

Interactive comment on “Winter observations of CO₂ exchange between sea-ice and the atmosphere in a coastal fjord environment” by J. Sievers et al.

Anonymous Referee #1

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The manuscript by Sievers et al. describes three separate eddy covariance datasets collected over sea ice under winter conditions during a campaign in early 2012. The paper tries to explain the observed CO₂ fluxes by comparing them to ancillary parameters such as heat fluxes, temperature etc. This paper can be an import addition to the limited data that's available from sea ice-covered areas, an environment in which it's challenging to perform fieldwork. However, I'm concerned that the current version does not convince strongly enough. A major problem lies with the instrumentation: under the extremely cold conditions that were prevalent during the measurement campaign, a sensor such as the Licor-7500 (used at the POLYI and DNB sites) has serious problems with correctly quantifying vertical fluxes. This is due to heating

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of the sensor itself from the internal electronics and surface heating from incoming radiation. This can lead to apparent downward fluxes when none are present, and these are correlated to sensible heat flux and incoming radiation (Ono et al. 2007). This is exactly what is shown for the sites where the Li-7500 was used, despite hugely different conditions of the ice. The suspiciously strong correlation with sensible heat (and also net radiation) is only visible for the sites with that sensor, and absent from the ICEI site where the closed-path Li-7200 was used. My disconcerted feeling that there is a serious problem with the data collected at the POLYI and DNB sites is further fed by the huge discrepancy at the POLYI site between the chamber flux measurements (which are near zero) and the eddy covariance (which shows a large uptake). At the same time, there is good agreement between the chamber flux measurements and the eddy covariance at the ICEI site where the closed-path sensor is used.

Despite these obvious shortcomings, this problem is not addressed in the text. It is only mentioned in reference to other studies and as a general problem with open-path sensors, without acknowledging that the sensor used has this problem. Also, no reference to Burba et al. (2008) is given, where a correction for the problem is proposed. I suggest, therefore, a major revision where the authors revisit their data and consider what the effect has been of the heating problem of the Li-7500 on their data, include a thorough discussion on the subject, and how this may affect their conclusions. Since such a discussion is currently lacking, it's very difficult to critically evaluate the conclusions currently presented in the manuscript. This review is therefore incomplete, but I would gladly review a revised version of the manuscript once above changes have been included. If correcting for the heating problem is not possible or problematic, I suggest that the authors focus more of their argument on the data collected with the Li-7200.

Some other, smaller remarks I have so far are listed down below. Remarks on the discussion and the conclusion section are currently withheld however, since it's not

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possible to properly evaluate those without addressing the Burba-heating issue.

- Line 4,5, page 47: 'fluxes were found to increase'. In which direction? Was there more uptake or more release? Unclear.
- Line 7, page 50: how was the frosting on the sensors monitored? Was there a daily visible inspection or is it inferred from the data?
- Line 16-19, page 50: please rephrase this sentence. I know what you're trying to say but it's hard to read.
- Line 20-25, page 50: were the conditions in the valley the same as at the fjord? No differences in cloud cover perhaps?
- Equation 1, page 51: Why are there other symbols used here than in Else et al. and Persson et al? It makes more sense to use the same conventions. Also, it's confusing to use G in this equation for the upward conductive heat as in these equations that's normally reserved for ground-heat flux, which is not the same.
- Line 20, page 51: please place this equation separate. Also, it is not clearly specified how R^T is derived or measured.
- Line 5, page 57 to line 9, page 58: this whole paragraph belongs in the theory and method section, not in the results.
- Line 6, page 61: line 6, page 61: I think Figure 5 clearly shows that the results are not the same at all sites. Figure 7 can easily be extended to show these patterns for the other sites, too.

References:

Burba, G. G., McDermitt, D. K., Grelle, A., Anderson, D. J. and Xu, L.: Addressing the influence of instrument surface heat exchange on the measurements of CO₂ flux from open-path gas analyzers, *Global Change Biol.*, 14(8), 1854–1876, doi:10.1111/j.1365-2486.2008.01606.x, 2008.

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Ono, K., Miyata, A. and Yamada, T.: Apparent downward CO₂ flux observed with open-path eddy covariance over a non-vegetated surface, *Theor. Appl. Climatol.*, 92(3-4), 195–208, doi:10.1007/s00704-007-0323-3, 2007.

Interactive comment on *The Cryosphere Discuss.*, 9, 45, 2015.

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