

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

Anonymous Referee #1

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Cao et al. apply a simple framework (OceMar) based on carbon/nitrate mass balance to semi-analytically predict CO₂ air-sea fluxes in the upwelling system off the Oregon coast. They find that they are able to represent observed fluxes in regions of the shelf that act as sinks of CO₂, but are unable to represent the source regions following the OceMar approach. Then, they add one extra assumption to the applicability of the method, namely the requirement of steady state conditions. I think the manuscript is interesting and worth of publication after some main issues are addressed.

I agree with Dr. Wanninkhof’s comment about the overall applicability of the method and think that the manuscript would benefit from an expansion of this discussion. Moreover, the source data the authors described (Feely et al 2008, Feely and Sabine 2011) show many more transects along the California Current System that may provide more

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insight into the un/applicability of OceMar in wind-driven upwelling margins (in particular, regions to the south, further away from the Columbia River). I am wondering whether the authors could incorporate some of these transects or otherwise explain their chosen focus on the Oregon region. In the latter case, they should discuss whether they would expect OceMar to work in the regions to the south and north of Oregon within the California Current System.

Another suggestion would be to test the robustness of the results with respect to the choice of the carbon to nitrogen ratio. The authors could perform a sensitivity analysis where they repeat their calculations replacing the Redfield ratio by some deviations of the 6.6 value (maybe observed ranges of C:N in the region?).

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Specific comments:

The abstract would benefit from a slightly more detailed explanation of the OceMar framework.

Also in the abstract: I found lines 15-17 a bit misleading (“we showed significant CO₂ outgassing in the nearshore regions associated with intensified upwelling and minor biological consumption. . .”). In my opinion, it reads as if the method was able to capture the outgassing, while actually the pCO₂ observations showed the outgassing and the method failed to reproduce it. Then, the authors argued for a modification to the method to address this issue.

Page 7393, line 12-14: the region of interest is part of the California Current System. The southern part of this system has permanent upwelling-favourable winds.

Page 7397, lines 15-18: I’d suggest being explicit about how X_{eff} is calculated, so the reader doesn’t have to dig into Dai et al. (2013) to understand this step in the calculation. The explanation could be added as an appendix.

Page 7400, line ~26: why are these values not shown in figure 3?

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Technical comments:

Every now and then I had problems with specific sentences that I think could be improved, e.g.:

Page 7390: lines 10-11: English here could be improved - besides, this text is part of a really long sentence, line 10 to 15!

Page 7393: line 7: should say “broad” instead of “board”

Page 7394: line 18: “to quantify the conservative portion of carbon and nitrate.”? (or nitrogen nutrients, but I think the authors only use NO₃ in their calculations).

Page 7395: line 16: “parameters” should be replaced by “variables”

Interactive comment on Biogeosciences Discuss., 11, 7389, 2014.

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