Biogeosciences Discuss.,
doi:10.5194/bg-2016-57-RC1, 2016




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

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Received and published: 18 April 2016

The ms. by Bourgeois et al. is the very first attempt to quantify the air-sea CO2 flux for the global coastal ocean using a highly-resolved 3D model. The authors compare in a convincing way their model results with observational data and discuss in detail the obtained spatial variability in the air-water CO2 exchange. The approach is well described and model results are solid ; I am thus very supportive of this research. In addition, the authors have attempted a quantification of the anthropogenic perturbation on the air-sea CO2 flux, with the key finding that the magnitude of the perturbation could be significantly smaller than previously taught. This is obviously an important result that further strengthen the value of this contribution. However, the latter aspect has several shortcomings that I believe need to be adressed fully (see in particular major comments 2 and 3) before publication.



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Major comments

1) Uncertainties are only reported once for the anthropogenic CO2 flux (0.1 +- 0.01). You need to explain how this uncertainty has been estimated. It is also much lower than the uncertainty on the total simulated flux (0.27 +- 0.07), which is quite surprising. More generally, uncertainties and their quantification method should be reported for all fluxes and consistently throughout the text.

2) Section 4.1.2 on anthropogenic fluxes provides a suitable comparison with previous estimates. However, the last paragraph is misleading as one of the key reason why the size of the perturbation could be larger in Mackenzie and co-workers is the stimulation of the biological pump by enhanced land-derived nutrient inputs. These aspects should be included in the discussion, but also much earlier in the text (introduction and, eventually, title). That is, the authors should clearly state right from the start that they only consider atmopheric CO2 as their sole anthropogenic driver. As a result, I believe that only the physical dissolution pump is impacted, i.e., the model should simulate constant net ecosystem productivity (NEP) and (I suspect) constant net ecosystem calcification (NEC) during the entire historical period. The values for NEP and NEC should be reported and discussed (a subject of intense debate within the coastal C community) as this could be (another) plausible reason for the discrepancy with earlier estimates. Finally, nothing is said about temperature effects on the uptake of CO2. This aspect should also be included in the description/discussion.

3) Section 4.2 provides an explanation for the smaller relative magnitude of the global coastal anthropogenic CO2 uptake compared to the global ocean. As it is, Figs. 2, 5 and 8 do not satisfactorily substantiate the proposed mechanism. What is missing are plots of temporal evolution of (organic and inorganic) carbon accumulation (also % relative increase) and cross-shelf export for the entire simulation period. The Revelle factors shoud also be reported. Based on the proposed mechanism, I would suspect to see a progressive decrease of the ratio of anthropogenic carbon uptake of the coastal ocean to the global carbon uptake due to the accumulation of anthopogenic CO2 in

the coastal water column through time and this does not seem to be the case (Fig.2). I also would suspect to see a progressive increase in the Revelle factor (faster for the coastal ocean than open ocean) through time. In addition, it would be interesting to brielfy discuss why the uptake fluxes per unit surface area for the shelf seem to be larger than for the open ocean under pre-industrial conditions (fig.5). Furthermore, the authors should report the calculated horizontal cross-shelf transport of water as this is a crucial number to sustain their conclusion (a first sensitivity analysis could have been useful in this context). Finally, I believe that comment 2 above (focus on the physical dissolution pump only) is also relevant in the context of section 4.2 -

Other comments

Abstract and conclusion : the authors should also summarize the main results on the total fluxes as this is the first time that a model-data comparion is performed with a physically-resolved model at the global scale.

Abstract page 1, line 8 : a high resolution is required not only to resolve the bathymetry, but also the complex coastal currents (which in my opinion are not all induced by the bathymetry)

Page 2 line 4 : I suspect that the word export refers to ‘export production’. I would clarify because in the context of this paper, it could also refer to ‘cross-shelf export’

Page 2 line 5 : the carbon export and burial fluxes are highly uncertain – see, e.g. Krumins et al. 2013 (BG) for a review. The same also holds for the productivity (even the sign of the NEP is uncertain – see Bauer et al., 2013. It is thus not correct to state that the air-sea CO2 flux is the most uncertain of the C fluxes for the coastal ocean.

Page 2 line 15-16 : I agree about the CO2 switch of the coastal ocean from source to sink, but do not agree fully with the proposed attribution. MacKenzie and co-workers highlight the change in NEP (from enhanced land nutrient inputs) as one of their key driver to explain the shift (see aslo Regnier et al., 2013 & Bauer et al., 2013 – for reviews). Please clarify (see also major comments).

Page 3, line 20 : I recommend making reference to the few published long time series of CO2 observations (> 1 decade) for the coastal ocean. I agree nevertheless that these time series alone are sparse and short. Thus, an observation-based global extrapolation of the anthropogenic component is highly uncertain

Page 4 line 24 : calcite particles are included. This is not a satisfactory description. Please state clearly if your model accounts for calcification as this process has a potentially important impact on the air-sea CO2 exchange (see also major comments).

Page 4 line 29 : do you mean atmospheric deposition ?

Page 6 line 4 : Assuming that land derived DOC is entirely labile is a strong assumption. The flux (0.15 PgC yr-1 from the top of my head) is also significant. Thus, the extend to which your results depend on this assumption has to be discussed.

Page 6 lines 11-19 : The implication of a model outside of ' equilibrium ' has to be addressed. For instance, when you refer to a global ocean anthropogenic uptake of 2.3 PgC/yr-1, this number is obtained with a natural flux of -0,33 PgC yr-1 for the natural flux. Correct ?

Page 7, section 2.4 evaluation dataset : To leave no ambiguity, did you compare your model results with LA14 using the Wanninkhoff 1992 formulation or the updated formulation ?

Page 9, lines 12-14 : I would say 'weak carbon sources' and 'strong carbon sinks'

Page 9 lines 15-25: The phrasing is misleading (' our model results tend to underestimate total carbon flux, with 76% of the simulated specific fluxes lower than the data-based estimates '), as the absolute fluxes are actually larger in the model (i.e. larger negative sinks). ' Likewise ' is also not appropriate because the Arctic region is in fact the only latitudinal band where the model results predict a smaller sink than the observations. More broadly, I find that the results are quite comparable for the southern hemisphere and the low latitude regions, but that discrepancies are significantly larger in the Norther hemisphere with a stronger sink modeled for the 30-60°N and a weaker sink modeled for the > 60°N latidudinal band (see also Fig 3 of LA14). Also, the fact that the areal-integrated fluxes show a weaker obs-model correlation than the fluxes per unit surface area requires discussion.

Page 10 line 14 : what do you mean by ' top two regions ' ?

Page 10 line 25 and further: It is important to state that (to my knowledge) only LA14 accounts for the sea-ice cover in the global estimates - this is an important effect on the quantification.

Page 11 line 5-10 : I agree that the exclusion of the proximal zone in the model assessment should have an impact on the sign of the flux under pre-industrial conditions. But what about the effect of the initialisation (the value of the sink is not reported for the coastal ocean in 1850) ? Stated differently, is the global coastal ocean in equilibrium at the onset of the simulations ? Regarding the proximal zone, bays, estuaries, deltas, lagoons are indeed sources of CO2 (see Laruelle et al., 2013 for the latest synthesis), banks should be too (a reference would be useful), but I am not sure about what is meant by ' marine wetlands '. If this refers to marshes and mangroves, they are then believe to be sinks for atm CO2 (see Cai, 2011, Regnier et al., 2013, Bauer et al., 2013). Thus, clarification is required here.

Page 11, section 4.1.2 first paragraph : I believe that regional scale studies have attempted an estimation of the anthropogenic CO2 uptake in EBUS. If true, they should be included in the discussion.

Page 11 line 21-22 : This sentence has to be rephrased as it implies that one modeling approach performs better than another. Please tone down.

Page 12, section 4.2 : the first two paragraphs on total fluxes should be merged with section 4.1.1 – Regarding the Amazon, what is the potential impact of assuming that



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all the DOC (a large flux) is transformed into DIC in this region ? More generally, do you assume that this instantaneous transformation has no impact on alkalinity ?

Page 12, section 4.2 : the latidudinal trends in anthropogenic CO2 fluxes are also very similar for the coastal and open ocean (Figure 5). This aspect needs to be discussed.

Page 13, lines 12-19 : The computation of Revelle factor values is interesting, but it is important to stress that (to my knowledge), a higher value for the global coastal ocean compared to the global ocean remains highly speculative as this has not been demonstrated from observational data. Also, - and this is an important point – the sentence ' That finding is consistent with the lower simulated specific fluxes into the coastal ocean ' is not convincing. At the end, the Revelle factor should influence the total fluxes (and not its anthropogenic component) for which the area-based estimates indicate significantly larger negative sinks than in the global ocean (fig.5), i.e. the opposite of the anthropogenic component fluxes.

Page 13 lines 23-24 : The chemical factors are presented as independent of the physical factors controlling the air-sea CO2 exchange. However, based on the model construct, I feel that the higher Revelle factor for the coastal ocean precisely results from the physics of the coastal zone, with a progressive accumulation of DIC due to weaker cross-shelf export than CO2 air-sea exchange.

Figure 8 : I assume that fluxes refer to total anthropogenic fluxes, i.e. organic plus inorganic carbon – please clarify.

Spelling

Page 1, Line 10-11 rephrase – this sentence is odd

Page 2 line 4 : remove 2nd 'relative'

Page 4 line 25 : not sure that 'model' can be used as a verb

Page 10 line 19 : remove 'that'

Page 14 line 14 : remove ’of’

Page 14 line 24-5 : parenthesis wrongly placed

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Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-57, 2016.