Interactive comment on “Impact of snow cover on CO_2 dynamics in Antarctic pack ice” by N.-X. Geilfus et al.

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I was asked by the Editor to provide comments on the discussion paper "Impact of snow cover on CO2 dynamics in Antarctic pack ice". The paper presents various measurements conducted during the Sea Ice Mass Balance in Antarctic cruise (SIMBA), and discusses various aspects and implications of the data. Given the unknown importance and poor understanding of CO2 dynamics in sea ice, as well as the dearth of measurements, I believe the work is definitely worthy of being published in The Cryosphere. I hope that the Authors, Editor, and Referees work together to enable this.

These are my comments on the manuscript in no specific order. I hope they are useful to the authors.

1. The abstract and introduction do little to inform the reader what separates this study from previous studies. Only on line 3-7 of page 3266 is it mentioned that Nomura et al found that a snow cover thicker than 9 cm could prevent CO2 exchange, but no word is lost on how the Nomura study differs from this one. If the reader is not already intimately acquainted with the research topic he/she will not know if similar studies have been done before, if the measurements the authors show are a standard approach, or if certain aspects of the methodology are new and unique.

→*We have added a short paragraph in the introduction, associated with our objectives, to lead the reader on the way as to why this study is important:*

“The role of ice-covered oceans in the CO_2 balance has been largely ignored because continuous sea ice cover is assumed to impede gaseous exchange with the atmosphere. However, recent studies show that sea ice may mediate the air to sea CO_2 transfer. Understanding of the seasonal and geographical conditions of the inorganic carbon dynamics related with sea ice is limited. The main goal of this study is to add to the still limited database on inorganic carbon dynamic in ice-covered seas.”

We provide, in the introduction, several references on key publications to help novice reader to get more information on the topic.
Nomura et al. [2010] measured air-ice CO_2 fluxes and observed that in snow-covered ice, CO_2 fluxes are lower than expected by comparison to other studies. Our present study is by far more comprehensive and robust.

1. *We carried out measurement in parallel in two nearby spots with different snow load*
2. *We described in detail the carbonate system within the ice, and sea ice physics. We observed that snow does not only modulate the flux of CO_2 but also affect the overall inorganic carbon dynamics within sea ice by not only impeding mass transfer, but also energy transfers. Indeed the main driver of air-ice CO_2 fluxes are the gradient of pCO_2 across the air-ice interface, snow being only a modulating factor. By affecting energy transfer, snow does indeed affect the whole carbonate system dynamics and therefore*

the air-ice pCO_2 gradient. As a result, by affecting sea ice physical properties, the effect of snow is not only a transient control of the magnitude of the flux. Snow load also controls the way of the fluxes (sink vs source) and the budget of fluxes integrated over large time scale.

2. The title is misleading. The paper presented is not a general study of the impact of snow cover on CO2 dynamics in Antarctic pack ice. The paper presents and analyses the SIMBA measurements, of which one aspect is snow cover. A quick glance at the conclusions shows that snow is not the true focus of this paper, and it is not clear why a general study of snow impacts is based solely on one field campaign. A more honest title would be: "A field study of CO2 dynamics in Antarctic pack ice"

→*We agree. According to all the comments received, undoubtedly the title of this manuscript was poorly chosen. We suggest to change it to: 'Sea ice pCO_2 dynamics and air-ice CO_2 fluxes during the SIMBA Experiment – Bellingshausen Sea, Antarctica'*

3. The conclusions do a poor job of communicating what the authors have learned from the measurements. It has been known for many decades that snow depth has a large effect on ice temperature, it is known that flooding brings seawater to the ice cover, it is known that flooding is caused by snow loading, it is known that temperature (and accordingly brine salinity) affect sea-ice chemistry. What was not known?

→*The main messages found in the conclusion are:*

- *Spring sea ice pCO_2 swiftly responds to short term meteorological events, with redistribution processes linked to the brine network dynamics,*
- *The spring sea ice pCO_2 is undersaturated and largely controlled by the brine dilution upon warming, although a potential impact of primary production could contribute to the undersaturation,*
- *Despite episodic flooding events brining supersaturated seawater in the brine network, the spring sea ice remains undersaturated and a sink for atmospheric CO_2,*
- *Both techniques, measuring the pCO_2 within sea ice and brine, address different parts of the brine network*

We highlighted these finding in the revised manuscript.

4. Although the authors have made a solid attempt to detail the experimental methods and resulting uncertainty, it would be very helpful to the reader if the data uncertainty were directly marked in the plots. This is what the reader wants to know, how does the uncertainty relate to the measurements. Few readers will be interested in the instrumental precision by itself, and why should the readers have to deduce the impact of measurement uncertainty by themselves? Especially for the pCO2 values this would be of great importance.

→*The precision of the bulk ice pCO_2 is about 5%, see the paper presenting the method, Geilfus et al [2012]. The bulk ice pCO_2 in our study ranged from 9 to 193 µatm, precision will range from 0.45 to 9.65 ppm. The in situ brine pCO_2 ranged from 63 to 392 µatm and the precision of the Licor is 2-3 ppm. Illustrate these range in the plot will be irrelevant due to the large range of concentration reported. Regarding the precision for TA (± 4 µmol kg^{-1}) and pH (± 0.01)*

these margins of error will be, as for the pCO_2, unreadable in the different plots.

5. The paper should put more emphasis on the sampling issue of sea-ice cores due to the strong spatial variability of sea ice. Although it is briefly mentioned and stated that the core sites were chosen to be as heterogeneous as possible, as only one core was taken every five days it is not possible to fully attribute differences between two following cores to changes over time. It is very likely that much of the difference is due to random spatial heterogeneity. Gough et al performed a thorough statistical analysis of the sampling issue in Antarctic ice cores in their 2012 paper "Sea ice salinity and structure: A winter time series of salinity and its distribution".

→ *We are well aware of the potential spatial variability of the sea ice cover and of its impact on the biogeochemistry, especially when the biological patchiness is involved. A thorough statistical study of the sampling issue (such as the one proposed by Gough et al.) is only thinkable for simple and quickly measured variables such as temperature or salinity. With the complexity of biogeochemical measurements it is simply not an option. We can then show two types of behaviour: a) give up and decide it is simply impossible to characterize the temporal evolution and processes driving the evolution of sea ice biogeochemistry parameters (and let the models drift on purely theoretical concepts) or b) attempt to minimize the spatial variability the best we can by collecting ice samples in a small area of 5m by 5m, on a homogeneous sea ice surface, as we have done and explained in the methods section:*
"Each site was 100x60 m and subdivided into small work sub-areas approximately 5m x 5m. The 25 m^2 sub-areas were located adjacent to each other to minimize spatial variability [Lewis et al., 2011]."
As mentioned by the reviewer, it is always possible that variability will interfere with our results. Therefore, we add in the text:
"It is also possible that some natural variability in the sea ice at the two sampling locations existed, though given the textural evidence of dynamic processes at Liège [Lewis et al., 2011] one might intuit that variation within salinity especially would be greater at Liège but that was not observed."
There are however some indications that what we see is at least coherent with a temporal evolution, such as the thermal response of the ice cover to the cyclonic events. Also, it is interesting to see that, at Liège Station, where the ice texture is extremely variable from one sampling place to the next, the ice thickness is not significantly different and the temperature and salinity profiles are extremely smooth despite the many textural boundaries. This is something that has been noted by previous workers in several occasions. Finally, in this case, even if we look at the Chl a profiles at Liège Station, we see that despite small scale differences it shows similar profile trends between sampling events, and also somehow respond to the brine transport events (e.g. from 3 to 18 and from 18 to 23 October)… to us, these are all hints that we are not seeing overwhelming spatial variability.