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.

Sayaka Yasunaka et al.

yasunaka@jamstec.go.jp

Response: We appreciate the many thoughtful comments from the reviewer on our manuscript. Addressing all of the comments, we will carefully revise the manuscript. To our knowledge, this is the first report to significantly quantify the Arctic CO2 sink, which is the major breakthrough that warrants publication in Biogeoscicences. Both reviewers' comment is that the manuscript is technical and needs more scientific interpretation of the results. We will add more comprehensive analyses and descriptions in the revised manuscript in terms of data handling (see response to comment #2), the improvement of pCO2 estimate (see response to comment #1), the methodology of

the estimate (see response to comment #3), the robustness check of the result (see response to comment #3), the interannual variations in area-mean CO2 flux (see response to comment #4), and others. We also add the implication of the effect of Re, and the suggestions from our results (please see response to the reviewer #1). We are now confident that the revised manuscript is much improved and hopefully acceptable for publication. Point-by-point responses to the reviewer's comments are given below.

Reviewer comment: 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.

Response: In our first paper we did not include Chlorophyll a concentration (Chl-a) as the available products (e.g. NASA's OceanColor dataset) have several shortcomings. For example, there were a lot of missing values and large uncertainties in the Arctic area because of cloud cover, low angle of sunlight, and sea ice. In addition standard Chl-a products are also prone to error due to high colored dissolved organic matter (CDOM) and total suspended matter (TSM) concentrations in the Arctic shelf seas (e.g., Matuoka et al., 2007; Lewis et al., 2016). In this submission we now deal with this by carefully reviewing the algorithms for extracting Chl-a from satellite remote sensing reflectance (Rrs) with the aid of an expert for the satellite color image (Eko Siswanto

as the second author). As a result, three algorithms were chosen for our analysis (Section 3.1). We also carefully interpolated Chl-a so as to fit with the original data (Section 3.2). Finally, we carried out a thorough examination of the uncertainty of the obtained Chl-a values (Section 4.1). We believe that Chl-a data used in this study has much less uncertainty than standard Chl-a data for the following reasons: 1) we used local Arctic Chl-a algorithms; and 2) pixels with invalid Chl-a data due to high CDOM and TSM were discarded (before interpolation); thus 3) interpolation conducted here was only based on valid Chl-a data. Reducing uncertainty of input data (e.g., Chl-a in this study) is a prerequisite when dealing with quantitative analysis related to biogeochemical processes. We will rephrase the abstract to emphasize these points.

Response: We consider the reduction of the uncertainty to be a substantial improvement over earlier estimates. To our knowledge, this is the first report to significantly quantify the Arctic CO_2 sink and that is the major breakthrough that warrants publication in Biogeoscicences. Reducing the uncertainty of this quantification is a key contribution to the larger work of constraining the global carbon budget (e.g., Le Quere et al., 2016). Because the Arctic is an important CO_2 sink, quantifying its fluxes and minimizing the uncertainty is of great scientific value. We will add these statements in the revised manuscript to clarify the added value of our study. We additionally note that the pCO_2 data set used in the present study was significantly enhanced compared to that used in previous study.

Response: We will add more comprehensive analyses and descriptions in the revised manuscript according to, and based on, the reviewers' comments in terms of data handling, the improvement of the pCO_2 estimate, the methodology of the assessment, the robustness check of the result, the interannual variations in area-mean CO_2 flux, and others (see below).

Reviewer comment: 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 pCO_2 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).

Response: We present the difference in bias and RMSD for pCO2 estimated with and without Chl-a; Figure A shows the time-evolution and Figure B shows the spatial distribution. pCO2 estimates in winter tend to be low and those in summer tend to be high, and the systematic biases and RMSDs are suppressed in the estimates with Chl-a in both seasons (Figure A). Biases and RMSDs are reduced in the Canada basin, the western Bering Sea, and the boundary region between the Norwegian Sea and the subpolar North Atlantic (Figure B). It means the strong east–west contrast in the Bering Sea and the contrast between the Canada Basin and the Chukchi Sea (see Figure 4) were better represented in the estimates with Chl-a. That is, including Chl-a when estimating pCO2 yields not only better representation of the pCO2 decline in spring to summer season but also better representation of the contrast in time and in space. We will add these results to the revised manuscript.

Reviewer comment: 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 JAM-STEC 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.

Response: JAMSTEC pCO2 data are publicly available through the JAMSTEC web site (http://www.godac.jamstec.go.jp/darwin/e). We have additionally encouraged the PIs of these data to submit them to the SOCAT and LDEO database.

Response: As the reviewer mentioned, pCO2 data in the SOCAT and LDEO database have been quality-controlled. It must include realistic values, but sometimes quite extreme values reflecting the small and/or short time scale variations which can be very different from the large-scale variabilities. Therefore, to limit the possibility of biasing the results with small scale and short-term variability, we decided to conduct the additional quality control to exclude outliers not representative of the basin-wide distribution. We will rephrase this description in the revised manuscript.

Response: We checked the difference between observed pCO2 and calculated pCO2 using the data at cruises both underway pCO2 and bottle DIC/TA samples are available (245 pairs). Observed and calculated pCO2 values above 10m are 289 ± 11 μatm and 299 ± 41 μatm. The mean values are slightly lower for observed pCO2 values than calculated ones, but the difference are smaller than the standard deviation and the uncertainties of the calculation (14 μatm; Lueker et al., 2000). The difference are not depended on the depth where the TA/DIC samples were obtained; observed pCO2 values generally at 4–6m and calculated pCO2 values above 10m are 288 ± 11 μatm and 298 ± 43 μatm, and those in the 10–30m range are 294 ± 9 μatm and $302 \pm 36\mu$atm. We will add these results to the revised manuscript.

Reviewer comment: 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.

Response: According to the inter-comparison study by Rodenbeck et al. (2015), the pCO2 trends estimated by feed-forward networks and by SOM agreed quite well. To represent the anthropogenic pCO2 increase, several previous studies have assumed a homogeneous increase in the whole analysis area (Nakaoka et al. 2013; Zheng et al. 2014). They subtracted the trend before the estimate and recombined it after the estimate. But the homogeneous increase has not been assured. Actually, the spatial

pattern of the pCO2 trend (see Figure Ca) is far from homogeneous. Instead of that, we included atmospheric xCO2 as a training parameter following Landschutzer et al. (2013, 2014). We will add text about these points in the revised manuscript.

Response: As the reviewer mentioned, the pCO2 values observed in the latter period may be used for the pCO2 estimate in the former period. To validate our estimated pCO2 values for periods and regions without any observed data, we repeated the mapping experiments after systematically excluding some of the observed pCO2 data when labeling the neurons; four experiments were carried out, by excluding data (1) for 1997–2004, (2) for January to April, (3) from north of 80°N, and (4) from the Laptev Sea (90°E − 150°E), where there are only a few pCO2 observations. We compared the pCO2 estimates obtained in each experiment with the excluded observations and found that the pCO2 estimates reproduced the general features of the observed spatial distribution and temporal variation, and they were also similar to the pCO2 estimates obtained by using all observations, although RMSDs between the estimates and the excluded observations are 1.8 times the RMSDs of the estimates based on all observations as mentioned in Section 4.2. It means that our estimated pCO2 would reproduce the general features both in space and time even when and where there are any observed data. We will rephrase the relevant text in the revised manuscript.

Response: According to the reviewer's suggestion, we examined the correspondence between pCO2 trend and the year when the first observation was made in each grid point (Figure C). Spatial distribution of pCO2 trend did not correspond to the year when the first observation was made. We also checked the pCO2 interannual variation and the year of observed in the several regions. Here we show them in 73–77°N 175°E −175°W, 73–77°N 160–150°W, and 73–77°N 135–125°W where the trend and the observed year are different separating only several degrees in longitude (Figure D). The pCO2 trends increase from west to east, while the observational data are from 2004 in the west, from 1999 in the middle, and from 2011 in the east. It shows again the amplitudes of trend do not correspond the year when the first observation was

conducted. We will add these descriptions in the revised manuscript.

Reviewer comment: 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.

Response: We agree the reviewer's comment, and will add a figure of the CO2 flux in several regions of the Arctic Ocean and some text explaining that in the revised manuscript. Figure E shows interannual variation of CO2 flux and related variables in several regions. In the Greenland/Norwegian Sea, interannual variation of CO2 flux negatively correlates with the wind speed (CO2 influx to the ocean is large when the wind is strong; correlation coefficient R = −0.41), while delta_pCO2 and sea ice change is small. In the Barents Sea, interannual variation of CO2 flux negatively correlates with the sea ice concentration (R = −0.50), while correlation with wind speed is not significant and delta_pCO2 change is small. In the Chukchi Sea, CO2 influx to ocean is decreasing with the increasing delta_pCO2 (R = 0.87); high pCO2w (>500 μatm) has been sometimes observed in the Chukchi Sea after 2010 (Hauri et al. 2013). The interannual variability of the CO2 flux averaged over the Arctic Ocean is small because the increasing delta_pCO2 compensates for the effect of sea ice retreat (R = −0.70).

Reviewer comment: 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)?

Response: We converted from SOCAT fCO2 values to pCO2 values, and then combined these with the LDEO pCO2 values. This will be clarified in the revised manuscript.

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

Response: We agree. We will combine Figures 4 and 5, and change the order.

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

Response: We agree. We will extract Figures 6c and 6d into a separate figure, and put them after Figure 7.

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

Response: The reviewer is right. We will change the order.

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

Response: We plan to make public the pCO2 data and the interpolated Chl-a data by this study on the same website. We will add the information to the revised manuscript.

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

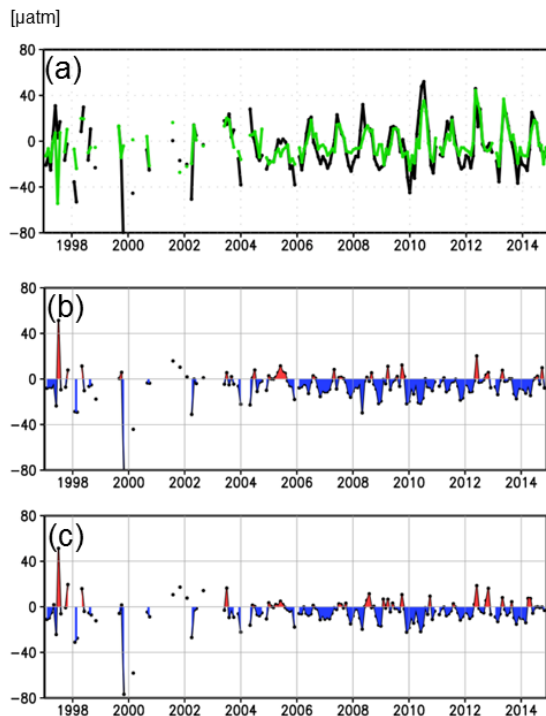
Response: The reviewer is right. We will correct this.

[µatm]

Figure A: (a) pCO2 bias (estimate – observation) averaged over the entire analysis area [µatm], for the estimates with Chl-a (green) and without Chl-a (black). Difference in (b) absolute bias and (c) RMSD for pCO2 estimated with and without Chl-a averaged over the entire analysis area [µatm]. Negative value means improvement of the estimates in b and c.

Fig. 1. Figure A

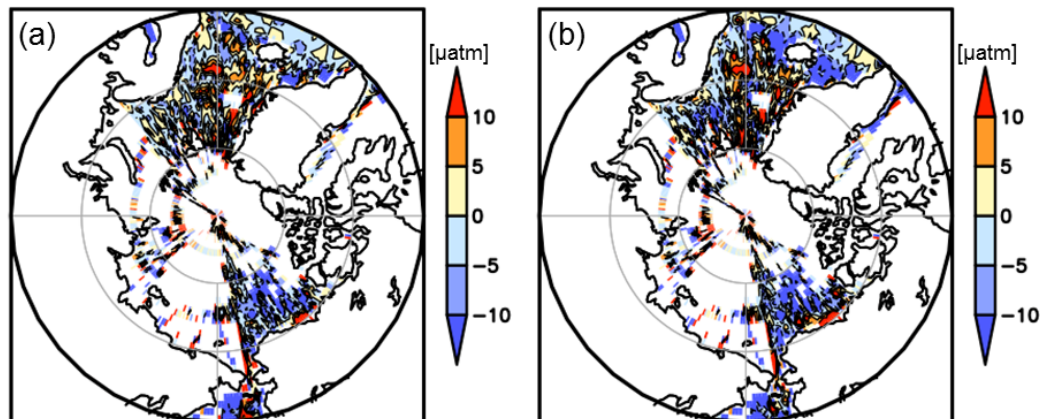



Figure B: Difference in (a) absolute bias and (b) RMSD for pCO2
estimated with and without Chl-a averaged over the whole analysis
period [µatm]. Negative value means improvement of the estimates.

Fig. 2. Figure B

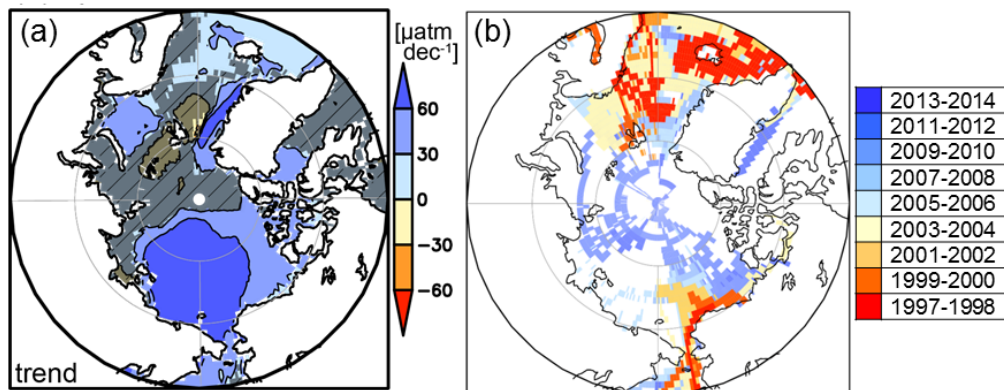



Figure C: (a) pCO2 trend. Darker hatched areas represent values in grids where trend values were less than the uncertainty. (b) Year when the first observation was retained.

Fig. 3. Figure C

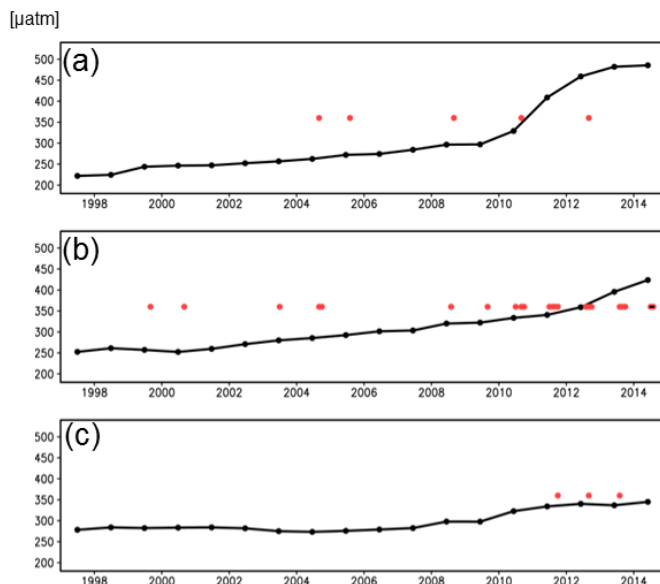



Figure D: The pCO2 interannual variation and the year of observed in (a) 73–77° N 175° E –175° W, (b) 73–77° N 160–150° W, and (c) 73–77° N 135–125° W.

Fig. 4. Figure D

CO2 flux (mmol/m2/d) delta_pCO2 (µatm) wind speed (m/s) sea ice (%)

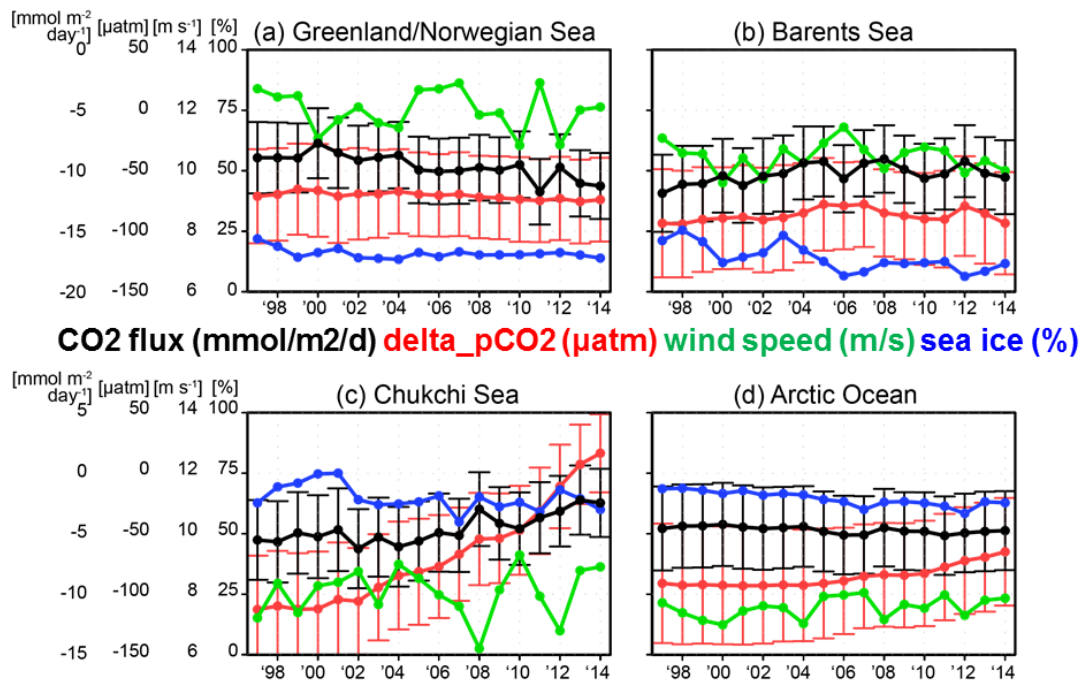



Figure E: Time evolution of the air-sea CO2 flux and its driving factors in (a) the Greenland/Norwegian Seas, (b) the Barents Sea, (c) the Chukchi Sea and (d) the Arctic Ocean.

Fig. 5. Figure E