

Interactive comment on “Impact of snow cover on CO₂ 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.

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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,
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"
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?
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.
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

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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".

Interactive comment on The Cryosphere Discuss., 8, 3263, 2014.

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