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Discussions
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Interactive comment on "Reconstruction of super-resolution fields of ocean pCO_2 and air–sea fluxes of CO_2 from satellite imagery in the Southeastern Atlantic" *by* I. Hernández-Carrasco et al.

I. Hernández-Carrasco et al.

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We are grateful to the reviewer for her/his comments that helped us to improve our manuscript. Our responses are provided below every comment. We have also enclosed, as supplemental document, a pdf with the new version of our manuscript and a detailed response to the suggestions and comments.

Please note that we added two co-authors, with their approval, in our publication to be in agreement with the SOCAT atlas rules when using SOCAT in situ data. The

two authors are: M. Gonzalez-Davila and J. M. Santana-Casiano from the Instituto de Oceanografia y Cambio Global, Universidad de Las Palmas de Gran Canaria, 35017, Las Palmas de Gran Canaria, Spain.

Reviewer:

Hernandez-Carrasco et al present an interesting new approach to map the partial pressure of CO2 in the surface ocean and the resulting air-sea gas flux, using satellite data. The authors convincingly show that their new high resolution approach obtains better results than a low resolution product (CARBONTRACKER) in the Benguela system when being compared to in-situ observations. The manuscript offers a method to the reader that on the one hand can be used to monitor the carbon cycle in the important EBUS regions but further has the potential to be applied globally.

I do believe the manuscript offers (a) a novel approach, (b) is clearly written – particularly the method section is easy to follow for the reader – and (c) describes an approach with potential for many future applications, hence I do recommend the manuscript for publication in BG. My specific comments below are intended to further improve the manuscript:

Response:

We thank the reviewer for his/her positive comments.

Specific comments:

General:

Reviewer:

I only have one overarching point of criticism and this is the choice of data. While the authors do a great job testing several satellite chlorophyll-a and sea surface temperature products, the more fundamental question is why temperature and chlorophyll alone? E.G. it becomes very clear when looking at figure 11 (see longitudes 12.5 to

13.5 differences >20 μatm) that there is a stronger in-situ to product disagreement close to shore. Is this not a sign that near the coast the available data streams are possibly not enough to capture all the variability, whereas the more open ocean areas are better represented? At least some discussion would be useful.

Response:

To address this comment we have plotted in Fig 1 (see below) (pCO2insitu vs. pCO2ctrack) and (pCO2insitu vs. pCO2infer) with points coloured by longitude using all the CarbonTracker and inferred pCO2 values in the intersections with in-situ pCO2 during 2006 and 2008. This is for the case using Globcolour OC and OSTIA SST in the reconstruction of pCO2. We have used this scatter plot to see the difference in the results between points close to the coast with those in the open ocean. For longitudes greater than 10 degrees (closer to the coast) pCO2ctrack and pCO2infer values are overestimated with more points closer to the diagonal for longitudes smaller than 10 degrees (open ocean region). This shows that near the coast the available input data do not capture all the variability, whereas the more open ocean areas are better represented. This could be explained by the attenuation of the transitions fronts revealed by the merged Globcolour and OSTIA products used to alleviate cloudiness issues but we have obtained the same results (not shown) using the different merged and non merged products combinations. Thus, this disagreement with in situ data close to the coast can only be induced by the shortcomings of the CarbonTracker products in regions near the coast.

Reviewer:

Abstract lines 1-4: circular sentence – remove or revise

Response:

We rewrote these 3 lines as: “An accurate quantification of the role of the ocean as source/sink of Green House Gases (GHGs) requires to access the high-resolution of

the GHG air-sea flux at the interface”.

Introduction:

Reviewer:

General: In the introduction there is a use of GHG’s and CO2. The manuscript itself has its focus on CO2. Is the intention to motivate the reader that this approach can be used for all GHG’s (then please state so explicitely)? Otherwise for clarity the use of GHG may be replaced by CO2

Response:

This approach can be used to reconstruct all GHGs and we have included a sentence in the introduction to point out that the method has a wide applicability (Pag. 2 line 151-155). In addition we have replaced GHG by CO2 in the cases where we focus, specifically, on CO2 (Page 2 lines 103 and 145).

Reviewer:

page 1407 line 6: “resolve” not “solve”

Response:

We have replaced “solve” by “resolve”

Reviewer:

page 1407 line 8: “prevent us”

Response:

It has been corrected.

Reviewer:

page 1407 lines 19-20: Your products big advantage is its high resolution. It seems unfair in the introduction to present the 4x5 degree monthly climatology from Takahashi

et al. as the most “advanced” pCO2 based product in this respect. There are high(er) temporal resolution products (Rödenbeck et al 2014 – 4x5 degree daily) and spatial resolution products (Nakaoka et al 2013 – 0.5x0.5 degree monthly; Landschützer et al 2014 - 1x1 degree monthly), which I think fir better in this discussion. This however does not change the message as the product presented in this study is still of higher resolution.

Response:

Our intention in this discussion is not to present the product from Takahashi et al. as the most advanced but to enumerate current different approaches to estimate ocean pCO2 looking at their resolution. Thus, as suggested by the reviewer, we have included references on these products in lines 68-72 of the new manuscript to improve the discussion on different products at different spatial and temporal resolutions.

Reviewer:

Page 1408 lines 10-11: I am not convinced that this statement is true for the ocean (at least not as much as it is for the land)

Response:

We state that the spatial resolution of the CO2 fluxes in the ocean is not high enough from remote sensing data to resolve the small spatial variability of the source and sinks of CO2. On the other hand there is an uncertainty in extending ocean pCO2 over large gridded areas from limited coverage of the observations. Thus a better estimate of sub-gridscale processes and associated uncertainties using remote sensing is a high priority task to be conducted (Wang et al 2014, JGR).

Data:

Reviewer:

page 1410 line 23: “ENVISAT” - throughout the manuscript, some abbreviations are

explained (e.g. SCIAMACHY), whereas others (like e.g. ENVISAT) are not.

Response:

We have explained the following abbreviations:

- ENVISAT (Environmental Satellite)

- LEGOS (Laboratoire d’Etudes en Géophysique et Océanographie Spatiales)

- SeaWiFS (Sea-Viewing Wide Field-of-View Sensor)

- JPL (Jet Propulsion Laboratory)

- PO.DAAC (Physical Oceanography Distributed Active Archive Center)

Reviewer:

page 1411 lines 21-24: Globalview reports xCO2 in the atmosphere, whereas you report oceanic pCO2. Please clarify how you have dealt with this difference (unlike the fCO2 to pCO2 correction, the xCO2 to pCO2 correction is not minor, hence it is not necessary neglectable when you compute air-sea fluxes, i.e. it has to be explicitly shown)

Response:

We use the GLOBALVIEW time series to derive our atmospheric pCO2 value (and not the oceanic one).

Method:

Reviewer:

page 1418 lines 13-17: Please consider splitting this sentence in two to make it easier to read.

Response:

The sentence has been splitted in two as suggested by the reviewer (lines 494-499).

Results:

Reviewer:

Although the merged products provide more coverage, the missing data from cloud coverage provide a major limitation to the product especially when air-sea fluxes of CO2 and their variability are investigated. This is a problem on the local, as well as on the global scale. In view of the future applications the authors mention, how do you plan to deal with this issue?

Response:

Pottier et al. (2008) proposed a wavelet-based inference method for reconstructing ocean-color maps with missing pixels, so this methodology could be an avenue to follow to address the cloud coverage issue when the latter is not too severe.

Figures:

Reviewer:

I was a bit puzzled looking at figure 1: Both products illustrate a strong carbon uptake along the coast (purple color) whereas I would have expected the opposite.

Response:

Fig. 1 has been replotted with a different masking of the pixels (white instead of blue).

Reviewer:

Figure 6d: Is this the average flux density (averaged by latitude)? I think the integrated flux (in GtC/s or TgC/yr, etc.) is a better visualization than the flux density and it additionally makes it easier to put the importance of the sink into a bigger (regional/global) perspective.

Response:

In Fig 6d we have plotted a longitudinal transect of the maps shown in Figures 5e and 5f at a particular latitude (33.5°S in this case) in order to show the small scale spatial variability of the reconstructed pCO2 as compared to pCO2 derived from CarbonTracker.

Reviewer:

Figures 7 and 8: Why is there a difference between the estimated area here and in figure 1?

Response:

Fig. 1 has now the same area than the other figures.

References:

Wang, G., M. Dai, S. S. P. Shen, Y. Bai and Yi Xu (2014). Quantifying uncertainty sources in the gridded data of sea surface CO2 partial pressure. J. Geophys. Res. Oceans, 119, 5181–5189, doi:10.1002/2013JC009577.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/12/C1676/2015/bgd-12-C1676-2015-supplement.pdf

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Interactive comment on Biogeosciences Discuss., 12, 1405, 2015.

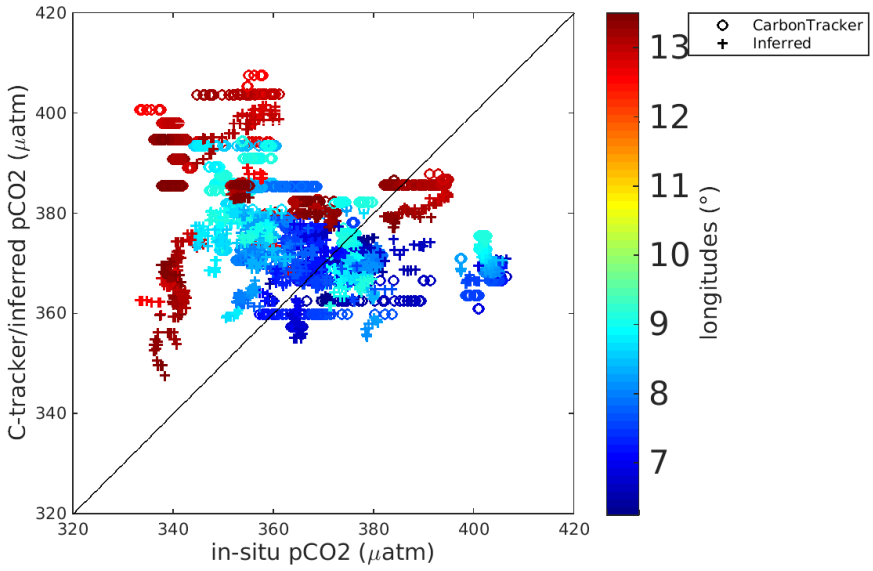

Fig. 1. Scatter plot showing pCO2 values from CarbonTracker vs in-situ (in blue) and inferred vs in-situ (in red) at the intersections coloured as a function of longitude.