

Interactive comment on “Arctic Ocean CO₂ uptake: an improved multi-year estimate of the air–sea CO₂ flux incorporating chlorophyll-a concentrations” by Sayaka Yasunaka et al.

Sayaka Yasunaka et al.

yasunaka@jamstec.go.jp

Received and published: 7 November 2017

Reviewer comment: The authors present an improved version of the Yasunaka et al. (2016) estimate of the uptake of CO₂ by the Arctic Ocean. The paper is very clearly written and the detail in quantifying uncertainty is impressive. The principal limitation of the manuscript is that it is very technical, and will likely need to touch more closely and directly on broader scientific questions to be recommended for publication in Biogeosciences. In my opinion, this could be accomplished with minor revisions, consisting of adding to the Conclusions with a few paragraphs and emphasizing the broader implications and relevance for identifying key processes and/or optimization observing system

C1

design.

Response: We appreciate the positive evaluation and helpful comments from the reviewer. Referring to the comments, we will carefully revise the manuscript. Both reviewers' comment is that the manuscript is technical and needs more scientific interpretation of the results. We will add descriptions in the revised manuscript about the improvement of our pCO₂ estimate (see response to the first minor point), the inter-annual variations in area-mean CO₂ flux (see response to the first main point), the effect of Re (see response to the second minor point), and the scientific implications of our results (see response to the first and second main points). We will also add the analyses and descriptions about data handling, the improvement of pCO₂ estimate by the Chl-a, the methodology of the assessment, and the robustness check of the result (please see response to the reviewer #1). Point-by-point responses to the reviewer's comments are given below.

Reviewer comment: Main Points: Although the authors have done a very good job of quantifying uncertainty in pCO₂ and air–sea CO₂ fluxes, the paper would benefit from commentary on the implications for optimization of the observing network. The other missing component of the study is a mechanistic interpretation of the main results, perhaps as part of the Conclusions. In the Introduction, there is a broad overview of the mechanisms that might impact trends in carbon fluxes over the Arctic, but it was surprising that these points did not get addressed in the Conclusions.

Response: There are still too few observations in the Kara Sea, the Laptev Sea, the East Siberian Sea and the Eurasian Basin to determine seasonal and interannual variations there. To improve our understanding of the variability in air–sea CO₂ fluxes in the Arctic, it is therefore of critical importance to obtain additional ocean CO₂ measurements to fill these data gaps, and that these measurements are made publically available. Data synthesis activities like SOCAT must be encouraged. We will add these points to the revised manuscript.

C2

Response: The reviewer makes a good point, and we add a figure and additional description of the interannual variations in area-mean CO₂ flux in the revised manuscript (Please see Figure E in the response to the reviewer #1). In the Greenland/Norwegian Sea, interannual variation of the CO₂ flux negatively correlates with the wind speed (CO₂ influx to the ocean is large when the wind is strong), while delta_pCO₂ and sea ice change are small. In the Barents Sea, interannual variation of the CO₂ flux negatively correlates with the sea ice concentration, while correlation with wind speed is not significant and delta_pCO₂ change is small. In the Chukchi Sea, CO₂ influx to the ocean is decreasing with the increasing delta_pCO₂; high pCO_{2w} (>500 μatm) has been observed in the Chukchi Sea after 2010 (Hauri et al. 2013). Interannual variability of CO₂ flux averaged over the entire Arctic Ocean is small because increasing delta_pCO₂ compensates for the sea ice retreat.

Reviewer comment: Given the availability of forward ocean biogeochemistry models that include the Arctic, I believe that the burden on the authors to provide at the very least an account of why they do not consider an Observing System Simulation Experiment (OSSE) to assess the skill of their method. Presumably some of the models that participate in the Global Carbon Project are open-access, and could be sampled with the spatial/temporal coordinates of the SOCAT and other pertinent data products? If the authors have scientific reasons for not finding the process-representation of the current generation of models to be up to the task, what then are the critical scales and processes that would be critical to represent?

Response: We agree that the assessment of our estimate using numerical models or their outputs would likely be useful. Conversely, assessment of the numerical models using our estimate is also an interesting topic since numerical models are poorly validated in the Arctic due to the limited observations - especially for biogeochemistry. However, such experiments need thorough insight into the numerical models, which is beyond the scope of this study. We hope to perform such comparisons in future studies. Instead, in the present study, we assessed the accuracy of our estimate by

C3

systematically excluding some of the observed pCO₂ data when labeling the neurons (section 4.2). We will add the descriptions in the revised manuscript.

Reviewer comment: Minor Points: It would be good if the authors could point out whether there are important methodological differences between their method and others in the literature that use neural-network-type approaches.

Response: This is a nice recommendation from the reviewer and we will add a description of the major differences in the revised manuscript. Instead of the normalization of pCO₂ to the reference year, we used atmospheric xCO₂ as a training parameter. To avoid intricately intermingled estimates in space and time in regions and seasons with limited CO₂ observations, we added geographical position to the set of training parameters: $X = \sin(\text{latitude}) \times \cos(\text{longitude})$ and $Y = \sin(\text{latitude}) \times \sin(\text{longitude})$. On the other hand, we did not use mixed layer depth because of lack of reliable data in the Arctic.

Reviewer comment: The Revelle factor was mentioned in the Introduction, and it would be very helpful to know if the authors believe that this will be an important factor over the Arctic when considering future climate change. It would be useful to discuss this as it pertains to the transient signal, and whether there is evidence that it is more important here than in other subpolar or circumpolar regions.

Response: Revelle factor (Re), $(\text{pCO}_2' / \text{pCO}_2) / (\text{DIC}' / \text{DIC})$, is a measure of the amount of CO₂ which a parcel of seawater can dissolve for a given increase in pCO₂. Summer time Arctic surface waters (70–80°N) have high Re of about 18, whereas the summer Irminger Sea surface waters (~64°N) in the northern North Atlantic have about 12 and a global ocean mean is about 10 (Takahashi, personal communication). The high Re is a result of the reduced salinity, which is maintained by the strong vertical density gradients below the low salinity surface layer reducing vertical mixing of high salinity deep waters. Higher Re values indicate that a given mass of seawater can absorb less CO₂ in response to increasing atmospheric CO₂. This means that the

C4

Arctic surface waters have small absorbing capacity for atmospheric CO₂ even though surface water pCO₂ is low. Re will also be of importance for the changing Arctic as it is a function of temperature and salinity, both which are believed to change in the future. In specific, warmer temperature will lead to a decrease in Re (increase buffering capacity) while lower salinity will have the opposite effect and cause an increase Re. Given the future Arctic Ocean is warming and freshening, the net effect is unclear but would be small. In our current study, we used climatological-mean salinity for the pCO₂ estimate because of no reliable year-to-year salinity data. That might be one of the improvement points for future study. Other complicating effects, such as the increasing input into the Arctic Ocean of river water (of higher alkalinity than river input into most other oceans), are also interesting topics, but not in the scope of the present study. We will add these points in the concluding section of the revised manuscript.

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2017-320>, 2017.