Response to author comment on my review of tc-2014-88

The authors have clarified many aspects and improved the paper. However, I still find that the following points need improvements.

- The authors claim that processes related to cooling-warming of samples prior to bulk CO_2 measurements likely will not affect the results; I give some more arguments and reference from field work that indicate fundamental changes in microstructure during similar cooling-warming, and suggest possible effects on the method. These should be considered a bit more critically in the text.

  Indeed, as shown as shown for example by Cole (2001), reference provided by the referee, in field observations, warming/freezing cycles in the sea ice are likely to affect its microstructure. However, we believe that the impact of those changes linked with the ice core storage at low temperature do not significantly affect our gas composition measurements.
  Firstly, cooling in these conditions proceeds homogeneously from the outside to the inside of the core, therefore insulating the inner parts of the brine system from the outer atmosphere as impurities (including gases) are progressively enriched in the remaining part of the brine system. Phase changes might result in some internal fracturing of the sample but this does not result in any visible weakening of the outer part of the cores that might have been responsible for partial loss of the gas content.
  Secondly, Stefels et al. (2012, Marine Chemistry, 128-129, 34-43) compared sea ice measurements (DMS+DMSP) directly performed "on ship" to home-lab measurements performed on twin half-core two years later, and showed no statistically significant difference for this volatile compound.
  Finally, in an attempt to detect a potential bacterial activity during long term storage (2-3 years) at low temperature (-25°C) in the dark, we (Becquevort et al., unpublished data) have re-measured the oxygen concentration on twin-core samples from the NBP-1998 Antarctic winter cruise after more than two years, without seeing any significant changes in the concentrations.
  Therefore, to satisfy the referee, we added few lines in the methods section and in the discussion saying that microstructure may have change in the cooling-warming processes but these changes should only have a minor impact in our measurement.
  L201: "*This method is only valid if the ice is permeable at the in situ conditions [Geilfus et al., 2012b] and microstructure changes resulting from cooling during storage and warming prior to analysis are assumed to have a minor impact on the bulk ice* pCO_2."
  L423: '*Therefore, changes in bulk ice* pCO_2 *values are less variable, reflecting mostly internal melting due to temperature and resultant salinity changes in the ice cover. However, we assumed that microstructure changes due to cooling-warming processes during storage do not have any significant impact to our measurements.*

- One main conclusion proposed by the authors is now: Both techniques, measuring the pCO_2 within sea ice and brine, address different parts of the brine network. However, in this connection the authors have corresponded with arguments (that are also mentioned in the paper) that I find confusing (e.g. that the bulk pCO_2 measurements include information from isolated brine pockets). Have I misunderstood something in the Geilfus et al. (2012)

methodology paper? As I understand the latter, pCO_2 is measured from equilibration of a standard gas with the ice samples warmed to in-situ temperatures, and it is then the connected brine pore space that the gas equilibrates with, not the isolated brine pockets. If I am correct here, then this should be corrected in the paper (e.g. Line 418), and it should be noted that both the bulk pCO_2 the in situ pCO_2 methods can only measure CO_2 within interconnected brine networks. Regarding the comparison of the methods, the conclusion that in situ brine pCO_2 and bulk ice pCO_2 relate to different fractions of the brine network (the most mobile one), is acceptable, provided the critical notes on the in situ sack-hole technique by the authors. However, a note on disadvantages of the bulk ice pCO_2 technique, especially the possible consequences of microstructure change, brine expulsion and air space (see my comments below) need to be added.

The brine pCO_2 measurements take into account all the brine that seep into the sackhole. Therefore, if small brine pockets are isolated within the ice structure and does not seep into the sackhole, it will be impossible to sample them. This is one of the disadvantages of the sackhole technique. While, using the bulk ice pCO_2 technique, these small, isolated brine pocket will be in equilibrium with the standard gas and measured. This difference is mentioned in the manuscript (L423) and could explain the differences reported between both methods.

Regarding the comments dedicated on the microstructure changes, brine expulsion and air space:
Our previous comments should demonstrate that we could reasonably assume that microstructure changes will have a minor impact on our measurements. Moreover, the concentration of bulk ice pCO_2 reported in this manuscript ranged from 9 to 193μatm. If we had cracks and/or infiltration of atmospheric air (atmospheric concentration ≈ 400 μatm) into our samples, it should promote an increase of the bulk ice pCO_2 to similar level as the atmospheric concentration. This is not the case and supports the idea that such processes may not have affected our samples or that these processes did not have a significant impact on our results.

- Fig. 10 and text: A subsection 5d is termed "comparison" between Antarctic and Arctic. Based on Fig. 10 a difference between the regions is supposed, and written in the present text (L. 447...). In my opinion this should be tested statistically, even if the causes are unknown. Also, Fig. 10a would be, as noted, much more informative, if the y-scale would be changed.

  We will add a fourth panel in the figure 10 to satisfy the reviewer. We also performed a statistical test confirming the difference between the Arctic and the Antarctica (1-way ANOVA test, $F_{1,210}$=30.73, {p<0.001}) and we added this information in the text (L456).

- I still find some passages about brine convection speculative, mentioning too frequently the work by Brabant (2012) that is not per-reviewed. Also, if, as on Lines 331 ff., Rayleigh numbers estimated by Brabant (2012) are explicitly mentioned, then Ra should at least be defined. In my opinion, Brabant (2012) may be referenced, yet not in the detail given here.

  We add the definition of the Rayleigh number at the line 334: "*Brine convection is driven by*

> *the density difference between high-salinity brine in the ice and the seawater underneath. Its onset and strength can be described by the mushy-layer Rayleigh numbers (Ra)[Wettlaufer et al., 1997].”*

Provided that the authors address these aspects in the specific comments, which I rate as minor improvements, I recommend the paper for publication in “The Cryosphere”.

Sönke Maus, Trondheim, 20.10.2014

Specific comments

The specific comments on the unclear topics include my original comments (including authors text in italic), the authors answers in italic, my response in bold.

P 3269, L 21 –> subsequent analysis - how much time did it take between sampling and analysis? This might have an effect on the measurement quality.

→*The ice was sampled in Oct. 2007 and the analyses were performed during the year 2013. The ice samples were stored all time in a freezer at -30°C, impeding any exchange with the atmosphere as, at this range of temperature, the sea ice brine volume was significantly lower than the permeability threshold 5% as described by Golden et al (2007). Storage in the dark will cancel primary production and the effect on photosynthesis on CO_2. Bacterial activity could in principle occur, but at these low temperatures rates must be negligible. Storage temperature is below reported bacterial rates in literature. We have already tested, in a few occasions, the impact of storage on gas composition for oxygen. It was clear in these tests that no significant change occurred after 3 to 4 years of storage. If bacterial activity did happen it would result in increased CO_2 concentrations. However, pCO_2 in bulk ice was very low indicating that this was not the case, and that CO_2 diffusing into the ice core during storage was also not taking place. Thus, we assume that the storage of the ice will not affect the quality of our measurements.*

→→A problem with 6 years of storage may be the possibility of crystal structure rearrangement (due to presence of liquid also at -30 °C). This may add to my arguments below, that the in situ sea ice microstructure very likely cannot be restored by cooling-warming of samples. However, my main concern, as outlined below, is the cooling process, and a relatively short time interval after it.

If you consider that $O_{2(g)}$ may act like $CO_{2(g)}$, as explained earlier, we have tested, in several occasion, the impact of storage on gas composition. We haven't found any significant changes after up to 4 years of storage. So even if the crystal structure change during the storage, it doesn't have any impact on the gas composition. In addition, Stefels et al. (2012, Marine Chemistry, 128-129, 34-43) compared sea ice measurements (DMS+DMSP) directly performed "on ship" to home-lab measurements performed on twin half-core two years later, and showed no statistically significant difference for this volatile compound.

P 3270, L 1-4 –> After 24 h, the ice sample was assumed to have returned to the brine volume and chemical conditions at the in situ temperature.... Such a return is, after expulsion of brine from the core, very unlikely. I comment more on this aspect in the methods discussion.

→*In the paper presenting the method (Geilfus et al., 2012 - Journal of Glaciology, 58(208), 287-300) tests performed on ice samples (with bulk salinities of 4-6-8-10-16-23) showed that it takes less than 4 hours to warm up the ice from -30°C to -2°C. To ensure an optimal equilibration between our sample and the standard gas, we extend the equilibrium time to 24 hours. Also, the samples are selected in the very central part of the core (between 3 and 7 centimeters from the outside surface of the core), making it unlikely that brines were expelled from that part of the core during storage…*

→→Brine is not expelled during storage but during cooling. Also, as brine expulsion is the

consequence of expansion during partial freezing of brine, and this process is expected to take place everywhere in the sample, also in the center. I comment further on this below in the note on P 3277, L 26-.

Indeed, brine is expelled during the storage of the ice samples due to its cooling. This cooling occurs from the outside of the sample to the inside. Meaning that while the centre of the ice core cool down, its edge is already cooled and seals the sample, especially as we used ice core of 14 cm diameter (we added this precision in the text). Furthermore, we removed 4cm of the side of the ice core prior to analysis.
See previous comments and more below in the note on P3277, L-26.

P 3270, L 6 →*This method is only valid if the ice is permeable at the in situ conditions*. This condition is probably not the only problematic issue of the method (see last note on return of ice to in situ conditions: the validity will depend on the question to what degree the in situ microstructure will be restored after cooling/storage/warming). However, as you frequently mention/assume a 5% threshold for permeability, you should then indicate in the presentation below (Fig.6) for which samples this condition may not be fulfilled.

→*The ice structure won't affect the carbonate system of the ice samples and won't affect the method to measure the bulk ice p*CO_2*, as describe in the methodological paper (Geilfus et al 2012, Journal of Glaciology).*

→→According to the Geilfus et al 2012 methodological paper you mention, the method to measure bulk pCO_2 **is only valid if the ice is permeable at in situ conditions. Hence, the ice structure may affect this method.**

In the Geilfus et al (2012) methodological paper, we specifically mentioned that the ice structure do not affect our method. By ice structure we have in mind the crystal types (granular vs columnar), size and organisation. The sea ice permeability is a theoretical notion based on the probability to find some interconnectivity throughout the pore space (Golden et al 2007). This should be not affected by change in ice structure at micro-scale. We acknowledge that permeability of granular vs columnar ice is different, but cooling will not affect ice structure at that scale.

P 3277, L 26 - P3278, L 11 →*In comparison...window*. Here bulk ice pCO_2 and brine pCO_2 measurements are discussed. First I cannot understand, why small isolated brine pockets are included in the bulk ice measurement (that due to the methods section is only valid for permeable ice samples).

→*The bulk ice p*CO_2 *takes into account the* CO_2 *dissolved within the brines and into its gaseous form as gas bubbles trapped within the ice structure. The brine p*CO_2 *is a direct measurement of the* CO_2 *concentration dissolved within the brine. Therefore, if a small brine pocket is totally isolated (meaning not connected to the brine channel network) within the ice structure, it may not be possible to sample it using the sackholes technique. While, measuring the bulk ice p*CO_2*, we will have the possibility to take this brine pocket into account during our analysis. We added these precisions in the text. This difference may explain the difference between the two different types of measurements. For more details on the analysis of the bulk ice p*CO_2*, please refer to the* *methodological paper: Geilfus, N. X., B. Delille, V. Verbeke, and J. L. Tison (2012), Towards a method for high vertical resolution measurements of the partial pressure of CO_2 within bulk sea ice, Journal of Glaciology, 58(208), 287*

→→As noted in my first review, I cannot understand, why small isolated brine pockets can be expected to be included in the bulk ice pCO_2 measurement described in the Geilfus et al. 2012 methodological paper. As I understand this method, pCO_2 is measured from equilibration of a standard gas with the ice samples. It is then the connected brine pore space that the gas equilibrates with, not the isolated brine pockets as you appear to claim here. Indeed, that Geilfus et al. 2012 mention several times that the validity of the methods requires a permeable sample, while here something different is proposed, appears confusing.

We already reply to this comment earlier. The brine pCO_2 measurements take into account all the brine that seep into the sackhole. Therefore, if small brine pockets are isolated within the ice structure and don't seep into the sackhole, it will be impossible to sample them. This is one of the disadvantages of the sackhole technique. While, using the bulk ice pCO_2 technique, these small, isolated brine pocket will be in equilibrium with the standard gas and measured. This difference is mentioned in the manuscript (L423) and could explain the differences reported between both methods.
Moreover, the volume of a small brine pocket seeping in the sackhole will be low compared to the volume of brine originating from large and long brine channel network. For instance, if we consider a sackhole of 50 cm, diameter of 14 cm and ice conditions that correspond to a typical brine volume of 8%. The overall volume of brines within the 1st cm of the ice wall (i.e. the brines within the subcentimetric brine channels) is about 190 ml, while within a 50 cm sackhole in a permeable ice (brine volume is about 8%) you can expect to get at least 1 L of brines in the sackhole (this corresponds to 6.5 cm of brines at the bottom of the sackhole). This means that 80 % of the brine are coming from brine channel that are spreading more than 1 cm within the ice, what we call long brine channels.
The size of the ice cube that is used in the Geilfus et al. 2012 method is 5 cm per 5 cm per 5 cm (roughly). So in this method, we are exposing brines channel that are spreading only 2.5 cm in the ice interior. This layer would correspond to an ice wall of 2.5 cm thickness around the sackhole, that will contains max 475 ml of brine in the example above. 475 ml is less than 50% of the overall brine volume at the bottom of the sackhole. Hence, it appears that the bulk CO_2 is prone to an overrepresentation of sub centimeter brine compare to the sackhole method that mostly address brine located in brine channels that are spreading several centimeter within the ice. This difference in the origin of brine analyzed in both method (subcentimeter brine channel in bulk pCO_2 vs long brine channel over 1 cm in the sackhole method explain the observed difference in pCO_2 between both methods.

We added in the text this idea. Now on line 423 we can read: '*In addition, the sackhole technique has a poor resolution as it integrates brine through the all sackhole depth while the resolution of the bulk ice pCO_2 is significantly better as the measurement is performed on a ice sample size of (4 x 4 x 4.5 cm).*'

P 3277, L 26 - P3278, L 11 →... Second, while properly determined bulk ice and brine pCO_2 data should not differ much, they show differences of up to an order of magnitude. I thus feel that a more critical discussion of the bulk ice method is in order. One may imagine the following scenario of an ice sample that is cooled, stored and, prior to pCO_2 measurements, again warmed to its in situ temperature: When the bulk ice is cooled to -23 °C, this will first result in expulsion and loss of brine, which implies a loss of CO_2. Also, during the cooling and internal freezing of the core, CO_2 concentrations are increasing above the atmospheric background, and CO_2 will be lost from the sample, as long the brine network stays interconnected. The situation becomes more complex below -23 °C, when much salt is precipitating in from of hydrohalite, and brine network connectivity will likely drop to very low values. As salt crystal formation induces density changes and brine redistribution, the microstructure after redissolution (prior to pCO_2 measurements) may finally differ from the in situ values (e.g., more salt may reside in isolated inclusions). Finally, the sample may still loose CO_2 from its outer volume (connected to its surface), even once it has become impermeable on larger spatial scales. All these factors are difficult to quantify, but upon returning to in situ temperatures, the pCO_2 will be less than in the field. According to the data the loss of CO_2 appears to be rather large. Moreover, the data in Fig. 6 indicate that the bulk ice pCO_2 stays at a similar level for all stations, while the sackhole brine values vary much more with temperature (as one would expect from concentration/dilution of brine). One might thus also suspect that the bulk ice method has a tendency to produce results that may rather present the cooling procedure (e.g., minimum temperature and cooling rate, sample size and open porosity fraction at its surface) than its intrinsic pCO_2. If this would be the case, these data would be rather difficult to interpret. Regarding the vertical pCO_2 profiles derived, neither the absolute values nor the vertical distribution may have to do with natural conditions. The statement *Therefore, the bulk ice pCO_2 values changes are less variable, reflecting mostly internal melting due to temperature and salinity changes in the ice cover* needs to be tested against this hypothesis, i.e. that the bulk ice pCO_2 measurement procedure is problematic.

→*The reviewer is discussing about the impact of the storage (long term or not) of the ice samples for the bulk ice pCO_2 measurements, how the ice sample can go back to its in situ conditions after storage at low temperature (-30°C).*

- *We recognize that brine may have been lost upon extraction of the core from the ice (Barber and Yackel, 1999). We estimate brine loss to be approximately 5% based on unpublished data collected during the IPY-CFL project in 2008. The measurements were done by replicate samplings (core extraction) from a small area of uniform first- year sea ice (area of about 10 m). Salinity was measured from these cores, and the variability in the measurements was used to estimate the potential brine loss. Thus, the expectation was that the brine volumes would be approximately equal over this small area, and most of the variability would be due to variable brine drainage.*

- *The bulk ice pCO_2 were performed in the center section of the ice core to avoid any contamination during the manipulation but also because if brine were lost during the storage it will be, preferentially, from the extremities of the core, leaving the center undisturbed. Also, the cores are cooled down very quickly to the storage temperature, from the outside to the inside of the core. Therefore, if CO_2 is lost it is from the outer surficial part of the core which: a) was not sampled and b) quickly formed an impermeable outer ring preventing loss from the inner parts.*

→→I expand my comments on expulsion and microstructure changes: First, as noted above, brine expulsion is a consequence of expansion during partial freezing of brine, and this density change will also affect the center of samples. In my experience samples are not sealed upon cooling, yet the necessity to expel brine (due to internal expansion) creates tiny networks through which the brine reaches the sample surface. Second, if the sample would indeed be sealed from the outside during cooling, brine expulsion being absent, I would expect that for very rapid cooling the high internal pressure leads to cracking (such cracks may be tiny, yet they could render the sample artificially permeable, influencing the pCO_2 measurements). It is further important to note that, when the ice is warmed again, internal melting of ice leads to contraction (brine being more dense then ice), and a certain fraction of the pores will not be re- filled with brine, yet with air/gas. Note also that, when the ice is warmed again in the lab, there is no hydrostatic pressure from the underlying water that forces the brine back into the channels. A complete restoring of in situ ice conditions during cooling-warming of samples in the lab is thus very unlikely. For the bulk ice pCO_2 measurements this may have several consequences: (i) the sample contains considerable amounts of air, influencing the methods accuracy as discussed by Geilfus et al. 2012 (e.g. the concentration of standard gas, that is reported in Geilfus et al., 2012, yet not in the present figure 10); (ii) interdispersed air and brine pockets may now fill the original channels, affecting permeability/fluxes; (iii) the permeable fraction of the pore space may have changed; (iv) as long the opposite is not proven, there may be have been gas fluxes through tiny networks during cooling and storage: this may not be falsified by comparing measurements after for example 1 and 4 years, as these fluxes might take place rapidly.

We understand referee's concern, however, we believe that the impacts of those changes linked with the ice core storage at low temperature do not significantly affect our gas composition measurements. As mentioned earlier:
Cooling in these conditions proceeds homogeneously from the outside to the inside of the core, therefore insulating the inner parts of the brine system from the outer atmosphere as impurities (including gases) are progressively enriched in the remaining part of the brine system. Phase changes might result in some internal fracturing of the sample but this does not result in any visible weakening of the outer part of the cores that might have been responsible for partial loss of the gas content.
Stefels et al. (2012, Marine Chemistry, 128-129, 34-43) compared sea ice measurements (DMS+DMSP) directly performed "on ship" to home-lab measurements performed on twin half-core two years later, and showed no statistically significant difference for this volatile compound.
Finally, in an attempt to detect a potential bacterial activity during long term storage (2-3 years) at low temperature (-25°C) in the dark, we (Becquevort et al., unpublished data) have re-measured the oxygen concentration on twin-core samples from the NBP-1998 Antarctic winter cruise after more than two years, without seeing any significant changes in the concentrations.
Moreover, the concentration of bulk ice pCO_2 reported in this manuscript ranged from 9 to 193μatm. If we had cracks and/or infiltration of atmospheric air (atmospheric concentration ≈ 400μatm) into our samples, it should promote an increase of the bulk ice pCO_2 to similar level as the atmospheric concentration. This is not the case and supports the idea that such processes

may not have affected our samples or that these processes did not have a significant impact on our results.

- *The changes of the microstructure of the ice during the storage do not affect the CO_2 contents within the ice or the ability of the method to work properly. And this point has been discussed in the paper presenting the method (Geilfus et al., 2012).*

  →→As noted above, microstructure during cooling-warming as well as storage changes may imply permeability changes, as well as changes in air space, and thus affect the validity of the method.

  See previous comment

- *The precipitation of salt will not affect either the pCO_2 content in the ice because the ice is warmed up to reach its in situ temperature. Therefore any salt precipitating at low temperature will dissolve during the warming period. The main salt affecting the carbonate system will be the precipitation of ikaite. And this precipitation is strongly affected by the temperature of the ice. Indeed, ikaite will precipitate during its storage, but as the ice will warm up to its in situ temperature, these ikaite crystals will dissolve (Rysgaard et al., 2014).*

  →→Precipitation of salts always implies, as already noted, microstructure changes. These come in addition to the irreversible expansion and expulsion upon cooling. Please refer to D.M. Cole, The microstructure of ice and its influence on mechanical properties, Eng. Fract. Mech. 68, 1797-1822, 2001, with Fig. 8 as an example how fundamental microstructure changes after cooling below -23°Ccan be.

  See previous comment about the microstructure changes.
  The precipitation of salt will not affect either the pCO_2 content in the ice because the ice is warmed up to reach its in situ temperature. Therefore any salt precipitating at low temperature will dissolve during the warming period. The main salt affecting the carbonate system will be the precipitation of ikaite. And this precipitation is strongly affected by the temperature of the ice. Indeed, ikaite will precipitate during its storage, but as the ice will warm up to its in situ temperature, these ikaite crystals will dissolve [Rysgaard et al., 2014].

- *If all these processes were that important, they should concur to lower pCO_2s at all levels in all cores. However, 3 out of 5 bulk ice profiles are similar to the brine pCO_2 at Liege, and 2 out of 5 in Brussels and this is coherent with our interpretation with respect to differences in thermal regime due to the insulating snow cover.*

  →→At several places in the text the authors argue that in situ brine pCO_2 and bulk ice CO_2 may relate to different fractions of the brine networks. Now it is argued with the similarity of the results for the 5 out of 10 profiles where moderate (also in these profiles values may differ more then two- fold) agreement is found. This is not very consequent. Also, the fact that half of the profiles do not agree, is in my opinion noteworthy when the validity of a method is concerned.

In our answers, we should have responded to the referee's concerns regarding the validity of the bulk ice pCO_2 measurements. In the manuscript, we discuss the differences between both methods and linked these differences to surface flooding, snow accumulation and that both technique (sackhole vs bulk ice pCO_2) address different parts of the brine network.

P 3278, L 21 - P 3279, L 13 and Fig. 10 →*The bulk ice...Geilfus et al. (2012a).* .... The scale should be changed, as in the present Fig. 10 only little detail can be seen. →*About the scale of the figure 10: having the same scale between 10a and 10b allow the reader to realize that all data from the bulk ice (10a) are in the same range as the brine data (10b). The cloud of data is located at the same position on the X-axis. Which is a point that may be lost if the scale was different.*

→→Different scales could be noted in the Figure/text. What is lost in the present scale of Fig. 10a is the possibility to distinguish the data points.

We changed the figure accordingly.

Next, it is important, in particular due to the uncertainty to what degree the method is valid to obtain bulk ice pCO_2, to mention and discuss possible differences in the sampling, storage and measurement procedures of the different datasets. For example does the Arctic sea ice from Geilfus et al. (2014) appear to have been stored at -20 °C, compared to -30 °C in the present study. While at the lower temperature most salt will precipitate as hydrohalite, before it is redissolved prior to pCO_2 measurements, this is not the case during storage at -20 °C. Salt crystal precipitation/dissolution involves large density changes, and very likely changes in the microstructure that may be fundamental for the validity of the pCO_2 measurements. Please provide a comparison of all the data compared. →*The ice samples from Geilfus et al [2014] come from the melting period in Resolute bay. The salinity range of these samples are from -3 to 0°C. For this range of temperature, a storage at -20°C is low enough to ensure an appropriate storage impeding any gas exchange during the storage. About the precipitation of salt, yes, the storage will promote a precipitation of salt. However, as explain earlier, during the equilibrium at the in situ temperature, the salts will dissolve again. This process is also explained in Rysgaard et al. [2014]. Therefore this will not affect our measurement and we don't need to provide a comparison.*

→→As mentioned above, fundamental microstructural differences between sea ice stored at temperatures below -23°C and above have been documented (noted example in Cole, 2001). Therefore the different storage conditions should be mentioned for the reader. By the way, I am not aware of a study in the literature that clearly demonstrates time scales and degree of re-dissolution of different salt precipitates in warming sea ice. There is certainly potential for precipitation-dissolution hysteresis, in particular when air and brine are interdispersed, and on timescales of hours.

In the manuscript of Rysgaard et al [2014], as suggested in the response and cited in our manuscript, the authors explain the ikaite dynamics within sea ice associated with the changes in the ice temperatures. The impacts of potential crack and/or air infiltration has already been discussed in previous comments.

P 3279, L 9-10 →*The brine volume combines the effect of the high salinity and low temperature at the same time.* - The brine volume is indeed the property that (in thermodynamic equilibrium) follows from bulk ice salinity and temperature, yet it does not combine these effects. Rather it is an average property of the pore space. It is the brine salinity (or, due to their relationship, indirectly the ice temperature) that correlates with pCO_2 in a clear physical manner - via brine dilution and concentration (as apparent from the in situ brine observations, Fig. 10b). The effect of brine volume or porosity is more complex, as it affects the permeability of ice in situ (and thus CO_2 transport processes), as well as structural effects that influence the validity of the method to obtain bulk ice CO_2. Please distinguish this more clearly.

Due to the noted uncertainties with the procedures/methods to obtain bulk ice pCO_2, I rate the sackhole brine results in Fig. 10b as physically most meaningful. As mentioned for bulk pCO_2 I recommend to show in situ brine pCO_2 versus brine salinity, and perform a statistical test to determine the significance level of an eventual difference between Antarctic and Arctic brines. Also here a comparison of the sackhole sampling procedures (sampling times, depth) would be helpful to evaluate if difference in the measurement protocols might have influenced the results.
→We are not in a position to qualitatively rate our results on a scale of most meaningful to least meaningful. We have been very careful to keep any and all opinion out of our work and we would like it to remain as objective as possible. Second, any statistical testing of the difference between Antarctic and Arctic brine would be invalidated by the large differences in the ice physical factors.

→→A statistical test should be provided when comparing two datasets in a figure, in particular as the authors write (Line 447): "Although based on limited data, Antarctic sea ice may have lower pCO_2 values than Arctic sea ice at the same ice temperature (Figure 10), although differences in the sea ice texture and dynamical forcing between the two poles are important and may have substantial effects on permeability (and therefore fluxes) and should be further investigated".

We performed a 1-way ANOVA test, $F_{1,210}=30.73$, $\{p<0.001\}$, showing that Antarctic sea ice is significantly lower than Arctic sea ice. We added this information in the manuscript L456.

P 3279, L 14-28 →*During this study, ...atmosphere....* Second, removing snow lowers the surface temperature, increases the brine salinity, and thus changes the pCO_2 gradient between brine and atmosphere - an important parameter that determines the surface fluxes.

→*We don't think that, within the 5 min used to do the flux measurements, the change of temperature will be so significant that the CO_2 flux will change that much. It sounds like the reviewer has access to similar measurements, so he should know that the surface temperature within the chamber does not change appreciably in 5 minutes.*

→→The authors do not provide the time from removing the snow to start of measurements. However, ice surface temperatures may change rather rapidly (e.g., according to z 2 ϰt with ice thermal diffusivity ϰ, the upper millimetre may response within less than half a minute). That removal of snow imposes a disequilibrium may also be critical.

We carried out the chamber measurement over bare ice right after the snow removal. In Rysgaard et al (2014), we removed the snow cover from the surface of the ice while measuring the ice temperature (every 2m to 50cm depth) and indeed we notice a significant changes in the ice temperature. However this changes is about 2°C within a couple of hours. We could estimate the change of pCO_2 due to a decrease of the ice temperature of 2°C. Assuming a mean characteristic for the brine of TA=3000 and TCO_2=2500 µmol kg-1 at -2°C and a salinity of 40. The calculated pCO_2 will be at 146.94 µatm. If we decrease the temperature of 2°C, the pCO_2 will be at 133 µatm. This change of 13 µatm is not critical in term of impact on the CO_2 fluxes. In addition, in our case we took the measurement directly after the snow removal within less than 5 min.