Responses to comments from reviewers on the paper "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.***

Dear Editor,
please find enclosed a new version of our manuscript and a detailed response to the suggestions and comments of the referees.

Please also 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.

Sincerely,

The authors

Responses to Reviewer #1

We thank Dr L. Gregor for his positive and constructive review. The original comments are shown in italics, and our response in normal typeface.

GENERAL OVERVIEW

Reviewer:
The manuscript uses a combination of remotely sensed low-res air-sea CO_2 flux and high-res Chl-a and SST to arrive at high-res air-sea CO_2 fluxes. The authors present a method new to this application and the publication fits within the scope of BGD. The manuscript is well written and is relatively error-free with a few inconsistencies in abbreviations. The methodology presented to arrive at a high-resolution air-sea CO_2 flux result is comprehensive, but tricky to follow if the reader is not familiar with the jargon. The authors should be aware of this and simplify wording as much as possible. There is no discussion this paper, but given the methodological nature of this study I do not think this is a critical omission. I enjoyed reviewing This manuscript and I think this approach has great potential for high temporal and spatial resolution CO_2 surface data with some refinement.

Authors:
We appreciate your interest in and support for our work. We have modified the manuscript according to the suggestions and criticisms you have formulated making the manuscript clearer. In particular we have improved the description of the methodology incorporating a scheme of the algorithm so that the reading becomes easier for scientists not familiar with the method.

SCIENTIFIC REMARKS

Title

Reviewer:
The title does capture the topic that the paper discusses; however, I do feel that fields of does not contribute to the reader's understanding of the topic.

Authors:
We have removed "fields of" in order to make clearer the topic of the paper. The title is now: “Reconstruction of super-resolution ocean pCO_2 and air-sea fluxes of CO_2 from satellite imagery in the Southeastern Atlantic”.

Introduction

Reviewer:
The introduction introduces the topic well and do reference the appropriate work in most part. However, I feel that the authors should mention statistical learning methods in their introduction. While the approach is quite different it is also a data based approach to derive pCO_2 . Some noteworthy mentions are Landschutzer et al. (2014) and Telszewski et al., (2009). Though none of these methods have focused specifically on coastal regions.

Authors :
We have quoted in the introduction (Page 2, lines 77-81) the works by Landschutzer et al, 2014 and Telszewski et al, 2009 on the empirical relationships between ocean variables by using neural networks to estimate maps of pCO_2 .

Data

Reviewer:
It is good that the authors use and compare the different datasets.

Authors :
We thank the reviewer for this positive comment.

Methods

Reviewer:
I like the approach used in this study; however, it is fairly involved and may be confusing for some readers. It is noted that the authors do provide an overview of the methods on page 1415 L21, but it would be useful to have simple overview of the methodology such as that shown below.

1. CarbonTracker provides surface CO2 fluxes

2. Flux is used to calculate pCO_2^{sea} at low resolution ($pCO_2^{sea(LR)}$)

3. Use satellite SST, SSS and CCMP for winds

4. $F = K(pCO_2^{air} - pCO_2^{sea})$ ==> $pCO_2^{sea} = pCO_2^{air} - F/K$

5. Use MMF to extract the dimensionless singularity exponents of SST, Chla, CO_2^{LR} CO_2^{HR} from ROMS-BIOEBUS (various resolutions) output

6. Calculate the linear relationship between SST, Chl-a, CO_2^{LR} and CO_2^{HR} singularity exponents *from ROMS-BIOBUS*

7. Find singularity exponents of satellite SST, Chl-a and CO_2^{LR}

8. Use coefficients from ROMS-BIOEBUS (step 4) and apply to the singularities from the satellite data (step 5) to infer the singularity exponent CO_2^{HR}

9. Reconstruct pCO_2^{HR} *from the cross-scale inference of* pCO_2^{LR}

10. Calculate air-sea CO_2 *fluxes from* pCO_2^{HR} *temperature and wind.*

Authors :
According with the reviewer suggestion, we have included the following scheme of the algorithm, step by step, at the end of Sect. 3 in order to clarify the methodology used in this study.

i) After selecting a given area of study, compute the singularity exponents of SST, Chl and pCO_2 at low and high resolution from ROMS-BIOEBUS outputs. This is done once and then they can be used for every computation performed over the same area.

ii) Using Eq. 2 estimate ocean pCO2 at low resolution : $pCO_2^{sea} = pCO_2^{air} - F/\alpha K$, where :

- F is the surface CO_2 fluxes provided by CarbonTracker product.
- K is the gas transfer velocity obtained by the parametrization developed by Sweeney et al, 2007, as a function of wind.
- α is the gas solubility derived according to Weiss 1974.
- pCO_2^{air} is provided by Globalview-CO2 product.

iii) Obtain the regression coefficients a, b, c and d of Eq. 3 for the singularity exponents obtained in step i).

iv) Calculate the singularity exponents of available satellite SST, Chl at high resolution and pCO_2^{sea} at low resolution (step ii).

v) Use coefficients obtained in step iii) and apply Eq. 3 to the singularity exponents from satellite data (step iv) to estimate a proxy of singularity exponents of high resolution ocean pCO_2 , $S(pCO_2)$.

vi) Using Eq. 4 reconstruct p CO_2 at high resolution from the multiresolution analysis computed on signal $S(pCO_2)$ and cross-scale inference on p CO_2 at low resolution.

vii) Use Eq. 2 to calculate air-sea CO_2 fluxes from the inferred high-resolution pCO_2 obtained in step vi).

Reviewer:
I like the use of model data (ROMS-BIOEBUS) to estimate the MLR coefficients and estimating the accuracy of the method. This does make the assumption that dynamics of SST, Chl-a and pCO_2 *in the model and satellite data operate on the same scale. The authors do allude to this and justify the adequacy of ROMS-BIOEBUS. It would be good if this inference were stated a bit more explicitly. Perhaps a figure showing the PDFs of the ROMS-BIOEBUS data would address this concern?*

Authors:
The use of ROMS-BIOEBUS outputs to obtain the regression coefficients does not make the

assumption that dynamics of physical and biogeochemical variables of the model and satellite data operate at the same scale. However the singularity exponents (dimensionless values) of these variables (pCO2, SST, Chl) do present a functional relationship between them, whether we look at model outputs or satellite data. The ROMS-BIOEBUS capability to represent SST, SSS and density fields in the Benguela has been evaluated comparing the outputs of the model with annual and seasonal CARS climatologies (see Gutknecht et al., 2013 for more details).

Reviewer:
The authors mention an error of 2.4 μatm when the method is applied to ROMS data. A relative error of 0.6% is given - relative to the pCO_2 range? This is a small error relative to the range of pCO_2. What is this average difference/error between the ROMS high-res and the ROMS low-res data? An error relative to the (high-res/low-res) may be more telling.

Authors:
We have recomputed the mean absolute error over the 10 years climatology for the dual ROMS simulations and found 3.02 μatm and a relative error of 0.89%. These values are slightly higher than those mentionned in the original manuscript since we had only considered the last year of simulations. We have included these new results in the corrected manuscript (lines 582, 583).
As suggested by the reviewer, we have computed the absolute and relative errors between High-Resolution/Low-resolution ROMS to compare with the result obtained by our method. First, we resize the low resolution to the high resolution grid without any interpolation (1 pixel of low resolution is resized in 4x4 pixels of the new grid). After this, we compute absolute error = ABS(ROMShr – ROMSlr resized) and the relative error = absolute error / ABS(ROMShr) in each pixel, and finally we compute the mean of absolute and relative error for all pixels of the 360 images corresponding to the ROMS outputs.
In doing so, we obtain for the absolute error 12.1 μatm and for the relative one 3.6%. In conclusion, our method allows to decrease the relative error from 3.6 to 0.89% when going from ROMS low resolution to reconstructed ROMS high resolution.

Reviewer:
The authors also mention a paper by in review Sudre et al. (2015) on several occasions. I do not feel that this will be a problem once this paper has been published; otherwise I do not feel the authors should cite this work.

Authors:
The paper by Sudre et al. (2015) was with minor revision and the present status on line in the journal is “with Editorial decision”, so we think we can leave it and quote this work.

Results

Reviewer:
The use of mean error (ME) here is unusual. For their purpose of use, the use of ME seems OK, but it is essentially the difference of the means of the two datasets (the inference bias). Given its similarity in nomenclature to Mean Squared Error (MSE analogous to AE), I think that the authors should consider a different name for this error. This is especially true, as they do not use it for the same purpose as one would use MSE.

Authors:
To avoid misunderstanding, we have modified the nomenclature and in the new manuscript we use mean differences (MD) instead of mean error (ME) for the average of the difference point by point of the different data sources.

Reviewer:
It would be good to see (pCO_2^{insitu} vs. pCO_2^{ctrack}) and (pCO_2^{insitu} vs. pCO_2^{infer}) plots for more data. Points could be coloured by longitude.

Authors: As suggested by the reviewer we have plotted (pCO_2^{insitu} vs. pCO_2^{ctrack}) and (pCO_2^{insitu} vs. pCO_2^{infer}) using all the CarbonTracker and inferred pCO_2 values in the intersections with in-situ pCO2 for 2006 and 2008. In Fig. 1 (not included in the manuscript) we show the case for Globcolour OC and OSTIA SST data product combinations. This figure shows that correlation is not entirely satisfactory for both pCO_2^{ctrack} and pCO_2^{infer}, even if there are more points of pCO_2^{insitu} - pCO_2^{infer} closer to the diagonal straight line (in black), for instance the cloud of points around 360-370μatm. Fig. 2 shows the same as plotted in Fig. 1 but points colored as a function of longitude. For longitudes greater than 10 degrees East (closer to the coast) pCO_2^{ctrack} and pCO_2^{infer} values are overestimated with more points closer to the diagonal for longitudes smaller than 10 degrees (open ocean region). This can be a sign that near the coast the available input CarbonTracker data are possibly not good enough to capture the variability, whereas the more open ocean areas are better represented in this product.

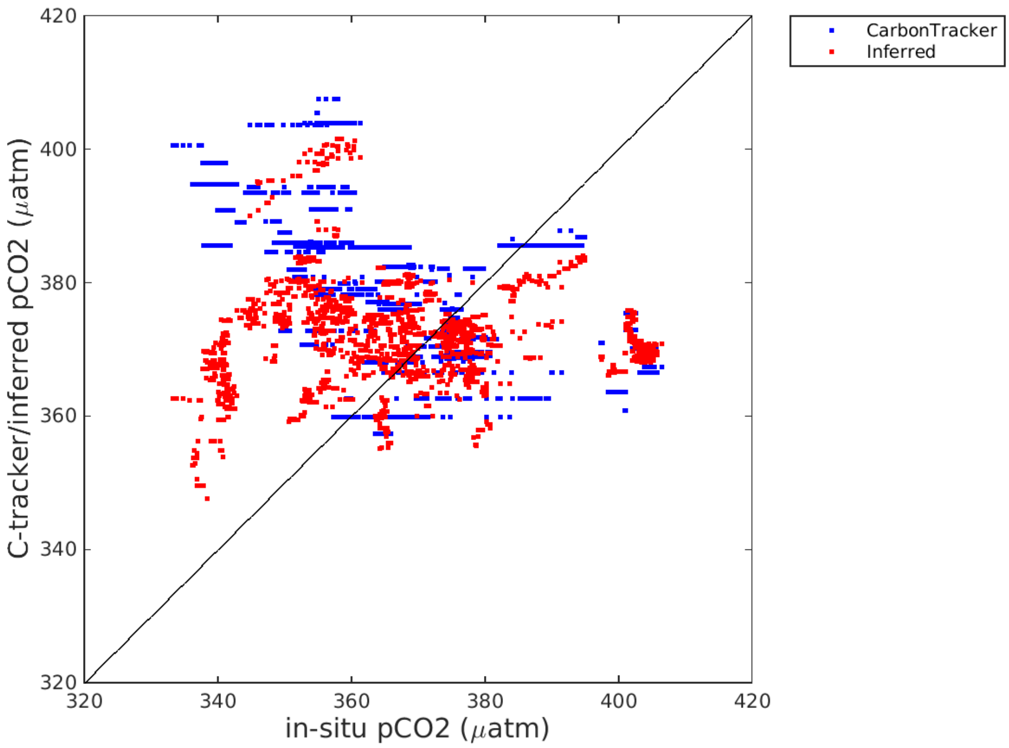

Fig 1. Scatter plot showing pCO2 values from CarbonTracker vs in-situ (in blue) and inferred vs in-situ (in red) at the intersections.

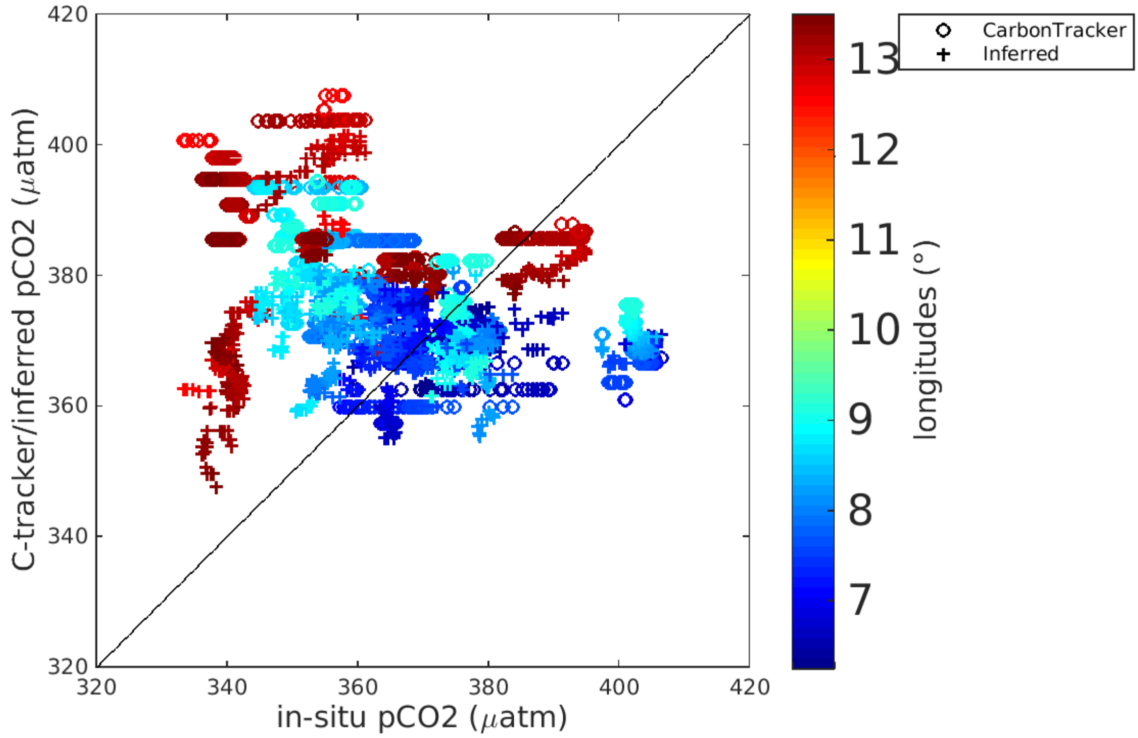

Fig 2. The same as Fig. 1 but coloured as a function of longitude.

Reviewer:
The comparison of in-situ, inferred and CarbonTracker data shows the potential of the method presented in this manuscript as well as the shortcomings of using Carbon-Tracker data for the estimation of air-sea CO_2 fluxes. I think that the authors should briefly state that the output will only be as good as the input.

Authors:
As suggested by the reviewer we have added the following sentence in the conclusions (Page 18 and lines 977-983): "The statistical comparison of inferred and CarbonTracker pCO_2 values with in-situ data shows the potential of our method as well as the shortcomings of using CarbonTracker data for the estimation of air-sea CO_2 fluxes. From these results it can be said that the outputs of our algorithm will only be as good as the inputs."

<u>Figures</u>

Reviewer:
General comment on line figures: as a colour-blind reader, I struggle to see yellow lines on white background. It is not imperative that this changed, but would be better in a darker shade.

Authors:
We have changed the background of the figures with gray colour as suggested by the reviewer.

SPECIFIC COMMENTS

Page and line	Phrase or topic	Correction or comment
P1406 L26	*interacts*	*interact* **Response:** This has been corrected
P1407 L19	*Let's cite here the work of. . .*	*This sentence seems a little clumsy* **Response:** We have reworded the sentence and now it reads : "Among others, we can find the work by…." (Page 2, lines 63-64)
P1407 L17-L25	*Possible missing citations*	*The authors fail to mention statistical learning methods and associate literature (Lachkar and Gruber, 2012; Landschutzer et al., 2014; Telszewski et al 2009 and several others)* **Response:** We have added new references on statistical neural networks (Page 2, line 77-81).
P1409 L7	*has been proved to be innovative. . .*	*has been proven innovative . . .* **Response:** Corrected
P1409 L16	*relates closely the*	*relates closely the - a bit clumsy otherwise* **Response:** The sentence has been improved
P1410 L4	*Section 3*	*Inconsistent abbreviation* **Response:** Corrected
P1411 L12	*sea-state*	*Sea state should not be included here as this is part of the parameterisation wind accounts for this.* **Response:** We wrote K, the gas transfer velocity, is a function of wind, salinity, temperature, sea state, which can be obtained from satellite data. Here we meant K is a function of all these parameters in a general sense. Since bubble mediated gas transfer depends on wave breaking, whitecapping and dispersion by mixing processes in the upper ocean, its environmental dependence (on wind speed, sea state, water temperature..) is a function

		of the environmental dependence of these processes.
P1411 L24	*pCO2 -air*	*Authors use Ascension Island as a reference. Would Cape Point, South Africa not be a closer reference?* *http://www.esrl.noaa.gov/gmd/ccgg/obspack/labinfo.html* **Response:** Reviewer is right and the Ascension Island station is not the closest one. The Ascension Island station is located at 7.97°S and 14.4°W with an elevation of 54m above the sea level, closer to the equator than our area of study. Another station is located at Gobabed (23.58°S, 15.03°E) but at 456m above sea level. The station at Cape Point in South Africa is closer but at 300m above sea level. We chose to use the Ascension Island because it is closer to sea level. We have clarified this point in Page 4, line 239.
P1411 L26	*Garbe and Vihharev (2012) approach*	*Briefly mention what their approach is reader does not know what this approach is.* **Response:** We have replaced the sentence "For this reason, an approach similar to Garbe and Vihharev (2012) has been developed and applied to the CarbonTracker data set." by "Garbe and Vihharev (2012) have developed an optimal control approach to invert interfacial fluxes using a simplified inverse problem of atmospheric transport. The inverse problem is solved using the Galerkin finite element method and the Dual Weighted Residual (DWR) method for goal-oriented mesh optimization. An adaptation of this approach has been applied to the CarbonTracker data set." (lines 250-256)
P1412 L5	*retain very well the structure of the CarbonTracker fluxes*	*retain the structure of the CarbonTracker fluxes very well* **Response :** The sentence has been corrected
P1415 L21	*The idea*	*Be a little more specific about which idea* **Response :** The idea refers to the idea behind the methodology. We have clarified this in the manuscript (see line 391).

P1415 L26	*Partial pressure pCO2*	*Partial pressure (pCO2)* **Response :** Corrected
P1416 L1	*good characteristics*	*What are good characteristics of a linear regression in this case?* **Response :** We have removed "with good characteristics".
P1420 L11	*relative error*	*Relative to total pCO2. See scientific remarks section for more on this.* **Response :** We refer reviewer to the detailed response described in the section methods of the scientific remarks.
P1424 L18	*how different can be the coverage of the pCO2 field can be depending. . .*	*how different coverage of pCO2 can be in the field depending. . .* **Response :** The sentence has been reworded.
P1426 L28	*Abbreviations*	*Why not apply these from the start. They make it much easier to follow the discussion.* **Response :** As recommended, in the new manuscript we use these abbreviations from the beginning of Sect. Results.
P1427 L24	*Showing that have of the measurements is geographically in the coastal region of Benguela, outside the. . .*	*Showing that half of the measurements fall within the coastal region of the Benguela (land masked by CarbonTracker)* **Response :** The sentence has been modified as suggested by the reviewer.
P1428 L8	*study qualitatively*	*qualitatively study* **Response :** Corrected : quantitatively study
P1440 Tab4	*No valid intersections*	*Should this be number? If so add No.* **Response :** 'No' is number. We have replaced 'No' with 'Nb' in order to avoid typing errors in the production process.
P1444 Fig3	*a, b*	*Make colour scales the same*

		Response: In the new Fig. 3a and 3b we have used the same colour scale.
P1446 Fig5	*c, d, e, f*	*Ensure that scales are the same for pCO2 and FCO2 for inter-comparison.* **Response: W**e have used the same colour scale pCO2 plots and the same colour scale for maps of CO2 fluxes.

Responses to Reviewer #2

We are grateful to the reviewer for her/his comments that helped us to improve our manuscript. The original comments are shown in italics, and our detailed response can be found in normal typeface below every comment and the corresponding changes made in the revised manuscript.

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:

Authors:
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.

Authors:
To address this comment we have plotted in Fig. 2 (see above) (pCO_2^{insitu} vs. pCO_2^{ctrack}) and (pCO_2^{insitu} vs. pCO_2^{infer}) with points coloured by longitude using all the CarbonTracker and inferred pCO_2 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) pCO_2^{ctrack} and pCO_2^{infer} 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

Authors:
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

Authors:
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"

Authors:
We have replaced "solve" by "resolve"

Reviewer:
page 1407 line 8: "prevent us"

Authors:
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.

Authors:
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)

Authors:
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).

<u>Data:</u>

Reviewer:
page 1410 line 23: "ENVISAT" - throughout the manuscript, some abbreviations are explained (e.g. SCIAMACHY), whereas others (like e.g. ENVISAT) are not.

Authors:
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)

Authors:
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.

Authors:
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?

Authors: 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.

Authors:
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.

Authors:
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?

Authors:
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.