

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.

Anonymous Referee #1

Received and published: 20 September 2017

General comments: The authors of this manuscript try to estimate the surface ocean partial pressure of CO₂ (pCO₂) distribution in the Arctic Ocean using their technique of Self-Organizing Map (SOM) and evaluated the air-sea CO₂ flux. Basically, major theme of the manuscript is the improvement of the pCO₂ estimate published by the authors (2016, Polar Science) in the same region by adding chlorophyll a concentration (Chl-a). I am wondering why the authors didn't plan to adopt Chl-a concentration in the previous article since the Chl-a product had already existed before. Moreover, it seems to me that the scientific insights are not sufficient on the manuscript since the estimated annual net air-sea CO₂ exchange in this study is quite same with that of Yasunaka et al (2016) and it only reduced the uncertainty. At this stage, therefore, I have not any

[Printer-friendly version](#)

[Discussion paper](#)



confidence that the manuscript is suitable for publication in Biogeosciences. I suggest that more careful analyses and descriptions are needed at least before re-submission of the manuscript for review.

Major comments: 1) Although the authors mentioned that the addition of Chl-a as a parameter in the SOM process enabled them to improve the estimate of pCO₂ via better representation of its decline in spring (I think the authors mentioned about the lower panels of the figure 11), it seems that the pCO₂ variation estimated with Chl-a in the observed regions was similar to that without Chl-a especially from spring to fall (upper panels of the figure 11). I suggest the authors show further evidences that pCO₂ estimate with Chl-a improved the pCO₂ variation in spring better (for example, monthly RMSD variations with Chl-a/without Chl-a, etc).

2) I am not comfortable with the author's data handlings. First, the combination data of "non-public" JAMSTEC pCO₂ data with "public" SOCAT and LDEO data seems to be bit unfair since no one can't get the same results even if they use the public datasets. To guarantee the fairness, the authors should mention that the JAMSTEC data used in this study would be submitted to SOCAT and/or LDEO database soon. Second, I could not understand why the authors executed the data selection described in Lines 220-225 while the SOCAT and LDEO datasets had been already quality-controlled by researchers. I agree that the data selection may be needed for non-quality-controlled data such as nutrient recorded in the World Ocean Database, but I think it is unnecessary in pCO₂. I am seriously concerned that the data selection in this work (and in previous work) might affect the apparent uncertainty in the pCO₂ estimate and the evaluated RMSD was underestimated. Third, the authors used DIC data from the upper 30 m if there were no samples from above 10 m. I think the use of the data close to 30 m needs to be more careful treatment especially in summer, when the mixed layer depth is likely shallower than the sampling depth. I suggest that a comparison between observed pCO₂ or calculated pCO₂ from DIC samples shallower than 10 m and calculated pCO₂ deeper than 10 m would be needed for examining the availability.

3) I found both the manuscript and the article of Yasunaka et al. (2016) adopted atmospheric $x\text{CO}_2$ as one of the training parameters to reconstruct oceanic $p\text{CO}_2$ trend. Since Yasunaka et al. (2016) seemed to adopt $x\text{CO}_2$ to estimate $p\text{CO}_2$ in the SOM process for the first time, I also read the article. Consequently, I was bit disappointed there was not any descriptions of effectivity and validation by adopting $x\text{CO}_2$ and found only the sentence in the manuscript that “We believe that this (adopting $x\text{CO}_2$) better represents the real variability and trends of $p\text{CO}_2$ ”, which is not reasonable explanation. Moreover, based on my thoughts, SOM technique may be rather unsuitable to reconstruct $p\text{CO}_2$ trend while other techniques such as feed-forward neural network are suitable for it. The reason is that each neuron in the SOM has only one $p\text{CO}_2$ value. As the authors know, neurons are classified in accordance with the variations of respective parameters (X, Y, SST, Salinity, Chl-a, SIC, $x\text{CO}_2$ in this study) at the training process and most of them are labelled by the respective $p\text{CO}_2$ values at the labeling process. For example, when the temporal $p\text{CO}_2$ distribution is weighted toward later period like in this study, many of neurons tend to be labelled by the $p\text{CO}_2$ values which were observed in the later period. In that case, though the estimated spatial-mean temporal $p\text{CO}_2$ variations in the region where the observations had been made showed good agreement with measurements as shown in figures 4 and 5, it may be seen that the $p\text{CO}_2$ value observed in the later period is likely assigned to the grid at the former period where the $p\text{CO}_2$ measurements have not been made. To clear my doubts, I would suggest that the authors show the temporal variations for 18 years in the respective regions including the region where a few/no observations have not been made in the manuscript and discuss the trends.

4) I am wondering why the authors didn't examine the temporal variation of air-sea CO_2 exchange and its relevant factors in the whole of the Arctic Ocean. I think those might make the manuscript more suggestive one to understand whether the oceanic CO_2 uptake will increase or decrease in the region as global warming progresses, even if the estimated budget has large uncertainty.

[Printer-friendly version](#)[Discussion paper](#)

Specific comments: Lines 99-103: While SOCAT publishes the data as fugacity of CO₂ (fCO₂), LDEO opens the data as pCO₂. Did the authors treat the fCO₂ and pCO₂ data as they are (without any correction)?

Line 324: The description of figure 4c is presented after those of figures 5a and 5b. It would be better to fix this.

Line 382: The description of figure 7 is presented before those of figures 6c and 6d. It would be better to fix this.

Line 387: The description of figure 6d is presented before that of figures 6c. It should fix it.

Lines 483-484: Is there any plan to open the pCO₂ data in the website?

Minor comment: Line 256: Telzewski et al. should change to Telszewski et al.

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

Printer-friendly version

Discussion paper

