Re: "Diagnosing CO_2 fluxes in the upwelling system off the Oregon-California coast" by Zhimian Cao et al. (bg-2014-130)

Dear Editor,

Enclosed is a copy of the re-revised manuscript "Diagnosing CO_2 fluxes in the upwelling system off the Oregon-California coast" by Zhimian Cao et al.

In this re-revised MS, we have fully considered all comments from Dr. Debby Ianson. Revision details are described in the enclosure. We sincerely thank you and the reviewers for the constructive comments and valuable suggestions, which certainly improved the quality of the paper. We hope that this further revised MS will now meet the highest standard of Biogeosciences.

Sincerely,

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Enclosure: Response to reviews

Response to the comments from Dr. Debby Ianson

The revised version of Cao et al. is much improved and suitable for publication after minor revisions.
The issue (5) of excess DIC uptake and (4) of the time-scale for which their method is able to diagnose source/sink has not yet been addressed satisfactorily. I summarize these concerns and include a few minor comments that are tied to the same numbers as my in my original review and the authors response.

[Response]: We are pleased that Dr. Ianson is positive with our revisions and are grateful to her additional comments and suggestions, which have been fully considered in our re-revisions.

General comments

1. *re: Sensitivity analysis and X(eff):*
The sensitivity analysis is helpful and shows that the algorithm is relatively insensitive to the TA and DIC of the Columbia. I suggest pushing the sensitivity envelope further, i.e., allowing C:N ratios as well as the assumed (15 μmol/kg) NO_3 concentration in the CR to have a (reasonable) range when estimating DIC(eff). The authors could still have a maximum and minimum scenario (not more scenarios) but the DIC(eff) would span a larger (and not unrealistic) range.

[Response]: We have improved the sensitivity analysis by including the NO_3 variations of ~10-20 μmol kg^{-1} in the CR, which still showed the minor influence of the DIC^{eff} variations on our diagnostic approach (see Line 429-450 and Table 1 of the re-revised MS). On the other hand, the influence of the possible non-Redfield C/N uptake ratio would also be minor, due to the fact that along the pathway of the CR plume the DIC removal corresponding to the NO_3 depletion was merely a small fraction (~10%) of the absolute DIC contents.

2. *re: showing all depths for TA-S curves, and reconsidering the lower limit of analysis*
AND
3. *water mass context:*
Inclusion of the all the data is a strong addition, is more convincing to the reader (in fact changed the depth region of their analysis) and allows context to discuss the water masses.

[Response]: We are pleased that the reviewer is happy with this addition.

4. *re: Time scale of relevance of the analysis (source vs. sink annually, seasonally, or just during the week during which data were collected?):*
The authors have partially addressed this concern with minimal edits to the text. Overall the document is still misleading in this regard: e.g., abstract lines 26-30 in the abstract, in particular \for semi-quantitatively diagnosing the CO2 source/sink nature of an ocean margin,

highlighting.." reads like their method can determine whether a region is a source or sink, period.

[Response]: We have rewritten the relevant sentences clarifying the timescale concerned. See Line 27, Line 41, Line 61, Line 84, Line 155-156, Line 315, Line 359 and Line 511 of the re-revised MS.

5. *re: excess DIC uptake*
Adding some sensitivity analysis is most helpful, however the Fassbender et al. C:N of 7.3 used for this analysis is truthfully "about Redfield" and so adds little to the study. Excess DIC uptake, if and when it occurs, may result in significantly higher C:N (uptake) ratios. Furthermore, it's a non-linear process, primarily occurring when nutrients become limiting. It is true as Martz et al. state that treating a snapshot of data (as used in this study) at one location with a constant C:N ratio may be appropriate, but that ratio would not necessarily be Redfield, nor would it be constant in time. In particular I am not convinced that T4 C:N uptake would be near Redfield. Even if the authors do no more sensitivity analyses and adopt their results, the limitations of these results need to be more clearly stated.

[Response]: We believed that we had extensively addressed this issue in our last response and the revised MS. Moreover, the nDIC-nNO_3 relationship based on the data collected along the three transects indicated a C/N uptake ratio close to the Redfield value (Fig. 6). Per Dr. Ianson's comments, we have stated in our re-revisions more clearly the possible non-Redfield behavior (Line 504-507).

Specific comments

- (3) p.7392 l.7 - eNP - add "Subtropical Gyre" to distinguish from Alaskan Gyre - eNP.

While the authors address the comment by deleting the original statement, I still suggest that they spell out "Subtropical Gyre" the first time that they define the "eNP" in the text (line 99).

[Response]: Modified as suggested. See Line 105, Line 143, Line 172-173, Line 225, Line 291 and Line 513 of the re-revised MS.