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Interactive Comment

Interactive comment on "Parameterization of atmosphere–surface exchange of CO_2 over sea ice" *by* L. L. Sørensen et al.

Anonymous Referee #2

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General: The issue of the oceanic role in the atmospheric CO2 budget is a very important one and not nearly as thoroughly studied as its relevance deserves, or as the terrestrial ecosystems. One of the main reasons for this is, that CO2 exchange rates over most oceanic surfaces are small in comparison with terrestrial surfaces at similar latitudes, and that the infrastructure in most places makes this type of studies very difficult.

The present manuscript makes an attempt to increase our understanding of the functioning of the arctic sea and sea ice, both though modeling and measurements of the CO2 exchange over an ice covered fiord in southern Greenland. Though such efforts are certainly needed and could potentially add to our understanding of the Global carbon budget, I do not find that the present manuscript adds much in this context.

The process modeling preformed is basic and sound , but cannot be confirmed through the exchange measurements carried out. The most likely reason for that is, that the fluxes over the ice covered fiord are simply below the detection limit of the instrumentation used, and that most of the displayed "measurements" for that reason represent the electrical noise of the instrumentation, rather than credible exchange rates of CO2 between the ice surface and atmosphere. ' Though the attempt to disclose mechanisms and rates of CO2 exchange between the sea, or in this case sea ice, and the atmosphere can be appreciated, I do not think that the manuscript adds to this, either with respect to modeling or measurement methodology. I am sure that the expedition on the sea ice has been both difficult and costly, but does in my opinion not pay off because the authors, simply do not have the instrumentation needed to document what they are after.

Despite that the authors must be aware that the fluxes that they have measured, are small and on the limit to, what would be possible, words like "detection limit" are not even mentioned in the ms and the measurements techniques are only very briefly described. All together this leaves me with the impression that little faith can be put in the observed exchanges rates, which is why also the modeling part of the manuscript seems unfounded.

Specific:

P3901 L20: was any melting or formation of sea ice observed in this study?

P3902 L9: Previous to 1976?

P3902 L16: TCO2? Please explain

P3903 L4-7: This seems to be the main hypothesis of the ms, and following the discussion above I think the focus should be changed, because it can in my opinion not be either confirmed or rejected - focus should be changes considerably. Section 2 Theory: could be shortened considerable in a rewrite of the ms, because most of the resistance and flux theory can be found elsewhere in literature. Focus should be put in deviations for the traditional use of resistance for terrestrial surfaces e.g. in determination of CO2 concentration gradient between ice and Atm.

P3909 L1-20: I find it highly surprising, that the flux measurement methodology is only very briefly described, considering the challenges that the authors are facing. Not even the origin of the instruments is disclosed, but I assume that what has been used in the study are in the good quality, but commercially available range of eddy instrumentation. If that is true, the most commonly reported detection limits are roughly 0.1 micromol CO2 m-2 s-1 or 4.4 microgram, which is then subject to a certain noise level. This would make most of the points in Fig, 3 fall under the detection limit which can explain why none of the fluctuation can be explained through pCO2 or any of the other displayed parameters.

P3910 L11: I am not sure that I understand the meaning of “Equality” when used in this context. Please explain.

P3911 L22: two refs not in references list.

P3912 L15 and Fig. 5: I fail to understand how resistances can become negative, please explain.

P3913 L5-11: I’ll not argue with this hypothesis, but it is difficult to confirm by looking at the figures and the most pronounced uptakes are not associated with the most dramatic drops in temp.

3914 L6: Rc seems primarily negative in the figure, which I assume is a result of opposite directed flux and CO2 gradient?

3914 L11-15: statements here seems to contradict, also to fig.?

3915 L3-6: Rc and Temp does not seem to be entirely independent parameters to me.

P3915 L9: Is this regarded “ice melt season” ?

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3915 L20-21: Please check consistency in numbers with P3910.

P3915 L20-30 + Fig 7: there is no visible correspondence between measured and modeled fluxes . If there is please show more clearly.

Fig 4. Are any of the pCO_2 values given here significantly different?

Interactive comment on The Cryosphere Discuss., 7, 3899, 2013.