Authors response to Referee #2

We thank the referee for reviewing the manuscript (ms) and for his/her constructive comments. Following recommendations from the two referees, we did a substantial rewrite of the Results and Discussion sections. The discussion was strengthened, new figures were included and complementary information was added as supplementary material. Below, we respond specifically to the *referee's comments (in italic).*

The manuscript "Rapid establishment of the CO2 sink associated with Kerguelen's bloom observed during the KEOPS2/OISO20 cruise" from Lo Monaco et al. describes obtained fCO2 underway data from an multidisciplinary research cruise around Kerguelen island. The authors analyze and interpret their data with regard to several driving mechanisms such as biological production, vertical mixing, micro- and macro nutrient supply and horizontal advection.
The manuscript is well written and results were mostly presented in a comprehensible way. However, the discussing of results can be sometimes a bit more streamlined. The content of this study fits well into the scope of BG. I suggest publishing this manuscript with minor revisions.

A. General Comments

A1) The way the authors presented their data for different subregions around Kerguelen is sometimes a bit confusing, particularly for those how are not familiar with the region. A uniform and more simple nomenclature for subregions should be used for the ease of reading (avoid confusion between plumes, blooms, middle, plateau, east of xy°E, etc.). Each map should contain margins of the discussed subregions (as done in Fig. 7b). Define distinct boxes would help to improve this.

Following the referee's suggestion, we identified the different regions on maps. Their characteristics are described in Section 3 (Hydrological context), before describing fCO2 distributions in different regions and periods.

A2) The availability of iron seems to be the dominating effect (next to vertical mixing), but nothing is shown about the iron distribution (only occasional citation of Queroue et al). Show at least a map of Fe surface distribution for relevant months (if that data is available). And what exactly is the process that transports iron into the surface layer?

We added information on iron concentrations measured during the cruise. The mechanisms identified to bring iron to the surface layer are internal waves and wind-induced upwelling. This is now mentioned in Section 3 (Hydrological context) with appropriate references.

A3) I wonder whether other islands and archipelagos in this ocean region have similar effects as Kerguelen. Please discuss briefly.

In the revised ms, we compare our results to observations collected around Crozet Islands where a recurrent bloom is also observed and likely controlled by iron availability (Crozex Experiment, Bakker et al., 2007).

A4) The methods used for the synthesis of available data for the study area (seasonal cycle of air-sea fluxes that required data processing of Quickscat and SOCAT data) are not presented in a comprehensible and traceable way. Please add some details. How robust is the seasonal picture that is presented at the end of the manuscript (especially for austral winter months with a potential lack of observations)? What is the resulted error for the derived annual CO2 uptake rate? This part should be a little bit more extended by the authors.

We added information on how we derived the seasonal evolution of air-sea CO_2 fluxes. The uncertainty associated with interannual variability and potential biases due to data distribution are also discussed. We evaluated the error on CO_2 fluxes estimates by performing random perturbations on fCO_2, temperature and wind speed data. It is usually less than ±2 mmol/m2/d during winter (when the flux is small), and around ±5 mmol/m2/d during the productive season (with flux usually >5 mmol/m2/d). The error on the annual flux is on the order of ±0.3 mol/m2.

B. Specific Comments:

B1) Page 3, line 5: Takahashi et al. 2012 gives a more detailed view on the southern ocean than the 2009 paper does

We changed the reference for Takahashi et al. (2012).

B2) Page 3, line 12 ff: The iron fertilization experiments should be discussed in a more controversially way. For instance, results from the LOHAFEX experiment indicated only minor effects on the uptake of atmospheric CO2 (Martin et al., 2013). Please extend this section.

We added some more information on the results obtained from different iron-enrichment experiments in the introduction. We refer to the syntheses by de Baar et al (2005) and Boyd et al. (2007) for more details.

B3) Page 5, line 10: Add one more sentence/citation about the characteristics of the polar front since the PF is part of your discussion to a major extent.

A new section (Section 3) was added to describe the hydrological context of the study, including characteristics of the PF.

B4) Page 5, line 22: Check date format and use it throughout the manuscript (e.g., October 17th)

Dates were corrected for a uniform format (Day Month).

B5) Page 6, line 1: I haven't seen any TCO2 underway data in your manuscript. I would like to see a plot of your pCO2 measurements overlayed with your 4h discrete TCO2/TA data (converted to pCO2). How well do both data sets agree with each other?

The comparison between calculated and measured fCO2 does not give any information about the mechanisms that drive variations in fCO2, and for this reason we believe it is beyond the scope of our study. Such a comparison was previously evaluated, for example by McNeil et al. (2007) because these authors then used reconstructed TA and TCO2 to evaluate air-sea CO2 fluxes in the Southern Ocean. Using OISO data (for both summer and winter cruises, and at the large scale), the mean difference between measured fCO2 and calculated fCO2 was 3.6 (±3) µatm (Fig. 4 in Mc Neil et al., 2007).
In the revised ms, we added figures showing TCO2 and TA data and we discuss the relationships between TCO2 and nutrients. In addition, we used TA and TCO2 data to evaluate the respective change on fCO2.

B6) Page 6, line 7 ff: Specify "standard gases". . . and an accuracy of 0.7 µatm would be way better than the commercial General Oceanics system. Either you have mixed up precision with accuracy or the 0.7 is only valid for the measurement of dried gases and not for water measurements. Further, please explain why all data were normalized to standard atm. Pressure. Finally, state an overall error of obtained fCO2 values.

We specified the xCO2 values and the origin of standard gases, and corrected accuracy for precision.
fCO2 data were normalized to 1013 hPa in order to correct for changes in atmospheric pressure, so that fCO2 variations are only driven by SST, TCO2 and TA. The overall error on oceanic fCO2 is on the order of ±2 µatm. This includes a comparison of ship-based and land-based atmospheric CO2, an inter-comparison of SST from 3 different calibrated sensors and the error on equilibrium temperature (< 0.05°C). This information was added in the revised ms.

B7) Page 6, line 9: I doubt that both, DIC and TA, were determined by a potentiometric method. Please correct and specify instrumentation used for analysis.

TCO2 and TA were measured simultaneously, in a closed cell, following a potentiometric method. We used an 'home-made' system during the KEOPS2 cruise, that was also used during many cruises, including all OISO cruises conducted since 1998 (data included and quality controlled in international syntheses such as GLODAP/CARINA, e.g. Lo Monaco et al., 2010; Sabine et al., 2010).

B8) Page 6, line 23 ff: Did you account for light-induced fluorescence quenching when calibrating your data? And, I didn't find any illustrated underway fluorescence data in your manuscript. Fig 6 only shows discrete chl-a data, I guess.

Underway fluorescence data are not shown due to the poor global correlation with Chl-a. We did not attempt to correct for quenching, because the relationship between fluorescence and Chl-a does not improve when using only night data. However, we used fluorescence data in two occasions in Table 1 (when no Chl-a data was available). These data were calibrated using the relationships obtained at the regional scale (better than the global relationship, notably in the PF zone: r^2=0.99).

B9) Page 7, line 2: CTD, already introduced on the page before

This was corrected

B10) Page 7, line 21: Please explain why a mean value for atm CO2 is used rather than your 4h data (which could be interpolated).

We used a mean value for atmospheric CO2 because it is very homogeneous in this region (at latitude 35S-60S) far from the continental influence (as observed during many cruises conducted in this region since 1991 and also well observed at the atmospheric monitoring station Amsterdam Island, 38S-77E).

B11) Page 8, line 1 ff: Be more precise on your climatological winds you derived from Quickscat. How many years were considered? How accurate is Quickscat data for that region, what is the estimated uncertainty?

We changed the wind data for MERRA that show a very good agreement with the ship-based measurements. We used the monthly data from 1991 to 2015 to calculate climatological winds. This information was added in the revised ms (as supplementary material).

B12) Page 8, line 26: Bathymetry in your figure is hard to distinguish. Consider using contour lines (incl. labels) rather than filled areas. . . this makes it easier to follow your discussion when talking about the different regions related to bathymetry.

We added contour lines and labels for bathymetry on maps.

B13) Page 9, line 7 ff: I had to read this paragraph twice to understand what you described. Consider rephrasing.

This paragraph was removed (the different regions are presented in the previous section).

B14) Page 9, line 18: Repeated sentence (see section 3.1).

This was corrected.

B15) Page 16, line 9: I didn't see anything in the methods section (2.1) about a drifter.

Please add (incl. estimated accuracy of data).

Details about drifters data and the deployment strategy can be found in the papers by Park et al. (2014, JGR), Zhou et al. (2014, BGD KEOPS2) and d'Ovidio et al. (2015, BGD KEOPS2). We added these references in the revised ms.

B16) Page 16, line 17: Please mark the eddy in the related figure and add information to the caption.

The term eddy was not strictly correct to describe the recirculation area within the PF meander. Results from d'Ovidio et al. (2015, BGD KEOPS2) actually show several eddies in this region. This was corrected in the revised ms.

B17) Page 17, line 18: Reword sentence. . . "vertical mixing due to light. . .". Also consider rephrase the paragraph. Apparently, you like to use long sentences which are not beneficial for the ease of reading.

The paragraph was rewritten. We were careful to avoid long sentences in the revised ms.

B18) Page 18, line 14: water column

This was corrected

B19) Page 18, line 19: ocean currents ("strong jets", lateral advection) are being used quite often for your discussion. It would be desirable to add 1 or 2 sentences to section 1 or 2.1 about the general current system in that region. . . this makes it easier to follow when you are talking about jets, up- and downstream etc. . . adding an arrow for the main current direction into a figure would also help.

We added a description of the hydrological context (including surface circulation) in a dedicated section early in the ms. Following the referee's suggestion, we also added the main circulation pathways in Fig.1.

B20) Page 20, line 14: ". . . we used the SOCAT database. . .", how exactly did you use the SOCAT data for deriving a seasonal picture? Where does the atmospheric data come from for Delta-pco2/flux data, what about inter-annual variability in temperature that affects the fCO2. Please add details about processing of this data to the method section.

We added detailed information on the computation of the seasonal cycle of air-sea CO2 flux. This includes figures showing interannual variations in fCO2 and SST. The atmospheric data for all selected cruises in SOCAT are derived from Global-View CO2 (this is a sub-product in SOCAT attached to each fCO2 data for corresponding latitude/time).

B21) Page 20, line 15: Fig7, color-coded cruises (month of year) is not ideal to illustrate the seasonal data coverage in that area. I suggest replacing panel c) with a kind of column diagram that, for instance, shows the no. of observations or no. of cruises for each month. This is more useful to assess the robustness of your seasonal cycle for CO2 fluxes.

The two maps in figure 7 were moved to the supplementary material, where we also added information on the number and the year of observations available for each month.

B22) Page 21, line 16 ff: again, one of those confusing sentences. . . please reword.

The sentence was shorten for clarity

B23) Page 21, line 21: downstream

This was corrected

B24) Page 22, line 5: How episodic are these events. . . how often do these high wind events occur? Does your wind climatology cover these events properly or are they averaged out?

In November 2011, we observed a deep mixed layer following an episode of strong winds between 15 and 20 m/s. Using the 3-hourly MERRA data over the study region, we found that about 25% of the winds observed by satellite over the productive season (from November 2011 to March 2012) were faster than 15 m/s, 12% were faster than 17 m/s, and less than 2% were higher than 20 m/s. The percentage of strong winds was minimum in November and December, and maximum in March (when almost half of the observations shows winds stronger than 15 m/s).
These recurrent events of strong winds are somehow included in the climatology since they impact the mean wind speed (monthly means are generally low in November and December and high in March).

References

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