142723). I then recommend substantial revisions in this way to allow the publication of the present paper of Ribas-Ribas et al. for the journal *Biogeosciences*.

SPECIFIC COMMENTS:

1. Abstract: please give quantitative data and numbers in carbon and nutrient fluxes (l.11, 12 and 13 p.14538). The influence of physical parameters on carbon and nutrient dynamics is not presented here whereas it is discussed in the results and discussion part.

2. Material and methods: - l.3, 4 and 5 p.14539: give estimations of the primary production in these two sites. - l.12-13, p.15540: why no cruise was done during the spring season? - l.17-22, p.14542: please describe a little bit more how fugacity of CO₂ measurements were computed even if it is presented in details in Ribas-Ribas et al., 2011b. It would help the reader to understand what exactly the fugacity of CO₂ represents here. - In the Material and methods part, the sampling frequency of measured

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parameters is lacking, for instance dissolved oxygen, fugacity of CO₂ and nutrients. Salinity and temperature measurements are not described, please add these information.

3. Results and discussion: - l. 6 and 7 p.14544: "the general trend was that velocity varied with tidal influence" This statement has to be support by quantitative analysis such as linear regression. - l.7-9, p.14544: it is true but it is not explained by statistics or relevant literatures. - 3.2: in this section, an integrative approach can be given to emphasize relationships between coastal and adjacent systems as proposed and just enounced in the present manuscript (l.4-5, p.14545). I am then wondering if some tidal and diurnal cycles were done inside the estuary and the bay. There are probably spatial differences in terms of water pCO₂ and it could be interesting to link vertical CO₂ fluxes versus lateral carbon transport between these coastal areas and adjacent systems (continental shelf and inland waters). l.6-15, p.14545: this paragraph only takes back the figure 4 caption without any descriptions or explanations about carbon and nutrient evolutions. Also, l.14-15, p.14545, the diurnal influence is not presented all along the manuscript and not shown in figures. It could be interesting to add it especially if the authors highlight the importance of diurnal variability on air-sea CO₂ flux estimations (l.23-24, p.14544). - 3.3: I am wondering about the relevance of computing fluxes at the annual scale with none completed tidal cycles and particularly with only three seasons. Even if I recognize the sampling strategy effort done by the authors, they should clearer enounce that the spring season is missing and that annual fluxes have to be taken with caution. l.7-9, p.14546: this trend is not clear solely from figure 7 and needs to be indorsed by quantitative tests.

4. References: Please add relevant references to allow comparisons with other systems and indorse explanations given in the present manuscript on carbon and nutrient dynamics (concentrations and fluxes) linked to environmental parameters in these two coastal systems.

TECHNICAL COMMENTS:

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l.9-12, p.14538: please rewrite this sentence. l.19-22: this sentence is too long and not clear, please reformulate it. l.15, p.14542: please give the size of the GF/F filters. l.1, p.14545: the sentence is not finished. Fig. 1: please add a spatial scale. Fig. 2, 3, 4 and 5: please homogenize the caption and the y-axis between each season and transect. Add also the atmospheric CO₂ concentration as a dashed line for instance in fig. 3.

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