Anonymous Referee #1

General comments:

Generally speaking, the paper reads well and documents the results from a number of cruises. Unfortunately a number of the conclusions do not appear to have any basis other than subjective viewing of the plotted data. As a result the conclusions appear to be based purely on subjective viewing of the plotted data (e.g. there are instances of phrases including 'trends', 'trend' and 'significant differences' with no statistical evidence to support them). The authors suggest that a number of variables appear to co-vary with the tidal cycle. However, no statistical evidence of this covariation is given (e.g. Pearson correlation coefficient would provide evidence to support such a statement). The updated manuscript should include additional evidence for the conclusions (ie statistical analyses). **We thank the referee for this constructive comment. We undertook an exhaustive statistical analysis that supports our conclusions.**

I was also puzzled as to why the work was submitted to a special issue of Biogeosciences on Earth observation, when the study makes no use (or even mentions) Earth observation data. I would suggest re-submitting an updated manuscript to the main Biogeosciences journal rather than this special issue. **The special issue is the result of the "Earth Observation for Ocean-Atmosphere Interactions Science; ESA, SOLAS, EGU joint Conference." We agree our work is closer to SOLAS than ESA. We think editors should decide if this ms are kept on the special issue or transfer to main Biogeosciences journal.**

Specific comments:

1. The abstract doesn't really give an overview of the main results. It would help if some of the statements were substantiated with values e.g. values of exported nutrients and carbon and correlation statistics with respect to the tidal cycle comment. For instance, some of these values appear on page 14546. **We rewrote the abstract, adding values and statistical test.**

2. Line 14, page 14545, where is the evidence for the statement 'mainly due to biological activity and daily variation? The evidence presented appears to be subjective and via a

reference to a journal paper about a study on the UK shelf. **We added some more evidences.**

3. Section 3.3. I would suggest adding some supporting statistics for the statements of 'trends' and 'significant differences' e.g. correlation coefficients, p-values, slopes etc. **This has been done, as part of the huge effort I put in the stats analysis.**

4. Page 14546, line 4. Do the authors mean the response in non-linear? (rather than a deviation from linearity?). The Bay of Cadiz plot appears to show a bell shaped response with minimum of F(TDN) at x=0 (Fsalt = 0). **Yes, we meant non-linear and we modified the test accordantly.**

5. Section 2.3. How was the accuracy estimate of +/- 2 µmol kg^{-1} determined? Was this experimentally determined or is this from another publication? If so, please add the reference. "**The TA measurements were validated with reference standards obtained from A. Dickson (Scripps Institute of Oceanography, San Diego, USA) to an accuracy of ±2 µmol kg^{-1}."**

6. Line 16, page 14541. How was the error in DIC determined? Or is this a value from another publication? If so, please add the reference **"The calculated error for DIC was ±2.6 µmol kg^{-1}, using error propagation of variances in the carbonate system (Dickson and Riley, 1978)."**

7. Line 19, page 14541. The atmospheric fCO_2 data are from the Azores. This is quite some distance from the study site. The Azores location is considered an open ocean site. Whereas, the study sites are coastal and so are likely influenced by anthropogenic sources of CO_2 (dependent upon the wind direction). It would be helpful if the authors justify the use an open ocean fCO_2 estimate? and/or discuss how such an assumption would affect their estimates of the flux. **We are aware that the Azores location is quite some distance from the study site and that is considered an open ocean site. The others options we considered were: a) the atmospheric CO_2 molar fraction measured with our same instrument at a frequency of 30 min. We decided to use this data only for quality control analysis rather than flux calculation because important source of air contamination from the ship. b) Data measured at Tenerife Atmospheric**

Observatory (Spain) taken from the World Data Centre for Greenhouse Gases (WDCGS/WMO) air sampling network (available at http://ds.data.jma.go.jp/gmd/wdcgg/). Even if this station is a little bit closer and more coastal, is in different latitude. There exist different in amplitude and phase of the annual cycle of atmospheric CO_2 concentration in different latitude. We have included a sentence in the ms discussing how the assumption would affect our estimates of the flux. However, these results have been previously published in Ribas-Ribas et al. (2011b). We added: It should be pointed out that this station is quite far away from our study site and that its location is considered an open ocean site. However, the study sites are coastal and likely influenced by anthropogenic sources of CO_2 (dependent upon the wind direction). This assumption could affect the estimates CO_2 flux by overestimating the flux we reported."

Anonymous Referee #2

General comments:

My first general comment is that all along the manuscript, nearly no quantitative 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 pCO_2). **As both referees agree in that, we realize it was a really important point so we did an exhaustive statistical analysis that supports our conclusions.**

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 (CO_2 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).

We thank the referee for this constructive comment. We have tried to improve our discussion. We added:

a) **"When one considers carbon dynamics over coastal zones, it is promising to link the carbon behaviour of such heterogeneous system (metabolic status with net ecosystem production (NEP)) with the lateral carbon transport with adjacent systems. Cai (2011) examine the net role of terrestrial loadings on ocean metabolism. He considers total organic carbon loading to the ocean as heterotrophic and dissolved inorganic nitrogen flux to the ocean as autotrophic loading. On a global scale, the impact of terrestrial loading on the ocean is to drive it to a more heterotrophic state (Cai, 2011). In the Guadalquivir estuary between June 2006 and February 2007, DOC loading exceed DIN loading driven the system to heterotrophy. This is in agreement with the annual NEP calculated in Ribas-Ribas et al. (2011a)."**

b) **"In an annual scale, the global behaviour was a net source in agreement with Ribas-Ribas et al. (2011b) who report flux in the a more extended area of the same shelf. This result also agree with Chen and Borges (2009) who distinguished between inner ecosystems as a source and continental shelf as a sink."**

c) **"Guo et al. (2009) evaluate the tidal effect on carbon flux in the estuary of the Yangtze River. They similarly conclude that tides have substantial effects on carbon sequestration, although solar and temperature factors exert major controls on the carbon balance at temporal scales lower than days."**

d) **"Total carbon export (F_{DIC}, F_{DOC}, F_{POC}) from the estuary to the ocean during the study period amounted to 7 Tg C, of which 95 % was in the inorganic form (DIC). Winter et al. (1996) reported that the 83 % of the total carbon export in the Swartkops Estuary was in the inorganic form."**

e) **"The Bay of Cádiz import DIC from the continental shelf, with an annual value of 6740 Gg C y-1. De la Paz et al. (2008) calculated the DIC tidal export from the Rio San Pedro (a tidal creek located within the Bay of Cádiz) to the Bay. They identified the tidal pumping as the key mechanism of an annual average transport of 10 Gg C y-1. Forja et al. (2003) calculated a C export from the Sancti Petri Channel (an arm of sea that connects the Bay with the Atlantic) to the Bay of 0.18 Gg C d-1 during summer."**

Specific comments:

1. Abstract: please give quantitative data and numbers in carbon and nutrient fluxes (l.11, 12 and 13 p.14538). **This has been done. We added: "During the whole study period, Guadalquivir Estuary exported components at a rate of 3 Gmol of SiO_2, 4 Gmol of DIN, 3 Gmol of TDN, 31 Gmol of DOC and 604 Gmol of DIC per year. On the other hand, Bay of Cádiz imported 3 Gmol SiO_2, 1 Gmol of DIN, 2 Gmol of TDN, 33 Gmol of DOC and 562 Gmol of DIC per year."**

The influence of physical parameters on carbon and nutrient dynamics is not presented here whereas it is discussed in the results and discussion part. **We added in the abstract: "Tides influence velocity and transport: we found statistically significant differences ($p < 0.0001$, n = 220) between the flood tide (the mean velocity was 4.85 cm s^{-1}) and**

the ebb tide (the mean velocity was -5.67 cm s^{-1}). Biological activity and diurnal changes have also an important role on the carbon and nutrient dynamics."

2. Introduction: l.3, 4 and 5 p.14539: give estimations of the primary production in these two sites. **This has been done. We added: "Coastal waters near the mouth of the Guadalquivir River and the Bay of Cádiz present the highest primary production within the Gulf of Cádiz (Navarro and Ruiz, 2006)."**

3. Material and methods: l.12-13, p.15540: why no cruise was done during the spring season? **We couldn't do the spring cruise because we had a different ship with limited ship time. Furthermore, this ship was not equipped with ADCP. We added to the text: "Unfortunately, due to logistical reasons, no cruise has undertaken during spring."**

l.17-22, p.14541: please describe a little bit more how fugacity of CO_2 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 CO_2 represents here. **This has been done. We added: "The surface water CO_2 molar fraction (xCO_2) was measured with a non-dispersive infrared gas analyser (Licor®, LI-6262). At the beginning and the end of each day, the equipment was calibrated with two standards: CO_2 free-air and a high CO_2 standard gas with a concentration of 530 ppm (with pre-deployment laboratory calibration against Air-Liquide France standard). The temperature inside the equilibrator was measured continuously by means of a platinum resistance thermometer (PT 100 probe). The temperature difference between the ship's sea inlet and the equilibration system was less than 0.8 °C during all the cruises. The accuracy (precision) of seawater fCO_2 measurement was ± 3 (± 0.5) µatm.**

The water-saturated fCO_2 in the equilibrator was calculated from the xCO_2 in dry air; the atmospheric pressure data was provided by the Spanish national government (Organismo Público Puertos del Estado); and equilibrium water vapour was calculated according to the protocol described in Dickson et al. (2007). The formulation proposed by Takahashi et al. (1993) was employed for the partial pressure corrections to in situ water temperature."

The sampling frequency of measured parameters is lacking, for instance dissolved oxygen, fugacity of CO_2 and nutrients. **This has been done. We added: "Salinity, temperature and fCO_2 were sampled with a frequency of 30 s from the surface seawater supply of the ship (pump inlet at a depth of 3 m) but average in the times the ship stopped for stations. The discrete samples (oxygen, nutrients…) were measured every 2 h (every forward transect).**

Salinity and temperature measurements are not described, please add these information. **This has been done. We added: "Salinity and temperature were measured, using a SeaBird thermosalinograph (Micro-SeaBird 45), before water entry into the gas equilibrator. Salinity and temperature are estimated to be accurate to ± 0.005 and ± 0.004 °C, respectively, according to the SeaBird calibration data.**

4. 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. **I can't do a linear regression because I don't have a continuous record of sea level in our study areas. There are some in stations nearby but due to the shallowness of the area and the highly variability it's better not to compare with our data. What I do have is the high and low tides times. So I grouped the stations according if there were flood or ebb tide. We added to the ms: "Positive velocity values indicate onshore, while negative values represent offshore flux. The general trend was that velocity varied with tidal influence. Grouping the stations between ebb and flood tide, we found that statistically significant differences ($p < 0.0001$, n = 220): during the flood tide the mean velocity was 4.85 cm s^{-1} while during ebb tide the mean velocity was -5.67 cm s^{-1}."**

l.7-9, p.14544: it is true but it is not explained by statistics or relevant literatures. **We added a good references and the following explanation: "North of the Guadalquivir Estuary, the predominant waves are from the West and the coast alienation is NW-SE so the longshore drift has no interruption until it arrives to the Estuary (Ortega et al., 2010). Once there, the current stop and the sediment fall, forming the *flecha de Doñana* (Ortega et al., 2010)."**

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 pCO_2 and it could be interesting to link vertical CO_2 fluxes versus lateral carbon transport between these coastal areas and adjacent systems (continental shelf and inland waters). **We answer that in the general comments.**

l.6-15, p.14545: this paragraph only takes back the figure 4 caption without any descriptions or explanations about carbon and nutrient evolutions. **We added a short explanation for figure 4 and 5.**

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 CO_2 flux estimations (l.23-24, p.14544). **We think we discuss the diurnal variation before (for example, p. 14544, l 22 in the original ms) so we will kept as it is.**

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.
We agree with the referee. We changed the section title to "Across-shore fluxes" and we enounced the calculations limitations as follow: "It should be point out that due to none completed tidal cycles and only three seasons measured, the annual fluxes should 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. **We did a Kruskal-Wallis test (the equivalent of nonnormal ANOVA). So modify the sentences: "Bay of Cádiz has significant greater deviation from conservative behaviour during positive salt fluxes in November (p = 0.01, n = 21). Seasonal differences also exist: for example, for the Bay of Cádiz, June TDN flux was significantly different than November TDN flux (p = 0.03, n = 87). For the**

Guadalquivir Estuary, June TDN flux was significantly different from November and February TDN flux (p = 0.0001, n = 78).”

5. 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. **We added the appropriated references.**

TECHNICAL COMMENTS:

l.9-12, p.14538: please rewrite this sentence. **This has been done. It was a point missing that interferes with the clarity of the sentence. In addition, values have been added. The final sentence is as follow: “Three cruises have been undertaken in June 2006, November 2006 and February 2007. During the whole study period, Guadalquivir Estuary exported components at a rate of 3 Gmol of SiO_2, 4 Gmol of DIN, 3 Gmol of TDN, 31 Gmol of DOC and 604 Gmol of DIC per year. On the other hand, Bay of Cádiz imported 3 Gmol SiO_2, 1 Gmol of DIN, 2 Gmol of TDN, 33 Gmol of DOC and 562 Gmol of DIC per year.”**

l.19-22: this sentence is too long and not clear, please reformulate it. **We found a more appropriate sentence (thanks to the references the referee gave to us) for this introduction so we deleted the previous and added: “Where, when, and how organic carbon is decomposed into carbon dioxide and lost to the atmosphere through the coastal continuum of rivers, estuaries, marshes, and continental shelves before reaching the slope and the open ocean is still controversial (Cai, 2011).”**

l.15, p.14542: please give the size of the GF/F filters. **We added: “precombusted Whatman GF/F filters of 47 mm diameter (0.7 µm nominal pore size) and Whatman GF/F filter of 25 mm diameter (0.7 µm nominal pore size), respectively”**

l.1, p.14545: the sentence is not finished. **We finished now. It was a typing mistake.**

Fig. 1: please add a spatial scale. **The scale is indicating by Latitude and Longitude by the edges of the graph.**

Fig. 2, 3, 4 and 5: please homogenize the caption and the y-axis between each season and transect. Add also the atmospheric CO_2 concentration as a dashed line for instance in fig. 3.

We double checked the caption and labels although I think it was a problem when changing the format in the printing. We added atmospheric CO_2 concentration.