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Interactive comment

Interactive comment on "Carbon cycling in the North American coastal ocean: A synthesis" *by* Katja Fennel et al.

GRUBER (Referee)

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1 Summary

Fennel et al. assess and synthesize a large suite of observation- and model-based studies in order to derive regionally specific carbon budgets for the coastal regions around North America, with a special focus on the air-sea CO_2 flux. They find that the entire North American coastal ocean acts as a sink for atmospheric CO_2, currently taking up about 160 $\pm$ 80 Tg C yr^{-1}. The majority of this total sink stems from the high latitude regions, as the sink strength of the temperate and subtropical latitudes is relatively weak. Fennel et al. also attempt to assess trends in these fluxes and in ocean acidification, but the number of studies available was too small to draw clear

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conclusions.

2 Evaluation

Even though the potentially substantial role of the coastal ocean to the global ocean carbon sink is well recognized, the quantification of the carbon fluxes in coastal regions has remained a large challenge. Despite a large increase in the number of observations and modeling studies, the data remain sparse in comparison to the large spatial and temporal variability that characterize these regions. Given these challenges, it is no surprise that the majority of the published studies focus on a single region or a small set of regions, with only a handful of studies having attempted to construct a larger scale synthetic view of the coastal ocean.

This is the opportunity that this study capitalizes on. It assembled a large suite of local to regional studies in the coastal regions around North America, assessed them, and synthesized them into a set of coherent estimates for the entire continent. Thus, such a synthesis is a highly welcomed addition to the scientific body of literature. Even though - by definition - there is only a limited amount of truly novel information in such a review, the synthetic aspect of this work is very important and fully justifies the potential publication of this manuscript. I commend the authors for their detailed efforts to bring the different studies together and to put them into a common framework for discussion and synthesis. The approach taken, and the conclusions drawn from them are clear and follow standard protocols. The manuscript is overall well written, with a set of clear messages. I am thus very supportive of the publication of this manuscript. But in addition to several specific points listed below, I have two major comments that I would like the authors to consider before giving my final ok:

- (i) *Length, Scope, and overall balance*: The paper is quite long and at the same time unbalanced across the different parts in terms of depths and insight. Overall, it feels as if the paper was taken in relatively unmodified form from the SOCCR2 report without thinking too much about the opportunity that the publication in a peer-reviewed journal would provide in order to push the synthesis and discussion further. I thus highly recommend to revisit the paper with fresh eyes, consider its scope, and to rewrite/edit it vigorously with the aim to enhance its impact.

For example: The first part of the paper contains a lot of rather basic review elements. Some of this material is certainly needed, but many of the things that are described are rather basic and often not even used later in the assessments. I thus suggest to prune this section rather strongly. This should be taken also as an opportunity to enhance the referencing in this section.

In contrast: The discussion is relatively brief, missing the opportunity for bringing new knowledge to the table. For example, the air-sea CO_2 flux estimates are not put into the context of the fluxes in the adjacent open ocean. Are these flux densities unusual compared to the open ocean? As far as I can tell, both the magnitude and pattern of the flux densities in the coastal ocean are, in fact, not that different from those we see globally across the whole ocean. I think this is worth discussing. Also, there is much interest to better understand the role of the coastal ocean as part of the "aquatic continuum", i.e., the lateral transport of carbon (and nutrients) from the land via river systems to the estuaries, and then through the coastal systems into the open ocean. This is briefly touched upon in the paper, but not further elaborated. Finally, it would be interesting for the average reader to get a sense what the 160 ± 80 Tg C yr^{-1} means compared to other important carbon fluxes, e.g., fossil emissions over North America, uptake by the North American continent, etc.

I also suggest to strengthen the conclusions. I think it would be good to know from this distinguished set of researchers what they recommend in terms of research/observational efforts. Which research aspect should be strengthened, which new types of observation should be put in place, or which impact should be studied in more detail?

- (ii) *Synthesis*: This concerns the approach taken in section 3.5 in order to arrive at a synthetic view of the North American coastal air-sea CO_2 fluxes. The authors decided - after having done a fantastic job to synthesize the regional fluxes - to mostly forgo the regional studies and rely primarily on the model study of Bourgeois et al. to arrive at a best estimate for the whole continent. This is a wasted opportunity, in my opinion. I think the authors should bring forward the local studies much more prominently and attempt to truly synthesize the more regional studies with those that take a more top-down approach. Concretely, I suggest to replace the Chen et al. (2013) numbers in table 1 (they upscaled a few point studies only) with a summary from the regional studies shown in Figure 3 (Table S1) and then arrive at a synthesis estimate by combining the three sources of information, i.e., the SOCATv2 based estimate by Laruelle, the model based estimate by Bourgeois, and the regional estimates from this study. This would give this work much more impact in the end.

3 Recommendation

I recommend to accept this manuscript after moderate revision. The core of the manuscript is solid and well described, but there is substantial potential for enhancing the impact of this paper by strengthening the discussion and conclusion sections at the expense of the background section.

4 Specific comments

Abstract line 10: "not well constrained": It is unclear whether this is a statement about the formal uncertainty estimate ($\pm$ 80 Tg C yr^{-1} or whether this is a statement of confidence, i.e., how uncertain are the authors about this uncertainty statement.

Abstract, general: The abstract is written in a relatively general and rather descriptive manner. It would benefit greatly from being more quantitative, and from making the link to the underlying processes.

page 3, lines 10 to 20: This set of clear definition is very helpful. Yet later on, when the different studies are compared and assessed, it is not always clear how the originally reported numbers were then rescaled/transformed to refer just to the EEZ.

page 4, Figure 2 and text: The perspective taken here is one that is almost purely one-dimensional in lateral direction, whereas in reality, alongshore transports and the vertical structure of the exchange with the adjacent open ocean matter a great deal (see e.g., Frischknecht et al. 2018). This is not that relevant for the air-sea CO_2 flux, but matters greatly for any discussion about the nature and magnitude of the lateral exchange of inorganic and organic carbon within the ocean.

pages 6-17, entire section 3: As mentioned above, it would be helpful to know how the numbers from the original studies were re-scaled or transformed into the numbers reported and discussed in this section.

pages 6-17, entire section 3: It would help the reader if the individual sections were structured in a similar manner. While the air-sea CO_2 flux is clearly always in the center, the different sections treat the other elements, such as the processes explaining the fluxes, and in particular, the lateral fluxes of organic and inorganic carbon very differently.

page 6, line 10: "The anthropogenic component of a given carbon flux is defined as the difference between its preindustrial and present-day fluxes." This definition of the anthropogenic carbon component differs in an important manner from that adopted for global ocean carbon studies (see e. g., McNeil and Matear (2013).) With this very broad definition, the authors include also changes induced by natural processes as "anthropogenic". In contrast, the open ocean community tends to define the anthropogenic carbon component as just this part of the flux/change in storage that occurs as a result of the increase in atmospheric CO_2. I am painfully aware that there is no generally accepted definition, but I would argue that the chosen, very broad definition of "anthropogenic" is in the end more confusing than illuminating. This is particularly the case since the Bourgeois et al. paper used to constrain the anthropogenic CO_2 component follows the more classical open ocean definition (see there comment on page 4171).

page 7, Figure 3: I very much like this figure. I suggest to color code the different estimates to emphasize the very different types of estimates. For example, by differentiating between model- and observation-based estimates. And then one could further differentiate between those that really represent a long-term mean flux, versus those that pertain to a single year only, etc.

page 12, line 9: You may want to consider adding the Frischknecht et al. (2018) study here. They show that about a third of the organic matter produced in the first 100 km gets transported offshore.

page 13, lines 24-27: This paragraph does not fit here. I recommend to move it to the trends section.

page 14, section 3.5: I urge the authors to reconsider their decision to downplay their regional estimates that much (see my second major comment above).

page 15, figure 5 and text: I very much like this synthesis of the fluxes. But I also think that the authors need to caveat this in an important manner. First, the uncertainties are probably underestimated given the unrepresented error contribution from temporal

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variability and other systematic error sources. Second, this implicitly assumes some form of steady-state. And third, there are probably very large regional differences hidden beneath this continental summary. It is probably worth to open the Pandora box a bit with regard to the regional aspects here, since I would expect that it is primarily the Arctic/high latitude regions that contribute to this net offshore transport.

page 17-18: Section 4: Relative to the previous section 3, this section could benefit from some better structuring. It is also not so clear what message the authors want to convey except that "things are inconclusive". I think that more is possible here.

page 17, lines 20 through 23: "by definition, changes in total carbon fluxes imply changes in anthropogenic fluxes as well." See my comment above about the definition of anthropogenic CO_2. I suggest to reconsider this very broad definition of "anthropogenic CO_2".

page 18, line 6: "pCO2 has increased faster than in the open ocean due to the combined effects of atmospheric uptake..." What is the evidence for this process being important? Such a behavior is not easy to achieve in a system that is generally outgassing - here, only a reduction in the surface residence time could achieve this. Thus, the much more important process explaining the higher than expected trend is ocean circulation and biology.

page 18, lines 10ff: "Some studies suggest that trends in the air-sea pCO_2 gradient (ΔpCO_2) are indicative of a strengthening or weakening of the net CO2 uptake by shelf systems...". This is not quite correctly formulated. A trend in ΔpCO_2 does not "indicate" a trend in the air-sea CO_2 flux, but actually "implies" a trend.

page 18, lines 20 through 26: I don't agree with the line of arguments here (or at least how I understood them). The authors seem to suggest that because the anthropogenic trend cannot be detected before 30 years have passed, the trends by Laruelle are likely not real. In my opinion, the discussion needs to be structured much more carefully. First, the trends detected by Laruelle et al. (2018) cover a rather short period, and thus are much more likely the result of internal variability than the result of an anthropogenic (forced) trend (in climate). Second, the time of emergence described by McKinley et al. (2016) checks for the emergence of a forced climate change signal versus that driven by internal variability. This does not imply that there is no trend to be detected, for example, in the Delta pCO2, if this trend was solely caused by the increase in atmospheric CO2, i.e., the uptake of anthropogenic CO_2 (sensu strictu). Please reconsider the writing of this paragraph.

page 19, lines 4 and 5: I don't know of any occasion where the impact of the changes in wind-stress on the gas transfer velocity had been more important than the impact on the ocean circulation. Thus, this sequence, i.e. gas exchange first, circulation second, does not reflect the common situation.

page 19ff, section 4.2: As section 4.1, this section could benefit from some better structuring. It largely consists of some general statements about ocean acidification, but does not really advance the knowledge much. It would greatly benefit from some more quantitative assessments of the distribution and the trends in OA.

page 20, line 4: "...biological production and respiration". Even more important is ocean transport and mixing. (see the next sentence)

page 20, line 19: "events". Here is one example of why a better structuring of this section is needed. This sentence starts without context, and as a reader one is baffled about the expression "events", when the subject is long-term trend. etc.

page 20, line 21, "Polar regions are naturally prone to acidification because of their low temperatures" This sentence is problematic for two reasons. First, the polar regions are not really prone to acidification per se (there sensitivity to change is actually lower, e.g., del pH/del pCO2), but what makes them prone is them being much closer to critical thresholds. Second, this sentence implies that their low pH or low Omega is a result of the waters being cold. This is not really correct either. It is because these waters have a high DIC/Alk ratio. And yes, the low temperatures are one of the reasons for the high DIC/Alk ratios, but also the freshwater inputs, the much more common mixing with deeper high DIC/Alk waters (owing to remineralization), and their generally low Alk are important factors. This is readily illustrated if one contrasts the pH and Omega values in the Atlantic to the Pacific at the same temperature.

page 21, lines 34ff: "The main contributing factor to the relatively high rates of acidification in polar waters is retreating sea ice, which adds meltwater from multiyear ice and increases the surface area of open water, thereby enhancing the uptake of atmospheric CO2 (Cai et al., 2010a; Steiner et al., 2013)." This sentence is another good example for the need to better structure this section. It makes a statement about the cause of the faster trend for OA in the Arctic even before a statement has been made that the trends are actually larger. Thus, across the board in this section, it behooves the authors well to first establish a good base line of the expected trends, then report the observed trends and only then start to discuss the potential drivers for these trends.

page 21, lines 21ff: "more prone". See my comment above. It would be good to be more specific what is meant here.

page 21, Conclusions. Before advancing to the conclusion section, it would strengthen the manuscript substantially if the authors expanded the discussion section by considering additional material (see my first major comment above). Examples, are coastal versus open ocean, comparison of fluxes across compartments, and recommendation for future studies, etc.

page 22: line 9-10: "Ultimately, the removal of anthropogenic carbon is the relevant quantity for assessing the contribution of ocean margins to the uptake of anthropogenic carbon". This is confusing - what is the difference between "removal" and "uptake"? Please explain better what is meant here. Is it global versus coastal?

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5 Cited References

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Nicolas Gruber, January 2019

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