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Biogeosciences Discussions

Interactive comment on "Coastal-ocean uptake of anthropogenic carbon" *by* Timothée Bourgeois et al.

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1 Summary

Bourgeois et al. use an eddy-permitting global ocean biogeochemistry model to investigate the relative contribution of the ocean margins to the uptake of anthropogenic CO_2 from the atmosphere. They find that these regions take up a disproportionally low amount of anthropogenic CO_2, i.e., only 4.5% relative to this regions areal contribution of 7.5%. The authors suggest that it is the limited degree to which the anthropogenic CO_2 is transported and mixed offshore that leads to this low uptake. These results are in stark contrast to earlier studies based primarily on a few point observations that suggested a very high coastal ocean uptake of CO_2 from the atmosphere.

2 Evaluation

Understanding and predicting the future evolution of the oceanic sink for atmospheric CO_2 is of paramount importance for determining how much CO_2 we can emit in the coming decades without exceeding any climate target. While our confidence in the net exchange fluxes of CO_2 over the open ocean has increased substantially in recent years thanks to better observations and novel methods to interpret these data, our ability to constrain the fluxes along the continental margins has not increased commensurably. The existing observational constraints are still relatively weak and associated with sizeable uncertainties. By far the most extensive and detailed assessment to date by Laruelle et al. suggested a relatively small global net uptake of atmospheric CO_2, but their data-based approach did not permit them to separate this net flux into its "natural" and "anthropogenic" components.

Thus the study by Bourgeois et al. is much welcomed as it brings a consistent global perspective to the problem. Their model-based approach is far from perfect, owing to many issues ranging from its still coarse resolution to the limited of consideration of several processes that are of relevance in coastal systems (e.g., limited consideration of benthic processes), but by using - for the first time - an eddy permitting 3D model, it is a big step up from previous model based approaches that used highly simplified models. I particularly like that the authors spend a considerable amount of effort in order to assess their model's performance against observations and that they run two parallel simulations in order to determine the anthropogenic CO_2 uptake explicitly. The paper is overall well written, adequately illustrated and referenced. The discussion is generally thorough and the conclusions supported by the provided evidence including an appropriate consideration of the caveats. All in all, this is a very good study that is well suited for publication in *Biogeosciences*. However, before giving the green light, I would like the authors to consider my concerns regarding important model shortcomings.

- (i) *Model drift*: The model's drift is quite substantial, and I am not entirely convinced that the authors have fully considered the implications when discussing their results. I think the drift is large enough that it cannot be ignored.
- (ii) *: Reflective bottom boundary:* The model's lower boundary is assumed to "reflect" any settling organic matter back into the water column in remineralized form. Thus, this model does not incorporate any delayed response of remineralization nor any other benthic remineralization process of relevance such as benthic denitrification etc. This is a rather important omission, as these processes influence coastal biogeochemistry in many shallow marginal seas. I thus recommend to address and discuss this issue in somewhat more detail and to give it better consideration.
- (iii) *: Coastal-open ocean exchange:* This issue is actually addressed a bit more than the other two in the current version, but I still consider it worthwhile to assess the implications of this shortcoming in more detail.

3 Recommendation

I recommend acceptance of this manuscript with minor revisions.

4 Minor comments

Abstract, line 10: "absorbed by the coastal ocean": I suggest to define also in the abstract how the authors define the "coastal" ocean.

p1, line 15: "the ocean naturally mitigates..." I am not sure why the authors use the expression "naturally" here. I suggest to delete it.

p5, line 5, equation 2: The authors still use a coefficient of 0.30, while there are numerous studies that have shown that this coefficient needs to be lowered in order to close the oceanic C14 budget. It's time to change, no?

p5, lines 17 to 25: By neglecting the oceanic uptake of anthropogenic CO over the period 1750 until 1870, the authors underestimate the total anthropogenic uptake by about 10% or so. This should be taken into consideration more explicitly. I also suggest to avoid the use of the term "preindustrial" when referring to 1870.

p6, line 14: "as compared to the estimate of natural carbon outgassing of 0.45 Pg C yr-1". I find this comparison confusing, since the two processes are clearly distinct. The first number is the drift of the model, while the second one refers to the outgassing of river-derived carbon from the ocean. I do not think that these two numbers can be compared as done here.

p8, line 14: "Regionally, [the] overall patterns in the air-sea CO_2 flux are similar between the ..." How was the large model drift considered when making this statement? This issue is mentioned further down (line 24), but not really elaborated.

p9, lines 16-17: "Correlation [..] only 0.5 for area-integrated fluxes". How important is the remapping error here? (see my comment on Table 2).

p11, lines 23-24 ".. carbon sink in the Amazon river plume [..] is not reproduced". I think that this also has something to do with the model's shortcoming with regard to the benthic-water column interactions.

p14, lines 1-3: "... is transported offshore to the deeper open ocean". This deserves a little more discussion, particularly since a few lines below (e.g., lines 16-17) this issue is identified as "a critical question".

Table 2: some of the areal discrepancies are huge. I thus think that one needs to be very careful when comparing areally integrated fluxes. In fact, this should not be done, in my opinion, if the areas differ by more than let's say 10%.

Figure 2: reduction in uptake during the 1940s. Where is this coming from? To me, this looks like the model is overly sensitive to the rate of change in atm. CO2, perhaps due to the drift.

Figure 6: See also my comment about Table 2. I am not sure whether it is really appropriate to compare the areally integrated fluxes when the areas are that different to begin with.

Nicolas Gruber, April 2016

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