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Interactive comment on “Carbon, oxygen and biological productivity in the Southern Ocean in and out the Kerguelen plume: CARIOCA drifter results” by L. Merlivat et al.

Anonymous Referee #2

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Review of Carbon, oxygen and biological productivity in the Southern Ocean in and out the Kerguelen plume: CARIOCA drifter results, by Merlivat et al.

Merlivat et al, present the results of the deployment of the CARIOCA drifter during the Keops 2 over the Kerguelen Plateau experiment (Southern Ocean). The drifter provides some pCO₂ values that might be of interest. However, the authors convert the pCO₂ values in DIC in order to assess the Net Community Production based on a suite of assumptions. Some of them does not appear very robust to me. So in my mind, a careful assessment of how the uncertainty in the main assumptions (in particular the mixed layer depth) propagates through the series of computation is needed to assess the robustness of the estimates proposed in this study. Alternatively a comparison with

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other determination of Net Community Production or Net Primary Production carried out during the same experiment could give confidence in the assessment of NCP derived from the data provided by the CARIOCA drifter. Otherwise the assessment of NCP should be removed.

Major comment: CARIOCA drifters are good tools for survey of pCO₂ and related parameters. However, for determination of Net Community Production, it is necessary to make several assumptions. Among them, one of the most critical is the Mixed Layer Depth (MLD). In this study, MLD is set to 20m for all the estimates of Net Community Production (NCP) according to the study of Park et al. 2014. However, I did not find the information about the MLD in the paper of Park et al. 2014, so that it is difficult to assess how robust is this assumption, and what is the variability of the MLD, since this variability will affect the accuracy of the determination of NCP. In my mind, as the drifter is moving along the polar front, and possibly meanders or eddies, and then crossing the polar front towards the Subantarctic Zone, there is very little chance that the MLD remain constant. In line, in the study from Cavagna et al. in the same issue, it is stated that "Except for the HNLC reference station, the euphotic layer depth is relatively constant between stations while mixed layer depth varies significantly. The latter is generally deeper, more variable and extends more significantly below the euphotic layer over the Kerguelen plateau and at the HNLC reference station". Indeed in this study, the MLD range from 35 m to 120 m. In the same way, Jouandet et al assessed the MLD to be around 70m with an uncertainty of at least 15m. I acknowledge that the last study has been carried out south of the area covered by the Carioca drifter, and that the criteria for determination of the mixed layer depth are different. But still, to me, the assumption of a constant mixed layer depth is not supported by reports in this area, and 20 m may be an underestimate. This call for a careful assessment of the variability of the mixed layer depth in the area covered by the CARIOCA drifter, and how this uncertainty propagates in further computation and ultimately in NCP computations.

An alternate way to provide the reader with some clues about the robustness of the

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computation of NCP could be a careful comparison (a table with the number of both studies at similar sites) with other assessment of the NPP as the estimates provided by Cavagna et al. 2014 during the same experiment. This latter is potentially robust and implied less assumptions than in the current study. I acknowledge that Cavagna et al. 2014 address NPP, but still, in such pelagic environment, I do not expect so much differences. Comparison with other studies like the one of Jouandet et al. 2008, could have also been proposed to the reader.

The manuscript reads "With larger values of the MLD, the relative part of the air–sea flux in the DIC and O₂ measurements would have been smaller and make the slope of the oxygen–carbon relationship closer to 1". I agree, but the point is that the MLD is subject to change, and probably to increase. How this affects the discussion/conclusion related to the status of production (new vs regenerated) issued from PQ computed from fig 8 ?

Finally, If the changes in DIC over one day time corresponds to the NCP, I'm not sure what the evolution of DIC/O₂ during the daytime interval corresponds to. For me this latter correspond to something between Net and Gross primary production. Hence, at first sight, I would not mixed them up, and I would refer only to the changes in DIC from dawn to dawn.

Minor comments

P16878, L18. It not clear to me what the "mean" of fluxes correspond to. I would have indicated the range of fluxes.

P16881 L25. Even, I do not expect large shift in total alkalinity, what is the impact of change in TA on the assessment on DIC. What is the overall accuracy of the estimation of DIC ?

P16883 L7. Is there particular reasons to choose the formulation of Sweeney et al. 2007, instead of a widely used formulation like the formulation from Wanninkhof (1992),

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or a formulation that has been specifically developed for the Southern Ocean (Ho et al. 2007) ?

P16886, L17 "It is worth noting that the absolute values of the fluxes are larger for O₂ than for CO₂ due to the buffer factor of ocean water carbonate chemistry." I think that the differences in the Schmidt number for CO₂, and O₂ should also play a role in the differences in the fluxes, together

P16890, L 15 "Finally, the linear distribution of the data points (Fig. 9) demonstrates that our technique satisfactorily identifies the biological signature during the selected periods that we have considered." So far I understand, an error in the MLD depth should affect DIC and O₂ in a similar way, so that this does not provide so much information about the potential errors on the NCP. The dot should just moved along a line with a slope of 1.

P16890, L 26. "This is an issue regarding the in-situ estimates of NCP based on dissolved oxygen measurements at the ocean surface (Cassar et al., 2009) in high wind regions when the air-sea flux is large." That the reason why Cassar et al. 2009 are using O₂:Ar ratio. By measuring Ar they can somehow compensate the effect of physical processes (i.e. air-sea exchange, bubble injection...).

P 16892, L12 "Assuming that the value of NPP depends only on the stock of DFe, NPP in aged waters, respectively 35 and 50 days old, would be respectively equal to 205 and 91 mmol m⁻² d⁻¹ leading to NCP/NPP ratios respectively equal to 0.63 and 0.71. These values sound reasonable and indirectly support the choice of MLD equal to 20m." This is not a very robust assessment of what could be the NPP production. Also, what support the statement that the difference is reasonable. You might cite some other comparison found in the literature. I would have expected closer agreement.

Figures must be reordered according to the text. For instance in the text, the Figure 5 come first, then figure 1, then figure 3.

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Figure 4. I think that the figure caption must refer to the original paper of d'Ovidio et al. 2014. It is stated in the text, but this information should also appear in the figure caption of the figure

Figure 7. I found it difficult to see the purple dots and lines superimposed to a red curve. You may consider to choose another color.

Figure 8 & 9. At first sight, I would not mix day to day estimates with estimates over daytime, since the estimates over daytime does not correspond to NCP in my mind.

Typo P16880 L7, "Kerguelen" should be changed in "Kerguelen"

P16881 L8, "Hood and Merlivat,2001" should be changed in "Hood and Merlivat, 2001"

P16886 L22. In the subtitle, you should write either Dissolved Inorganic Carbon, or DIC

P16892 L9. replace decreasing by decrease

References:

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Ho D Law C Smith M Schlosser P Harvey M Hill P, 2006. Measurements of air-sea gas exchange at high wind speeds in the Southern Ocean: Implications for global parameterizations, Geophysical Research Letter, vol: 33 (16) pp: L20604

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