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Interactive comment on "Rapid establishment of the CO_2 sink associated with Kerguelen's bloom observed during the KEOPS2/OISO20 cruise" *by* C. Lo Monaco et al.

Anonymous Referee #2

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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.

General Comments:

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.

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?

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

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.

Specific Comments:

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

2) 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.

3) 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.

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

5) 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?

6) 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.

7) 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.

8) 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.

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

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

11) 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?

12) 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.

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

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

15) 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).

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

17) 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.

18) Page 18, line 14: water column

19) 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.

20) 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.

21) 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.

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

23) Page 21, line 21: downstream

24) 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?

References:

Martin, P., et al. (2013), Iron fertilization enhanced net community production but not downward particle flux during the Southern Ocean iron fertilization experiment LOHAFEX, Global Biogeochem. Cycles, 27, 871–881, doi:10.1002/gbc.20077.

Takahashi, T., C. Sweeney, B. Hales, D.W. Chipman, T. Newberger, J.G. Goddard, R.A. Iannuzzi, and S.C. Sutherland. 2012. The changing carbon cycle in the Southern Ocean. Oceanography 25(3):26–37, http://dx.doi.org/10.5670/oceanog.2012.71.

Interactive comment on Biogeosciences Discuss., 11, 17543, 2014.