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Interactive comment on “Arctic Ocean CO_2 uptake: an improved multi-year estimate of the air–sea CO_2 flux incorporating chlorophyll-a concentrations” *by* Sayaka Yasunaka et al.

Anonymous Referee #1

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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

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 xCO2 as one of the training parameters to reconstruct oceanic pCO2 trend. Since Yasunaka et al. (2016) seemed to adopt xCO2 to estimate pCO2 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 xCO2 and found only the sentence in the manuscript that "We believe that this (adopting xCO2) better represents the real variability and trends of pCO2w.", which is not reasonable explanation. Moreover, based on my thoughts, SOM technique may be rather unsuitable to reconstruct pCO2 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 pCO2 value. As the authors know, neurons are classified in accordance with the variations of respective parameters (X, Y, SST, Salinity, Chl-a, SIC, xCO2 in this study) at the training process and most of them are labelled by the respective pCO2 values at the labeling process. For example, when the temporal pCO2 distribution is weighted toward later period like in this study, many of neurons tend to be labelled by the pCO2 values which were observed in the later period. In that case, though the estimated spatial-mean temporal pCO2 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 pCO2 value observed in the later period is likely assigned to the grid at the former period where the pCO2 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 CO2 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 CO2 uptake will increase or decrease in the region as global warming progresses, even if the estimated budget has large uncertainty.

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.

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