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.

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 umol/kg) NO3 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.

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.

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.

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, its 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.

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).