

Point-by-point responses to Review #1, #2 and #3.

Biogeosciences Discuss., 11, C64–C67, 2014, Nomura et al.

"Winter to summer evolution of pCO₂ in surface water and air–sea CO₂ flux in the seasonal ice zone of the Southern Ocean"

For clarity, the authors' responses are in green.

Response to Reviewer # 1

The paper presents and discusses measurements of CO₂ and accompanying variables (T, S, Chl) for one week in January 2006 in the southern ocean seasonal ice zone. It estimates the winter to summer evolution via estimations of the various contributing components and the assumption that conditions in the temperature minimum layer (TML) represent conditions of the previous winter. While the paper does not provide any particularly exciting and groundbreaking new science aspects it adds to the sparsely available pCO₂ data in ice covered waters. The analysis is for the most part straight forward and clear although some assumptions could be evaluated a bit further and the addition of uncertainty ranges would be valuable. The paper is also lacking a conclusion section, which I however think the authors can easily provide. Generally, I think, the authors made the most out of the limited data they had available. I hence recommend publication after some minor/major changes/additions. (Please note that I clicked the button for accepted subject to minor revisions, but it should be somewhere between minor and major)

We are grateful for your favorable assessment. We agree with your comments. In response to your comments, we have added information on the uncertainty ranges for each instrument as well as a Conclusion section. In addition, we have removed repetitious text and improved the clarity of the text.

Abstract p658 line 11-14 rm “with” behind flux and rephrase sentence for clarity: line 15 add “temporary” before CO₂ source: line 18 add is a CO₂ sink => is again a CO₂ sink

We agree with your comment. Corrected accordingly.

Text p 658 line 20 the site => an area ...CO₂ fluxes

We agree with your comment. Corrected accordingly.

p661 Are there error margins available for the pCO₂ measurements?

We have added information for pCO₂ measurements “The precision of pCO₂ measurements was less than 2 μatm, as estimated on the basis of the uncertainty of the rise in seawater temperature between the surface and the equilibrator (±0.1°C) and the precision of the NDIR analyzer (1 μatm).” to the text.

P 662 line 24 Maybe reformulate: sea ice was at it's minimum during our observation period => sea ice just reached it's minimum before disappearing in the summer (otherwise it sounds like the sea ice min is in January).

We agree with your comment. Corrected accordingly.

P 662 line 25 Sea ice concentration data...this trend=> This part of the sentence can probably be deleted and Fig 4 just be referenced at the end of the previous sentence.

We agree with your comment. Corrected accordingly.

P663 Line 4 occurred later here => give time

We agree with your comment. We have added time “(end of February)” and “(middle of January)”.

P663 is uncertainty measure for Seawifs Chl available?

Cannizzaro et al. (2013) compared chlorophyll a data from in situ with SeaWiFS. Results indicated that they were highly correlated ($r^2 = 0.84$, $n = 289$) although SeaWiFS values were positively biased with a ratio of 1.25.

We have added “Cannizzaro et al. (2013) compared chlorophyll a data from in situ and SeaWiFS measurements and found that they were highly correlated ($r^2 = 0.84$, $n = 289$) although SeaWiFS values were positively biased by 25%.” to the text.

We have added this paper in the reference list “Cannizzaro, J. P., Hu, C., Carder, K. L., Kelble, C. R., Melo, N., Johns, E. M., Vargo, G. A., & Heil, C. A.: On the accuracy of SeaWiFS ocean color data products on the West Florida Shelf. J. Coast. Res. 29, 6, 1257–1272, doi.10.2112/JCOASTRES-D-12-00223.1., 2013.”, accordingly.

P664 line 17-20 repetition, just refer to intro

We agree with your comment. We have deleted these sentences from the “introduction”. Please see Reviewer #2 comments “P659, L14-18: it would have been interesting to provide aThe authors provide a full list of references without giving any information to the reader to guide him in his reading.”.

line 24 ice covered water gave way => retreating sea ice gave way (The water is probably the same :))

We agree with your comment. Corrected accordingly.

p665 line14-18 again repetition, refer to intro

We agree with your comment. We have removed this part from text. In addition, please see Reviewer #2 comments “P665 L14-23: all this section is already in the introduction, and more work should be done on it (see previous comments). Instead of repeating the same information, it would have been interesting to discuss how the results fit with what we can find in the literature.”

p666 line 16 the authors provide a maximum ikaite concentration which they use for their calculations, shouldn't rather a mean concentration be used? Or at least an error margin provided?

To examine the upper limit of ikaite dissolution for carbonate systems in surface seawater, we used maximum ikaite concentrations in sea ice. Taking your comment into account, we have revised our estimate of the effect of ikaite dissolution on surface carbonate systems based on the recent literature (Rysgaard et al., 2013), as suggested by reviewer #2 (Please see the comment “P666 L16: why using the estimation from Dieckmann et al (2008)? Recent work from Rysgaard et al (2012, 2013)..... and not in the “seawater pool”).

We have added sentences to the text “The amount of ikaite in Antarctic sea ice has been estimated to be 100–900 $\mu\text{mol kg}^{-1}$ ice (Rysgaard et al., 2013). Under the assumption that all ikaite in sea ice is dissolved in sea ice, and that meltwater is supplied to the upper 20 m of the winter water column, the meltwater input should alter n-DIC from 2206.6 $\mu\text{mol kg}^{-1}$ to 2211.6–2251.6 $\mu\text{mol kg}^{-1}$ and n-TA from 2327.6 $\mu\text{mol kg}^{-1}$ to 2337.6–2417.6 $\mu\text{mol kg}^{-1}$ during the transition from winter to summer at station L8.”.

All equations are a bit convoluted due to the lengthy subscripts, I think they could be significantly simplified. Some suggestions are: $\text{PCO}_2 \text{ air} \Rightarrow \text{PCO}_2\text{a}$, $\text{PCO}_2\text{water} \Rightarrow \text{PCO}_2\text{w}$
 $\text{pCO}_2 \text{ water(winter to summer)} \Rightarrow \text{PCO}_2\text{w_w2s}$ or win2sum also remove all periods when using abbreviations in variables: $\text{pCO}_2\text{cal.} \Rightarrow \text{pCO}_2\text{cal}$

We agree with your comment. Corrected accordingly through the text.

Equation 7 and 11 contain S used for sea ice concentration. S is frequently used for Salinity, which is in fact done in this same paper in equation 10. I suggest picking a different variable. E.g. A is frequently used for sea ice concentration/ice area coverage.

We agree with your comment. We have changed from “S” to “A”.

P669 line 6-8 could an actual value be calculated based on the earlier estimate from delta DIC (p666 line16-20).

Based on our revisions from 2206.6 $\mu\text{mol kg}^{-1}$ to 2211.6–2251.6 $\mu\text{mol kg}^{-1}$ for n-DIC and from 2327.6 $\mu\text{mol kg}^{-1}$ to 2337.6–2417.6 $\mu\text{mol kg}^{-1}$ for n-TA in our ikaite dissolution calculation (please see you comments: p666 line 16 the authors provide error margin

provided?), $p\text{CO}_2$ decreased from 380.7 μatm to 300.3–370.0 μatm at constant salinity (34.25), temperature (0°C), and pressure (1 atm) using the program CO2SYS, version 01.05 (Lewis and Wallace, 1998).

We have added “These n-DIC and n-TA variations correspond to a $p\text{CO}_2$ decrease from 380.7 μatm to 300.3–370.0 μatm at constant salinity (34.25), temperature (0°C), and barometric pressure (1 atm), as calculated using the program CO2SYS.” to the text.

Line 24/25 this statement is a bit unclear. Although I understand the reasoning, the fact that $p\text{CO}_2$ obs changes and the difference reflects this biological effect, it seems odd to state that the difference only represents the thermodynamic effect. => reformulate, Maybe on that occasion also point out the similarity between Fig 10 and Fig 2b.

We agree with your comment. We have changed the text “. Thus, $p\text{CO}_2$ cal indicates only the thermodynamic effect on surface-water $p\text{CO}_2$. Therefore, the difference between $p\text{CO}_2$ cal and $p\text{CO}_2$ obs reflects other effects (e.g. biological effects) on surface-water $p\text{CO}_2$ from winter to summer.”.

We also added “The similarities of the longitudinal distributions of $p\text{CO}_2$ and chlorophyll a concentrations are consistent with these relationships (Figs. 2b and 10).” to the text.

Equation 11 is a bit confusing due and again the use of “Air-ice-sea FCO2” as a variable name adds to the confusion. I would suggest to just mention that a term FCO2i is added to equation 7 and limit eq 11 to $\text{FCO2i} = F_{\text{ice}} S/100$ (note the minus sign is also confusing make sure sign convention is consistent , generally I would expect fluxes to be additive. If they then turn out to be negative due to a specific process, it maybe so, but the equation should state the addition.

We agree with your comment. We have changed Eq. (11) according to your comment.

If I understand correctly the “flooding” case assumes ALL ice is IMMEDIATELY flooded. This strikes me as unrealistic and needs further explanation. I can see the flooding case as an extreme state, with true solutions between the flooding case and no exchange- if-ice case, however it would need to be discussed as such.

We agree with your comment. We have added the explanation for surface flooding “The formation of the surface flooded (slush or gap) layer likely occurs frequently in melting Antarctic sea ice (Haas et al., 2001; Kattner et al., 2004; Ackley et al., 2008; Zemmeling et al., 2008; Papadimitriou et al., 2009; Nomura et al., 2012, 2013b). Snow accumulation over sea ice and the formation of superimposed ice leads to the formation of a slush layer below sea level (Haas et al., 2001).”.

Conclusion section is missing and in consequence it is not really clear what are the main results and what is the take away message, please add.

We agree with your comment. We have inserted a Conclusion section by taking into account the final sentences in the discussion as follows: “The results of this study shed light on CO₂ dynamics and flux during the winter-to-summer transition in the SIZ. For the surface-water pCO₂, we demonstrated that pCO₂ variations have a thermodynamic origin before the onset of active biological productivity. With regard to the CO₂ flux, it is not yet certain whether the SIZ acts as a CO₂ sink or source relative to the atmosphere throughout this season. This study evaluated the air–sea CO₂ flux (including the air–ice CO₂ flux) as negative in winter, indicating a CO₂ sink (Fig. 9a). Although sea ice blocks the direct exchange of CO₂ between the ocean and the atmosphere, pCO₂ in the water under the ice (349.9 μatm) is low with respect to the atmosphere (Fig. 9a), suggesting that in addition to sea ice, water in the SIZ is a potential CO₂ sink in winter (Nomura et al., 2013a). Ishii et al. (2002), however, reported that pCO₂ in surface water (390 μatm) is high in winter with respect to the atmosphere near our study area and that CO₂ could enter the atmosphere (indicating a CO₂ source) through ice-free regions such as polynyas and leads. Further studies are needed to address these conflicting findings.”.

Fig 3: I don't think all the satellite pictures provided are needed, at least a and b can be removed, since they do not add value. The authors could rather consider adding a figure representing the annual minimum ice cover.

We agree with your comment. We have removed (a) 27 November–3 December 2005 and (b) 4–10 December 2005 from Figure 3, and we have added new figure (g) 12–18 March representing the annual minimum ice cover.

Response to Reviewer # 2

General comments:

The authors measured the partial pressure of CO₂ (pCO₂), salinity, temperature and Chl a concentration in surface water of the Indian sector of the Southern Ocean. Associated with these parameters, several profiles of temperature, salinity and carbonate system parameters (total alkalinity and total dissolved inorganic carbon) were collected. The goal of this study is to understand the CO₂ dynamics in the surface seawater during the seasonal transition between the winter and the spring. The main and only result suggested by the authors is that the surface pCO₂ increases from winter to spring, mainly due to the surface seawater temperature increasing. This conclusion has already been presented by several publications, references used in this manuscript (e.g. Ishii et al 2002, Metzl et al 2006, Chierici et al 2004, ...). Therefore this study doesn't bring anything new. This study is only based on a 8 day survey and an extrapolation on a hypothetical "winter situation". In addition, the authors suggest that the SIZ could act as a source of CO₂ while everything in their study suggests the opposite. Despite a really interesting development on how to extrapolate the "winter pCO₂" from the spring situation, I have the feeling that the authors try to combine a paper as fast as possible and make a lot of shortcuts in presenting their ideas.

We are grateful for your critical yet valuable comments. In this study, we observed pCO₂ variation over the period from sea ice melting to before the onset of active biological productivity. During this period, we demonstrated that pCO₂ variations are consistent with a thermodynamic effect. We have responded to your comments and changed the text, particularly for atmospheric pCO₂, distribution of chlorophyll a concentration, relationships between pCO₂ and flux, the depth of the TML and mixed layer as shown below.

Specific comments:

P658, L8-9: 'the temperature minimum layer' is unclear and should be defined in the abstract to help the reader to understand what the authors mean.

We agree with your comment. We have added " $-1.9^{\circ}\text{C} < T < -1.5^{\circ}\text{C}$, $34.2 < S < 34.5$ " to the abstract.

P658, L16: 'CO₂ source in summer immediately after sea ice melt'. I will be careful with this sentence. It really needs to be discussed in the manuscript, as immediately after the sea ice melt, the decrease of the salinity in the surface ocean will promote a decrease of the pCO₂ and therefore an uptake of CO₂.

We agree with your comment that immediately after ice melt, the decrease in salinity of surface water promotes a decrease in pCO₂, but not so much because of the compensation effect of DIC and TA changes.

Our results suggest that the seasonal variation of surface-water pCO₂ was mainly caused by the variation of water temperature even immediately after ice melt (Figure 9a and Table 2). With the SST rise, surface-water pCO₂ increased (Figure 9a). This temperature effect was

greater than the effect of meltwater input in decreasing surface-water pCO₂. Therefore, immediately after ice melt, surface-water pCO₂ was supersaturated with respect to atmospheric levels (Figure 9a), indicating a positive CO₂ flux from water to air (CO₂ source) (Figure 9b).

In addition, according to the comments from Reviewer #1 (Abstract p658 line 11-14 rm “with” behind flux and rephrase sentence for clarity: line 15 add “temporary” before CO₂ source: line 18 add is a CO₂ sink => is again a CO₂ sink), we have added “temporary” before the CO₂ source.

P659, L11: by definition, the SIZ is made by first year sea ice... the sentence is unclear; it seems they define SIZ twice.

We agree with your comment. To avoid the misunderstanding, we have removed “In the Southern Ocean, the SIZ covers a large area due to the large contribution of first-year sea ice to the total sea ice area (Comiso, 2003).” from the text.

We have added “in the Southern Ocean” after “The distribution of surface-water pCO₂ in the SIZ”.

P659, L14-18: it would have been interesting to provide a range of concentration reported in the literature, in summer and winter periods, as well as the location of these studies. The authors provide a full list of references without giving any information to the reader to guide him in his reading.

McNeil et al. (2007) (Global Biogeochemical Cycles, 21, GB3011, doi:10.1029/2007GB002991) demonstrated the seasonal difference between winter and summer pCO₂ in the Southern Ocean. In their Figure 8a, seasonal differences between winter and summer pCO₂, both positive and negative, were distributed inhomogeneously. These results suggest that it is difficult to characterize the seasonal and areal pCO₂ distribution in the seasonal ice zone. Therefore, our study sought to accumulate pCO₂ data and clarify the factors controlling the temporal and spatial variations of pCO₂ in the SIZ.

We have added “McNeil et al. (2007) demonstrated the seasonal difference of surface-water pCO₂ between winter and summer in the Southern Ocean, and both positive and negative, were distributed inhomogeneously. These results suggest that it is difficult to characterize the seasonal and areal surface-water pCO₂ distribution in SIZ.” to the text.

P659, L19-27: the authors are debating about the high or low pCO₂ in the surface water in winter without giving any information on summer conditions, while their data set has been collected in austral summer. We are not sure if the writers suggest that the high pCO₂ in surface water is due to a mixing of high pCO₂ deep water with the surface water or is due to the association of 2 processes: high pCO₂ deep water and community respiration. However, in both cases, in winter, the sea ice formation and the associated release of brine to the underlying seawater is the main driver of the deep-water formation (with high pCO₂, as pointed out in line 22). So this water can't be found in surface. Moreover, it is generally

admitted that the high surface pCO₂ is due to the brine (with high pCO₂) rejection, as suggested in line 26, so why discussing the other process before? Few studies reported CO₂ fluxes over sea ice in winter time (e.g. Miller et al., 2011, Geilfus et al., 2013, Nomura et al., 2006)

We agree with your comment. We have changed the text according to your comments “In winter, sea ice formation and the associated release of brine to the underlying seawater is the main driver of deep water formation, and this process contributes to the formation of high-pCO₂ water (Nomura et al., 2006; Rysgaard et al., 2007). Respiration creates high-pCO₂ water during the season of low sunlight. In addition, sea ice may inhibit the release of CO₂ from the sea surface to the atmosphere, leading to the accumulation of CO₂ under the sea ice, although recent studies have proposed that CO₂ is released directly from sea ice (Nomura et al., 2006; Miller et al., 2011; Geilfus et al., 2013).”.

P660, L2: it is a complicated topic, but it's doesn't help to say it, the author could take the opportunity of this manuscript, with all the references provided in it, to give a short abstract of what can be found in the literature and organize it. And not make it even more complicated.

Please see your comment “P659, L14-18: it would have been interesting to provide a range of concentration reported in the literature,information to the reader to guide him in his reading.”.

P661, L1: the authors need to be more precise about how the CO₂ concentration in the atmosphere was measured. I don't think the measurements were done the same way as for the seawater.

We agree with your comment. We have changed the text to the following: “For measurements of atmospheric CO₂ concentration, air samples were pumped through a Teflon tube from the ship's mast and introduced to the CO₂ analyzer every 6 min.”.

P661, L3-5: what is the precision and the accuracy? You provide these informations for the CO₂, why not giving them here as well? Especially as the calculations made later in the manuscript are highly linked to these values.

We have added information “Accuracy of temperature and salinity is $\pm 0.005^{\circ}\text{C}$ and ± 0.001 , respectively.”.

P661, L5-10: precision and accuracy?

We agree with your comment. However, we cannot find information about the precision and accuracy of the fluorescence probe (WETStar, WETLabs Inc.), only information about its sensitivity of $0.03 \mu\text{g L}^{-1}$ (<http://www.wetlabs.com/content/wetstar>). Nor could we find information about the precision and accuracy of the fluorometer (Model 10AU, Turner Designs, Inc.), only its detection limit of $0.025 \mu\text{g L}^{-1}$

<http://www.turnerdesigns.com/products/laboratory-fluorometer/10au-laboratory-fluorometer>).
We have added this information to the text.

P661, L15: could the authors provide the sampling depth used with the Niskin bottles? We only know that they measured the pCO₂ at 10 m depth, which is not the surface water.

We have added the depth information to the text “0, 10, 20, 30, 50, 75, 100, 150, 200, and 500 m”.

P661, L24: the chl pigments were not extracted in a freezer. The filters were stored in the freezer until analysis.

We agree with your comment. Corrected accordingly.

P662, L5: the data set presented here come from the austral summer. Sea ice does not start to form in January in Antarctica.

We agree with your comment. We have removed “concentration” and “formation” from the text. In addition, we have added “during the winter-to-summer transition” to the text.

P662 L6-7: this ‘temperature minimum layer’ seems to be a key parameter of this study. I will suggest to define it before using this notion. Why normalize TA and DIC to this value? I would move this part from the material and method section to the discussion where the ‘temperature minimum layer’ is defined and where the authors use this notion.

We agree with your comment. We have moved definition of temperature minimum layer (TML) from discussion to method section in the text. In addition, please see your comment “658, L8-9: ‘the temperature minimum layer’ is... the authors mean.”. According to your comment, we have also added the definition of TML to the abstract.

We have used the mean salinity of the TML (34.25) for normalization of n-DIC and n-TA because salinity was uniform in this layer (e.g. Figure 6b), and the TML is thought to retain winter conditions. Therefore, the salinity of the TML was likewise treated as an initial condition before the input of ice meltwater. We have added “Tomczak and Liefink (2005) proposed that a water mass beneath the summer surface water with temperature between -1.9°C and -1.5°C and salinity between 34.2 and 34.5 constitutes the TML, and the TML is thought to retain the winter condition. Therefore, we used a salinity of 34.25 for normalization of DIC and TA data as an initial condition before change by the input of ice meltwater.”.

We included n-DIC and n-TA data in the Results section. Therefore, we would like to keep the definition of TML and the normalization of n-DIC and n-TA in the Methods section. We also moved the depth information of TML from the Discussion section to the Results section.

P662 L11: the authors explain that the atmospheric pCO₂ has been measured during the study. Why not show the actual data instead of just a “mean value”? And more importantly, how come the mean atmospheric concentration was so low (366 μatm)? In the data base from NOAA (<http://co2now.org> or <http://www.esrl.noaa.gov>) the mean concentration of CO₂ in the atmosphere in 2006 was 381 μatm. Could the authors explain this difference? What could be the impact of this difference on this study?

You may have misunderstood the units of CO₂ concentration. We used “μatm” in this study, whereas NOAA CO₂ concentration data use “ppm”. NOAA air CO₂ data at the South Pole during our study period in the middle of January 2006 (ftp://aftp.cmdl.noaa.gov/data/trace_gases/co2/in-situ/surface/spo/co2_spo_surface-insitu_1_ccgg_hour_2006.txt) show that the CO₂ concentration, measured continuously, was 377.93 ± 0.14 ppm. We could not find a reading as high as 381 ppm.

As shown in the Methods section, pCO₂ (μatm) was calculated from xCO₂ (ppm) taking into account the water vapor pressure (e) and atmospheric pressure (P). For example, we used the mean air CO₂ concentration of 377.9 ppm at the South Pole during our study period as xCO₂, the saturated water vapor pressure of 5.7 hPa at mean air temperature (−1.0 ± 1.1 °C) at the Syowa station (near our sampling area) during our study period, and a mean atmospheric pressure of 988.5 ± 4.6 hPa at the Syowa station. We calculated pCO₂ (μatm) as follows:

$$\begin{aligned} p\text{CO}_2 &= x\text{CO}_2 * (P/1013.25 - e/1013.25) \\ &= 377.9 * (988.5/1013.25 - 5.7/1013.25) \\ &= 366.5 (\mu\text{atm}). \end{aligned}$$

This calculation is very close to our mean air pCO₂ of 366 μatm.

According to your comment, we have added air CO₂ data to all points in Figure 2a.

P662 L13: from the Figure 2, it looks like the surface pCO₂ decreases from 399 μatm at 36–37°E to 275 μatm.

We agree with your comment. Corrected accordingly.

P663 L1 and Figure 4: the authors should explain what are the AMSR-E data, where did they come from?

We agree with your comment. We have added information of AMSR-E to the text: “Satellite images of sea ice concentration near sampling stations were derived from passive microwave imagery from the Advanced Microwave Scanning Radiometer–Earth Observing System (AMSR–E) (<http://nsidc.org/data/amsre>; the Level-3 gridded product (AE_SI12) was downloaded from http://nsidc.org/data/ae_si12). Spatial resolution was a 12.5 km × 12.5 km grid and temporal resolution was 1 day; we used 8 days average of data in this study. For deriving sea ice concentrations, we used the Enhanced NASA Team (NT2) algorithm.”.

P662 and 663: 3.2 Satellite images of sea ice and Chl a concentrations. This section has nothing to do in the ‘Result’ part of the manuscript. These are not the authors’ results. It should be introduced in the discussion where the authors would use them.

Section 3.2 and Figures 3-5 were derived in this study (Please see your next comment “P663 L7: how did the authors derive the Chl a concentration?” “P663 L1 and Figure 4: the authors should explain what are the AMSR-E data, where did they come from?”). We have added information on our data processing to the Methods section, and we have clarified our derived results in the Results section.

P663 L7: how did the authors derive the Chl a concentration?

We have added information about SeaWiFS in the text: “Satellite images of the chlorophyll a concentration near our sampling stations were derived from Sea-viewing Wide Field of view Sensor (SeaWiFS) data from NASA (<http://oceancolor.gsfc.nasa.gov/SeaWiFS/>), specifically the standard level-3 products of 8 days composite SeaWiFS Chlorophyll concentration data. We derived chlorophyll a concentrations with the OC4 Chlorophyll-a algorithm (O’Reilly et al., 2000) (<http://oceancolor.gsfc.nasa.gov/REPROCESSING/R2009/ocv6/>). Spatial resolution was a 9 km × 9 km grid and temporal resolution was 1 day; we used 8 days of composite data in this study. Cannizzaro et al. (2013) compared chlorophyll a data from in situ and SeaWiFS measurements and found that they were highly correlated ($r^2 = 0.84$, $n = 289$) although SeaWiFS values were positively biased by 25%.”.

P663 L10: how come the authors have this significant increase of Chl a at the station FG3? Is it significant to show and discuss data after your sampling period as you are discussing the winter – summer transition based on 1-week sampling session?

We have added information about the satellite chlorophyll a data (Please see Reviewer #1 comment “P663 is uncertainty measure for Seawifs Chl available?” and your comment “P663 L7: how did the authors derive the Chl a concentration? ”).

In addition, the comparison of chlorophyll a data between in situ and SeaWiFS by Cannizzaro et al. (2013) provided the reliability of the SeaWiFS data to indicate the real ocean chlorophyll a concentration.

P663 L18-19: the sentence is unclear, according to the Figure 6, nDIC doesn’t seem constant at all. It will be helpful that the authors provide a range of values (as for nDIC than for nTA).

We agree with your comment. We have added the standard deviation of values “(1 SD = $\pm 5.4 \mu\text{mol kg}^{-1}$)”.

P663 L15: ‘C shape’ for seawater profile? This term is more widely used in sea ice literature...

We agree with your comment. We have removed this term (C shape) from the text.

P664 L3-4: low and high Chl a, compared to what? Why this separation? According to Figure 2, as suggested in the text, between 32° and 40°, the range of Chl is between 0.2 and 0.4 $\mu\text{g L}^{-1}$ (roughly) and not between 0 and 0.2. Between 40° and 58°, this range is slightly higher than previously, with 2 maxima... and looking at the Figure 5, the range of concentration observed during the survey is the same, so I strongly disagree that one area is biologically productive and the other is not. However, later on, the area at the edge of the sea ice cover becomes more productive than the area in open water. But what could be the impact on your discussion, goal of this manuscript?

We compared the chlorophyll a concentration throughout its longitudinal distribution during our study period. In the text, we did not indicate the range of chlorophyll a concentration although you pointed out that our chlorophyll a range was wrong. We indicated mean and standard deviation of the chlorophyll a concentrations.

We have examined the frequency test for chlorophyll a concentration between 32° and 40°E (see the following table). Most of the data were between 0.2 and 0.3 $\mu\text{g L}^{-1}$. We have added the median value of the chlorophyll a concentration to the text “(median = 0.2 $\mu\text{g L}^{-1}$)” and “(median = 0.4 $\mu\text{g L}^{-1}$)”.

Table. Number of samples for chl a. range for 32° and 40°E.

Chl. a range ($\mu\text{g L}^{-1}$)	Number of samples
<0.15	0
0.15-0.19	41
0.20-0.24	412
0.25-0.29	191
0.30-0.34	27
0.35-0.39	13
>0.40	0

Figure 2b shows an increase in chlorophyll a concentration around 40°E. Between 40° and 58°E, high chlorophyll a concentrations, up to 0.9 $\mu\text{g L}^{-1}$, were observed at 42.5°E and 51°E in our study area (Fig. 2b), indicating that biological productivity was spatially heterogeneous. Our statistics indicated that differences of chlorophyll a concentration between 37°–40°E and 40°–58°E were significant.

Figure 5 displays clear differences in the chlorophyll a concentration. Around 36°–40°E, the chlorophyll a concentration was less than 0.1 $\mu\text{g L}^{-1}$ (sky blue color) whereas it was greater than 0.1 $\mu\text{g L}^{-1}$ (green color) in the eastern part of our study area (e.g., 46°–60°E in Fig. 5d). Therefore, we believe that the 40°–58°E area was biologically more productive than the 37°–40°E area. In addition, the survey of the longitudinal distribution took a week, making it more likely that high chlorophyll a concentrations in the western part of the sampling area would be detected, as shown by the abrupt increase of chlorophyll a concentration at station FG3 (Fig. 4b).

P664 L11-12: I suggest changing the sentence. Despite the fact that the SST increases (which should increase the pCO₂), the sea surface pCO₂ decreases due to the strong impact of the dilution process. But then the authors need to discuss about the increase of the Chl a that occurs at the same time... So which process is dominant in the pCO₂ decrease? To discuss the impact of chl a, SST and SSS on the pCO₂ I will strongly suggest to do a X,Y,Z dot plot with: temperature in X, salinity in Y and pCO₂ in Z (and Chl a).

We agree with your comment. As shown below, we have made an X-Y-Z plot for SST, SSS, and pCO₂ (a) and for SST, SSS, and chlorophyll a (b). However, these figures do not show clearly which process was dominant.

When we examined the longitudinal distributions of pCO₂ (Fig. 2a) and chlorophyll a (Fig. 2b), we found a clear relationship between pCO₂ and chlorophyll a: when the chlorophyll a concentration increased, pCO₂ decreased, and vice versa, particularly around 40°–48°E. On the other hand, in the area affected by freshwater input (SST < 33.0), particularly at 37°–40°E (Fig. 2d), pCO₂ decreased owing to dilution by meltwater from sea ice. Therefore, our study found clearly separated areas with dominant processes, although in some areas both were present and a dominant process could not be distinguished.

In addition, we understood that Figure 10 provided useful information. Given that pCO₂_{cal} indicated only thermodynamic effects on surface-water pCO₂, the difference between pCO₂_{cal} and pCO₂_{obs} reflects other effects (e.g. biological effects) on surface-water pCO₂. Gas exchange was minimal through the transition from winter to summer (Table 2), supporting the dominance of biological pCO₂ modification around 40–58°E.

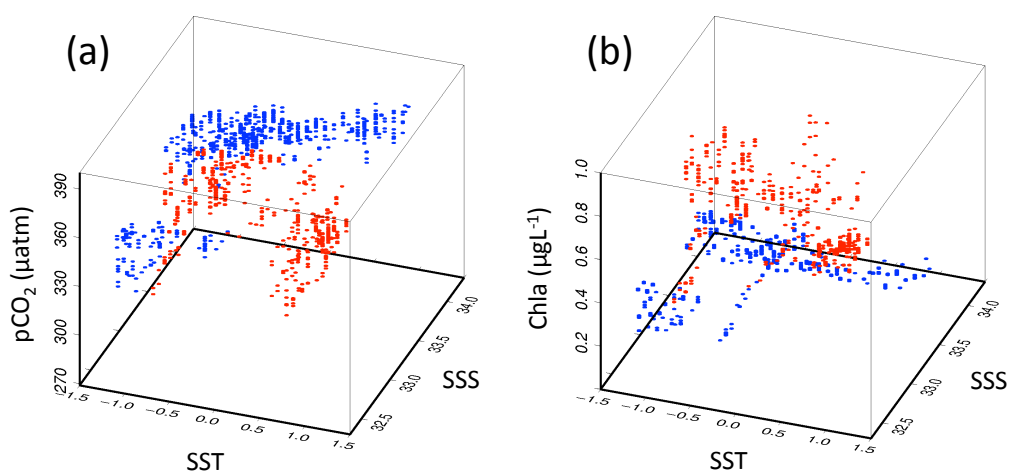


Figure: Three-dimensional plots of SST, SSS, and pCO₂ (a) and SST, SSS, and chlorophyll a (b). Blue and red indicate data from 32–40°E and 40–58°E, respectively.

P664 L17-23: this section should be part of the introduction.

Please see your comment “P659, L14-18: it would have been interesting to provide a range of concentration reported in the literature..... in his reading.” and Reviewer #1 comment “P664 line 17-20 repetition, just refer to intro”

P665, L5-13: the authors calculate a pCO₂ in the TML (50m depth) from TA and DIC, which is supposed to be the winter pCO₂, at 50m depth. Then they compare this value with an estimated pCO₂ in the surface seawater by extrapolation. Can we really compare the winter pCO₂ in sea surface water with a 50 m depth water? The pCO₂ of the surface water may change significantly between spring and winter (e.g. Shadwick et al., 2014...) .

In winter, the mixed layer is deeper than 50 m, indicating that the water is well mixed and coincident with the TML. During the winter-to-summer transition, warming, freshening and biological processes modify the surface water components while the TML retains its winter characteristics (Ishii et al., 1998, 2002; Rubin et al., 1998; Hoppema and Goeyens, 1999; Pondaven et al., 2000). Please see reviewer #3 comments “P668, eq 10. So far, I understand that the while it is not the case. Such formulation is convenient to compensate any input of rain/snow of low DIC content. How is it valid for sea ice?”.

P665 L14-23: all this section is already in the introduction, and more work should be done on it (see previous comments). Instead of repeating the same information, it would have been interesting to discuss how the results fit with what we can find in the literature.

We agree with your comment. We have removed this part from text. Please see your comments “P659, L14-18: it would have been inter.....without giving any information to the reader to guide him in his reading.” and Reviewer #1 (P664 line 17-20 repetition, just refer to intro).

P665 L24-29: I suggest adding, on the Figure 8, the data from the bottom water as TA and DIC are stable. These data could be used as the starting point. Then the authors could discuss on how the different processes, such as precipitation of calcium carbonate, gas exchange, biology could affect, the surface waters.

We agree with your comment. We have added n-DIC and n-TA values in TML in Figure 8. We also added explanation to the text “In addition, the decreasing of n-DIC and n-TA from TML during winter to summer transition (Fig. 8) would reflect photosynthesis or a CO₂ release.”.

We also added explanation to the text “The increase of n-TA from TML during winter to summer transition (Fig. 8) would reflect ikaite dissolution during the ice melting season.”

P666 L1-5: the station L1 has only 2 data shown on the Figure 1, it seems easy then to determine a relationship. All the stations are characterized by the sea ice melt and retreat (according to the Figures 3 and 4), not only the station L1 and FG3.

Please see your comment “Figure 8: the figure caption mentions that these data are the values above the TML. Going back to the Figure 6, above the TML, the different stations have between 2 to 4 points by station. It seems to have more data on the Figure 8, especially for the station 1.”.

P666 L13: how can the authors estimate an ice thickness from the difference of salinity in the summer surface water?

First, consider a $1\text{ m} \times 1\text{ m} \times 20\text{ m}$ water column during the season with no sea ice. We seek to determine ice thickness X by its effect on salinity in the volume V of the underlying water column. Therefore, the water depth under the sea ice is $(20 - X)$ (m) during the sea ice season. The total amount of salt, calculated from salinity S , should be equal in the two seasons:

$$S_{\text{sea ice season}} \times V_{\text{sea ice season}} = S_{\text{no sea ice season}} \times V_{\text{no sea ice season}}$$

$S_{\text{sea ice season}}$ is 34.25, $V_{\text{sea ice season}}$ is $(20 - X)\text{ m}^3$, $S_{\text{no sea ice season}}$ is 32.5, and $V_{\text{no sea ice season}}$ is 20 m^3 . Therefore, X is 1.02 m. From this calculation, we decided on a sea ice thickness of 1 m.

P666 L16: why using the estimation from Dieckmann et al (2008)? Recent work from Rysgaard et al (2012, 2013) suggests that the method used by Dieckmann et al (2008) may underestimate the amount of ikaite precipitated in the ice. Moreover, the assumption that all the ikaite in sea ice dissolved in the top 20 m depth of the water column is wrong. Recent paper from Rysgaard et al (2013), Sogaard et al (2013), Geilfus et al (2012) show that ikaite dissolved in sea ice, and that this dissolution, allow the ice to be a sink of atmospheric CO₂. Therefore the impact of this dissolution is mainly in the “sea ice pool” and not in the “seawater pool”.

We agree with your comment. According to your suggestion, we have recalculated based on Rysgaard et al. (2013) “The amount of ikaite in Antarctic sea ice has been estimated to be 100–900 $\mu\text{mol kg}^{-1}$ ice (Rysgaard et al., 2013). Under the assumption that all ikaite in sea ice is dissolved in sea ice, and that meltwater is supplied to the upper 20 m of the winter water column, the meltwater input should alter n-DIC from 2206.6 $\mu\text{mol kg}^{-1}$ to 2211.6–2251.6 $\mu\text{mol kg}^{-1}$ and n-TA from 2327.6 $\mu\text{mol kg}^{-1}$ to 2337.6–2417.6 $\mu\text{mol kg}^{-1}$ during the transition from winter to summer at station L8.”.

According to your suggestion, we have changed the sentence about ikaite dissolution within sea ice to the following: “Under the assumption that all ikaite in sea ice is dissolved in sea ice, and that meltwater is supplied to the upper 20 m of the winter water column,.....”.

P666 L20-24: what is the goal of this sentence? Other work that Rysgaard et al (2013) and Nomura et al (2013) have been made on ikaite precipitation within sea ice (e.g. Geilfus et al (2012, 2013), Sogaard et al (2013), Papadimitriou et al (2013)...). What are the “complex mixture of processes” that appear to be operating? Why not discussing them? Or at least cite them? Are the authors only discussing the station L1 and FG3? So what is the point to show all the remaining stations?

We agree that there have been many recent studies of ikaite precipitation and dissolution (Geilfus et al., 2012, 2013; Rysgaard et al., 2013; Nomura et al., 2013b; Papadimitriou et al., 2013). For stations L1 and FG3, we could attribute the responsible processes confidently (ice melting for L1 and biological productivity for FG3) on the basis of carbonate system parameters (Fig. 8), because these stations displayed extreme examples of each process (Table 2). For stations L2–7, we assigned each process proportionately in Table 2; however, Figure 8 shows that it is difficult to identify a dominant process. According to your comment, we have removed “the details are still poorly constrained and require further research (Nomura et al., 2013b)” to avoid any misunderstanding. We also have changed the text as follows: “Our estimate is consistent with ikaite formation and dissolution, which has been proposed as a contributor to changes in carbonate system parameters during the ice melt season (Fransson et al., 2011; Rysgaard et al., 2013).”.

P666 L25 to P667 L5: is it really significant to look at the difference of pCO₂ between the seawater at 50m depth and the surface seawater (see previous comments)? How come the atmospheric pCO₂ presented in this study are so low compared to the general data base of atmospheric pCO₂ as provided by the NOAA? This will have a huge impact of the estimation of the CO₂ fluxes. How can the authors estimate the surface water pCO₂ week by week? The dataset provided in the manuscript is running for a single week and show how sensitive is the surface pCO₂ with the dilution process, biological activity. So, how can the authors go back in time to estimate the CO₂ fluxes, the temperature dependence?

Please see your comments “P662 L11: the authors explain that the atmospheric pCO₂ has been measured during the study....What could be the impact of this difference on this study?” and “P665, L5-13: the authors calculate a pCO₂ in the TML (50m depth) from TA and DIC,..... change significantly between spring and winter (e.g. Shadwick et al., 2014...) . In addition, please see reviewer #3 comments “P668, eq 10. So far, I understand that the while it is not the case. Such formulation is convenient to compensate any input of rain/snow of low DIC content. How is it valid for sea ice?”.

P668 L15: could the authors provide reference for the equation?

We made this equation. Therefore, there is no reference.

P668 L20: doing the math with the table 2, we don't reach the exact same numbers.

We agree with your comments. We made re-calculation in order to balance for each component.

P669 L14-15: how can the authors have surface water supersaturated in CO₂ compared to the atmosphere? If we take into consideration an atmospheric CO₂ concentration of 381 μatm (as reported by the NOAA in 2006), their observations only show few data supersaturated. And if we plot the atmospheric concentration at 380 μatm in their Figure 9, we never have supersaturated surface water.

Please see your comment “P662 L11: the authors explain that the atmospheric pCO₂ has been measured during the study..... What could be the impact of this difference on this study?” and “P666 L25 to P667 L5: is it really significant to look at thehow can the authors go back in time to estimate the CO₂ fluxes, the temperature dependence?”.

P669 L15-17: the Chl a concentration at FG3 increases because the ice melt and the station is just at the ice edge. The Chl a at the station L1 to 8 increases with the melt of the ice then decreases as the ice disappears (Figure 5).

Please see your comment “P664 L3-4: low and high Chl a, compared to what?...But what could be the impact on your discussion, goal of this manuscript?”

P669 L23: again, the pCO₂ calculated is calculated from a winter TA and DIC from 50 m depth, can we compare a surface water pCO₂ with a 50 m depth pCO₂?

Please see your comment “P665, L5-13: the authors calculate a pCO₂ in the TML significantly between spring and winter (e.g. Shadwick et al., 2014...) .”.

P670 L5-8: this sentence is unclear... How can it be that the surface seawater could be a source of CO₂? It's under saturated (according to the authors results), their estimation show that the ocean is acting like a sink for the atmospheric CO₂ (cf. Table 2), so I don't understand the point.

Here we are discussing CO₂ flux, not the contribution of flux to the pCO₂ variation (Table 2). We have changed the text to the following: “During ice retreat, surface water was an early CO₂ source to the atmosphere because of its supersaturation with respect to the atmosphere. Therefore, F_{CO₂ (a-w)} reached up to +0.9 mmol C m⁻² day⁻¹ (Fig. 9b).”.

In Table 2, negative values of (ΔpCO₂ w) F indicate that surface water pCO₂ decreased because of CO₂ release from the water to the atmosphere, meaning that the ocean acts as a source for atmospheric CO₂. Because we stipulated that there was no CO₂ exchange between seawater and the atmosphere, (ΔpCO₂ w) F was zero during the ice season. However, a few weeks after melting of sea ice (after January 2006), surface-water pCO₂ was supersaturated with respect to the atmosphere (Figs. 9a and b). Therefore, at least until our observation time in mid-January 2006, the ocean acted as a source for atmospheric CO₂. Please see your next comment “P670 L9-11: of course the air-ice CO₂ fluxes are dom..... with an under saturated surface seawater?”.

P670 L9-11: of course the air-ice CO₂ fluxes are dominant if the all area is fully covered by ice... However, how can we have an efflux of CO₂ from the ocean to the atmosphere with an under saturated surface seawater?

After sea ice retreated (Figure 9a), surface-water pCO₂ was supersaturated with respect to the atmosphere because the temperature effect on surface-water pCO₂ dominated salinity and biological effects. Therefore, the CO₂ flux was positive (Figure 9b). Subsequent

photosynthesis and meltwater input would have lowered surface-water pCO₂ to the point that it was undersaturated with respect to the atmosphere, at which point surface water became a CO₂ sink. Please see your comment “P670 L5-8: this sentence isso I don’t understand the point.”.

Figures and Table:

Table 1: why the stations are not chronically ordered? Are there any reasons for the stations name? FG3 imply FG1 and 2, why are they not included in the present manuscript?

We planned Stations FG1 and 2. However, due to bad weather and limitation of the ship time, we canceled Stations FG1 and 2.

Figure 2: this figure should not be represented like that. We should have a map (similar to the Figure 1) where the cruise track is showed, and, on the cruise track, we should see the different variables (T, S, Chl, pCO₂) and the different stations.

We agree with your comment. We have added a map to the Figure 2.

Figure 6: the authors don’t seem to have any data of DIC and TA deeper than 200m. So why extent the plot line until 300m?

We agree with your comment. Please see your comment “P661, L15: could the authors provide the sampling depth....depth, which is not the surface water.”.

Figure 7: are the data for the Figure C included in the Figure A?

No, they are not. Figure 7a indicated data between 32–40°E. On the other hand, Figure 7c indicated data between 40–58°E.

Figure 8: the figure caption mentions that these data are the values above the TML. Going back to the Figure 6, above the TML, the different stations have between 2 to 4 points by station. It seems to have more data on the Figure 8, especially for the station 1.

The TML was at 50 m depth at stations L1–8 and at 200 m at station FG3. For combination of nTA and nDIC, we have two points (pink) for Station L1, four points (orange) for Station L2, zero point for Station L3, one point (green) for Station L4, five points (blue) for Station L5, four points (sky blue) for Station L6, three points (navy) for Station L7, five points (gray) for Station L8, and nine points (red) for Station FG3.

The authors should read through the manuscript and correct grammar and correct fluffy sentences. Few examples:

P658, L1-2: ‘vertical profiles of the aqueous carbonate system’

We agree with your comment. We have changed to “vertical profiles of the carbonate system parameters”.

P658, L20: ‘flux between the sea and the air...’

We agree with your comment. We have changed to “flux between ocean and atmosphere”.

P658, L22: ‘abyssal depth’

We agree with your comment. We have changed to “deep depth”.

P659, L15-17: low (...) or high (...), ... as compared to the atmosphere. The authors may just say supersaturated or under saturated compared to the atmosphere.

We agree with your comment. We have changed through the text.

P659, L18: lower than in the air... Please use the word atmosphere and use the saturation state.

We agree with your comment. We have changed through the text.

P661, L11: the authors did not “collect” temperature and salinity with a CTD, but they “measured” T and S with the CTD.

We agree with your comment. We have changed to “measured”.

P661, L14: the authors did not calibrate the salinity data with the “bottles samples”; they took sample to calibrate the salinity sensor.

We agree with your comment. We have changed to “In addition, seawater samples were taken to calibrate the salinity sensor”.

P662, L12: ‘lower than that of air...’

We agree with your comment. Corrected accordingly.

P664, L21: ‘higher to lower than atmospheric’

We agree with your comment. Corrected accordingly.

Response to Reviewer # 3

General comment I have a mixed feeling about the work of Nomura et al. On the one hand, the data-set presented here is not very common. I've been surprised by the winter under saturation in pCO₂, that is not a very common feature in the Southern Ocean ice covered areas. There is also an attempt to present also air-ice/sea CO₂ fluxes that is pretty original. But for me the reconstruction of the assessment of the contribution of the different factors is not fully convincing and very exciting. There are some assumptions that are not 100% robust, especially in a sea-ice covered zone. I'm confident that the authors, who are well versed in sea ice studies, know how the ice can affect the underlying water. From a physical point of view sea ice affects turbulence and then piston velocity. Sea ice hosts also peculiar biogeochemical processes (DIC changes, precipitation of CaCO₃), and these processes are ignored in the computation of eq (1) and following equations. I know that it could be difficult to take sea ice processes into account. But at least the authors could discuss the residual. Such discussion might be the most interesting part of the manuscript, since residual corresponds in some extent to the contribution of sea ice peculiar processes to surface pCO₂ changes. It would have been something new, compared to the computations presented in the present form that have been done several times in the past.

We are grateful for your favorable assessment. We agree with your comments. According to your comments, we have revised our discussion of the dissolution of CaCO₃ in sea ice and modified the carbonate system parameters within the water column. In addition, we have examined the effect of CaCO₃ dissolution on pCO₂ in the surface water.

Detailed comments:

P664, line 6. I do not see so much demonstration here. The chl a concentrations are not so large compared to what can be encountered in marginal ice zones. Furthermore, in the manuscript, pCO₂ appears to be mainly affected by temperature, rather than primary production. In some other sea ice marginal zones, huge effects of primary production of primary production on pCO₂ are observed, but, in my opinion, it is not the case here.

Please see Reviewer #2 comment “P664 L3-4: low and high Chl a, compared to what? Why this separation?.....more productive than the area in open water. But what could be the impact on your discussion, goal of this manuscript?”.

P665, line 6. Why do you present so many digits, while obviously the last one is not significant?

We agree with your comment. We have removed last one digit.

P665, line 23. I would have replaced “would be supported by these results” by “are supporting these observations.

We agree with your comment. Corrected accordingly. Please see Reviewer #2 comment “P665 L14-23: all this section is already in the introduction, and more work should be done on it (see previous comments). Instead of repeating the same information, it would have been interesting to discuss how the results fit with what we can find in the literature.”.

P665, line 5. same comment that for P664, line 6.

If we remove the last one digit, there will no value (all data will be zero) for chlorophyll a concentration. Therefore, we cannot understand about your comment. Please see Reviewer #2 comment “P661, L5-10: precision and accuracy?”.

P666, line 14. I would have written “that sea ice thickness was around 1m thick” instead of “a sea ice thickness of 1.0 m here in winter”

We agree with your comment. Corrected accordingly.

P666, line 15. I do not see the purpose of this paragraph. I would have understood that you compare previous estimate from the literature, if you would have proposed your own assessment of the impact of CaCO₃ sea ice precipitation. But this is not the case, since these assessments are based on the results of (Dieckmann et al 2008) and you do not provide an assessment of how much CaCO₃ could have been dissolved in the surface layer either. So for me, this part is useless for the understanding of the manuscript. P666, line 24. Finally, what is your overall feeling? Is CaCO₃ precipitation a significant process, or not? While reading your manuscript, I have just the feeling that this process can occur locally (only one station) and is not significant. You could address the significance of this process on pCO₂ more clearly.

Please see Reviewer #2 comments “P666 L16: why using the estimation from Dieckmann et al (2008)? Therefore the impact of this dissolution is mainly in the “sea ice pool” and not in the “seawater pool” and “P666 L20-24: what is the goal of this sentence?...Are the authors only discussing the station L1 and FG3? So what is the point to show all the remaining stations?” and Reviewer #1 comment “P669 line 6-8 could an actual value be calculated based on the earlier estimate from delta DIC (p666 line16-20).”.

P667, line 4. Why don't you try to assess the influence of sea ice melting? If you know DIC and TA of the ice, you could derive the effect of sea ice melting using the shift of salinity as a tracer of sea ice melting.

We agree with your comment. Please see Reviewer #1 “P669 line 6-8 could an actual value be calculated based on the earlier estimate from delta DIC (p666 line16-20).”. We have calculated the effect of dissolution of carbonate to the surface-water pCO₂ based on the results from Rysgaard et al. (2013) because we do not have sea ice samples for DIC and TA.

P668, line 5. Why did you use parameterization from Wanninkhof [1992], while other gas transfer velocity coefficients were developed more specifically for the Southern Ocean (see [Edson et al., 2011; Ho et al., 2006] for example) and could be of interest for this study? You have made the assessment of a linear relationship between sea ice concentration and the overall fluxes. So you suppose that when the ice is present there are no fluxes, and when sea ice is not present, parameterisation developed for open water can be used readily. But parameterization of gas transfer velocity depends on turbulence that it is not the same in open ocean, and sea ice covered areas. Indeed fetch and turbulence that are also affected by sea ice. Thus in my opinion transfer velocity coefficients for open ocean cannot be used readily water for sea ice covered area by just "switching off" the fluxes in the presence of ice. In addition, work of Loose et al has shown that they are specific turbulence generated in sea ice covered areas that you do not take into account.

We understand that studies vary on gas exchange in the polar ocean (Ho et al., 2006, Edson et al., 2011). In this study, we used observation data from the open ocean in summer. In addition, we are assuming that sea ice acts as an insulator or not under the normal water condition. Therefore, we would like to keep Wanninkhof (1992), which was cited in our sources (e.g., Bakker et al., 1997), to compare with our results.

We have added, "It is said that there are discussions for the parameterization of the gas exchange process in the polar ocean (Ho et al., 2006; Edson et al., 2011). In this study, observational data were obtained from the open ocean and we assumed that sea ice act as an insulator or not under the normal water condition. Therefore, we used the parameterization of Wanninkhof (1992) in this study." We also added two references (Ho et al., 2006; Edson et al., 2011) to the text.

P668, Line 7. You should precise in the text that wind speed is derived from NCEP re-analysis.

We agree with your comment. Corrected accordingly.

P668, eq 10. So far, I understand that the primary production contribution is derived from the change in DIC, but how do you take into account the contribution of air-sea exchanges to changes in DIC in the mixed layer? It is not clear to me. Furthermore, by using nDIC to correct DIC for salinity change, you assume that any meltwater does not contain DIC, while it is not the case. Such formulation is convenient to compensate any input of rain/snow of low DIC content. How is it valid for sea ice?

In general, changes in DIC are said to be the result of net biological uptake and respiration and net air-sea CO₂ exchange. In addition, water mixing also causes DIC to vary. In this study, the estimated CO₂ flux between the ocean and atmosphere was very small during the winter-to-summer transition because we assumed that sea ice blocks the CO₂ flux at the ocean-atmosphere interface. These results are also supported by the small contribution of $(\Delta p\text{CO}_2)_F$ to $\Delta p\text{CO}_2_{w(w\text{ to }s)}$ (Table 2). In addition, during the seasonal transition to warm conditions, vertical stability in the water column increased and it was difficult to exchange water between the upper TML (at near-freezing temperature) and the subsurface water.

Therefore, we think that net air–sea CO₂ exchange and water mixing have minimal influence on changes in DIC in this study.

Referring to the definition of salinity normalization, we considered the S/DIC ratio to be constant even in sea ice. Therefore, the DIC content of meltwater depended on the salinity. For the effect of carbonate crystals in sea ice on carbonate chemistry in the upper ocean, please see the Reviewer #1 and #2 comments “p666 line 16 the authors provide a maximum ikaite concentration.....Or at least an error margin provided?”, “P669 line 6-8 could an actual value be calculated based on the earlier estimate from delta DIC (p666 line16-20).”, “P666 L16: why using the estimation from Dieckmann et al (2008)?“sea ice pool” and not in the “seawater pool”, and “P666 L20-24: what is the goal of this sentence?So what is the point to show all the remaining stations?”.

P682, Fig. 2. What the grey parts corresponds to? I guess that it should help to improve the readability of the figures, but in my opinion, a figure should be as simple as possible with no "extra elements".

We agree with your comment. We have removed the gray parts from Figure 2.

P689, Fig. 9. I would replace air-ice-sea by air-ice/sea to underline that it is correspond to bot-air-ice and air-sea exchange. air-ice-sea is a bit confusing, because it might corresponds also to ice-sea fluxes, or to air-sea exchange through the ice.

We agree with your comment. Please see Reviewer #1 comment “Equation 11 is a bit confusing due and again the use of “Air-ice-sea FCO₂” as a variable name adds to the confusion. I would suggest to just mention that a term FCO_{2i} is added to equation 7 and limit eq 11 to FCO_{2i}=Fice S/100 (note the minus sign is also confusing make sure sign convention is consistent , generally I would expect fluxes to be additive. If they then turn out to be negative due to a specific process, it maybe so, but the equation should state the addition.”.

References

Edson, J. B., C. W. Fairall, L. Bariteau, C. J. Zappa, A. Cifuentes-Lorenzen, W. R. McGillis, S. Pezoa, J. E. Hare, and D. Helmig (2011), Direct covariance measurement of CO₂ gas transfer velocity during the 2008 Southern Ocean Gas Exchange Experiment: Wind speed dependency, *Journal of Geophysical Research: Oceans*, 116(C4), C00F10, doi:10.1029/2011JC007022.

Ho, D. T., C. S. Law, M. J. Smith, P. Schlosser, M. Harvey, and P. Hill (2006), Measurements of air-sea gas exchange at high wind speeds in the Southern Ocean: Implications for global parameterizations, *Geophys. Res. Lett.*, 33(16), L16611, doi:10.1029/2006GL026817.

Wanninkhof, R. (1992), Relationship between wind speed and gas exchange over the ocean, *J. Geophys. Res.*, 97(C5), 7373-7382.

[Thank you very much for providing the references.](#)