Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2017-148-RC2, 2017

Interactive comment on "Low $p\mathrm{CO}_2$ under sea-ice melt in the Canada Basin of the western Arctic Ocean" *by* Naohiro Kosugi et al.

Anonymous Referee #2

Received and published: 3 July 2017

General comments:

The Arctic Ocean is a rapidly changing system that has a highly dynamic CO2 system both seasonally and with the changing physical conditions and climate change scenarios. The authors presented CO2 system data during an autumn of 2013 cruise which will enrich the available CO2 data in the Arctic Ocean and benefit the scientific community (however the dataset is still available via the link). Their results largely support other recent observations that pCO2 is low in ocean margin but high (approaching to the atmospheric pCO2) in the Canada Basin. The explanations they provide are also consistent with other recent publications. Most interesting, the authors observed a subsurface minimal pCO2 structure in the Canada basin. They demonstrated this feature in fig. 5 and discussed the causes for low pCO2 water by analysis the water types, TA-

S and pCO2-AOU relationships. Finally, they discussed the possible fate of this "hidden CO2 sink" and its influence in the future Arctic Ocean (they basically rejected this possibility, which I also agree). I agree with most of their views. Their finding is worthy to be published. However, the main conclusion in this paper is undermined because of not enough data, i.e. low pCO2 in the subsurface of Canada Basin. It is also not clear to me whether pCO2 minimum at 30-50m is due to in situ biological production as they have suggested or subduction of surface water from the highly productive shelf. I think more complete depth profiles from the Niskin bottle samples down to 150-200 m rather than 50m alone from a CTD pumping system will help to elucidate this issue.

Another issue I have with this manuscript is writing. In general they have done a good job in writing except the text around Fig. 5. I don't think the authors put enough thinking into organizing the paper the best they can. One indication is they presented air-sea CO2 flux calculation method in the methods section but never presented air-sea CO2. Did they initially prepared a longer paper and then deleted the flux part? Another indication is in Fig. 5. While it is nice to see the pCO2 minimum with a high frequency depth profile, the depths of such profiles are limited to 50m. However the Pacific winter water and Pacific summer water are all deeper than 50m (if not in this region, they should say it). Thus the entire discussion is not clear. Most important, it is not clear to me whether pCO2 minimum at 30-50m is due to in situ biological production or subduction of surface water from the highly productive shelf. Again, I feel using the depth profile from the Niskin bottle based profiles will help to elucidate this issue. Such depth profiles will also present nutrient profiles to support the argument on nutrient availability (rather than just citing melting pond information).

In summary I'd support the eventual publication of this paper but not at this stage. More data are needed to support their arguments. Specific comments Page 1 Line 23 it is unnecessary to add "e.g." before the citation. Page 2 Line 14 changing "reduces" to "limits" Page3 The pCO2 data set presented in this paper is still not available to readers though the link: (http://www.godac.jamstec.go.jp/darwin/cruise/mirai/mr13-06_leg1/e).

Equation 1 is totally unnecessary. Just cite Takahashi would be enough.

If the authors didn't do any calculation of CO2 flux in this paper, it is totally unnecessary to have the description of air-sea CO2 flux calculation (page 3, line 17- line 25). Line 22, equation (4). I don't recall W92 has a non-zero term. Please check if you cited a more recent Wanninkholf paper and equation. Line 24-25 The wind speed at 24 m height is measured by an anemometer and is extrapolated to 10m. Using an instantaneous wind speed is probably not the best choice for CO2 flux calculation with underway data. The average wind speed from satellite data may make more sense due to equilibrium time for CO2 is pretty long. For example, at 1 pm, if a vessel is at point A where pCO2 is 350 uatm and wind speed is 4 m/s. When the ship arrives at point B at 11 pm the same day where pCO2 is 350 uatm and wind speed increases to 7 m/s. It doesn't make any sense to believe that CO2 uptake flux is much greater at point B than A. If you will use satellite wind, then the fluxes in these two locations are likely the same (that is winds are same for A and B but only changes over a day). However, I must say since calculating flux is not the goal of this paper, this is not a serious problem. Then, of course, there would be no need for the authors to even present the flux calculation equation. Page 4 Line 17 What software or package was used for calculation of carbonate chemistry? Page 5 Line 7 TARRO should be TARRO Line 22-24 The description of " (1) Barrow Coastal Water (BCW) was relatively warm and fresh (SST > 2 °C, SSS < 30.5). (2) Canada Basin Water (CBW) was cold and fresh (SST < 2 °C, SSS < 28). (3) Chukchi Sea Water (CSW) was saline (SSS > 28)" is a little confusing. BCW was fresh SSS<30.5 while CSW was saline (SSS>28). What is the reference for fresh and saline? As I see it (Fig. 3), most of BCW had SSS<28.5 except the very nearshore part while most CSW had SSS>30.5. Only minor clarification is needed here. Page 6 Line 3 removing "the resulting" as low DIC/TA and low pCO2 are the same thing or same result of physical and biogeochemical processes (biological uptake here, but in the basin CO2 evasion from the atmosphere plus strong stratification and low PP in surface water). There is no magic low DIC/TA that leads to low pCO2. Line 9 regarding "half-life of CO2 gas exchange", while I can guess how did you estimate this, it is better to tell

readers. Line 17 Font is different from other context Nutrient in melting pond cannot be a sufficient evidence for limitation of nitrate in surface water. Report directly the nutrient data in water would be better. Line 24 change "pCO2sea" to "pCO2sea " line 29-30 "Reduction in CO2 absorption capacity by riverine discharge was not as large as that by sea-ice melt." This conclusion is not solid. Need more explicit verification. Page 7 Line 4 changing "with depth" to "as depth increases" Line 17-18 "In CSW, the halocline, although not as clear as in the other two subregions, was at almost the same depth." But thermocline is very obvious in CSW (Figure 5a). Line 21 Should "In contrast" be "Likewise"? not clear what is the undertone by this. Page 7 line 19, what is "column variation"? must be water column? Same in lines 26, 29 and 31, all change to "water" column profiles. p.7 Line 26- Page 8 Line 12 The biggest problem here is pCO2 data in CBW is too limited. (only three water column data shown in Figure 5e). Considering the mixing layer structure is complicated in this subregion, it is difficult to see the real pattern. With only 3 station, how to distinguish the real reasons for low pCO2 in subsurface CBW, either due to the local net primary production in CBW or just the water with low pCO2 subducted and advected into the Canada Basin? If the authors could plot the entire water column data (deeper than 50 m in Figure 5), that would provide more information and be helpful to interpret their finding. Also, the Discussion of various waters does not related to Fig. 5 very well, thus causing confusion in reading as the deepest depth is only 50m while the winter water (rWML) is about 120m and summer water (PSW) is even deeper. I am somewhat confused in reading lines 5-13 in p. 8. Since this part is the new point that the authors want to present. It absolutely should be explained very clearly. p.8, line 30, replace "think" with "believe" or "suggest". Table 1 It is not clear whether the average of all the samples were within mixing layer or including the entire water columns. It is probably better to separate the data into mixing layer and below mixing layer for discussion. And please add standard deviation.

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2017-148, 2017.