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Interactive comment on “Diagnosing CO₂ fluxes in the upwelling system off the Oregon coast” by Z. Cao et al.

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Cao et al. use a relatively simple end-member mixing scheme to estimate the air-sea CO₂ flux from two transects in the coastal upwelling zone along the west coast of North America during the spring of 2007. This method has been used elsewhere but not yet applied to coastal upwelling.

General comments

This work uses an end-member scheme that relies on conservative behaviour of total alkalinity and salinity (and a suite of other assumptions nicely laid out in the Wan-

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ninkhof review). The simplicity of the method is appealing and a useful tool to investigate CO₂ fluxes, at least to first order, as long as assumptions are not violated (which is tricky in coastal upwelling zones as the authors point out). I have a few recommendations/comments (below) mainly involving the addition of sensitivity analyses, which would add significant value to the interpretation.

1. Since upwelling regions push the assumptions involved I suggest a sensitivity/error analysis/discussion be added. The limitations of the method could then be discussed in a quantitative fashion and perhaps boundaries on its utility imposed. For e.g. the X^{eff} terms are nicely discussed in the context of CR data and TA intercepts (with S) but then a single number is used for each of DIC and NO₃ (eff).
2. Including data below 200 m (or below a salinity of 34) in the TA-S plots in this dataset make the determination of the 'end-member' a little murky in my opinion. At about S=34 (a little shallower) the TA-S 'curves' become more steep in these data, presumably getting into the California Undercurrent and/or aragonite dissolution (relatively shallow in this part of the world). I don't dispute the author's choice of regression and end-member necessarily - but again recommend a sensitivity analysis (choose a range of end-members) and suggest showing the data below 200 m in Fig. 2 even if they are coloured differently (which could convince the reader that the end-member really was tight and unambiguous).
3. Related to the previous point, the authors do not discuss the California Undercurrent (CUC) and its unique properties. At least along T4, the core of the CUC is above 200 m (Thomson and Krassovski 2010 JGR) and must be present at some stations on the transect.
4. The authors discuss the method in the context of determining net source and sink (for CO₂) regions in a general sense - but with two transects (single visit)

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they are only able to look at a couple of snapshots in time in a system with large spatial and temporal variability. They would need seasonal data, in particular from the winter (downwelling) season where PP is light-limited on T4, to make a firm assessment of the source/sink capability of a region. (The steady state assumption is clearly violated.) This caveat should be more clearly stated.

5. There is evidence for 'excess' DIC uptake (uncoupled from NO_3^{-1}) when phytoplankton become nutrient limited (Ianson and Allen 2002 GBC and Druon et al. 2010 ECSS model this uptake - but the concept is much older - e.g. Sambrotto et al. 1993 Nature) which would affect the estimation of air-sea flux if present. For these transects (esp. T5) this feature may not be an issue (although on T4 depletion of silicic acid suggests that it might), but it should at least be discussed in the paper and again sensitivity analysis would be valuable.

Specific comments

1. p.7391 l.2-5 for general source/sink discussion need seasonal context (state)
2. p.7391 l.13 - why mention Ca ion if not measured?
3. p.7392 l.7 - eNP - add 'Subtropical Gyre' to distinguish from Alaskan Gyre - eNP.
4. p.7392 l.15-20 - a good place to mention the possibility of excess DIC uptake
5. p.7393 l.3 300 m seems too deep for CC ?
6. p.7393 l.9 - CUC is shallower than that (Thomson and Krassovski 2010 JGR)
7. p.7993 l.13 - I work further north, but these upwelling depths look too deep. Are you sure?

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8. p.7396 l.1 - remove 'very good' or be quantitative
9. p.7396 l.9 and l.15 'should' - why 'should' they? do they? suggest more explanation or better word choice.
10. p.7397 l.2-3 - given that its an upwelling zone wouldn't it be smarter to use a set isopycnal instead of depth? and I would choose a shallower depth in this zone as my end-member to stay in a linear zone of TA-S etc (see general comments above).
11. p.7398 l.5 (and further) what about non-Redfieldian C:N?
12. p.7399 sect.3.3 - again - consider 'excess DIC' uptake

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