

Response to Reviewer 2

Blue text:

Comment: Referee comment. Page and line numbers relate to the original submission.

Black text:

Response: Our comments to the reviewers and the public

Action: What we have done to change this in the text of the article. Page and line numbers relate to the edited manuscript.

Interactive Discussion

Comment: I checked reviewer responses "ver.1 "File Upload (23 Jul 2018) by Brian Butterworth". Authors respond all my review comments and I agree with their all comments.

Response: We would like to thank the reviewer for the thoughtful comments. As a note to readers, the reviewer submitted comments in the initial submission's access review stage. Therefore, they were not posted in the interactive discussion online. We have posted those comments and our responses below.

Access Review

General Comment: This manuscript presents the evaluation of the difference between enclosure and eddy covariance methods for CO₂ flux measurements over sea ice. This manuscript provides new aspect to understanding the sea ice contribution to the CO₂ budget to the atmosphere. On the other hand, this paper denied the data collected so far by eddy covariance in sea ice area. Can we use previous data or do you have idea to correct the vapor effect on CO₂ concentration measurement for previous data (then CO₂ flux data)? It is most important. You indicated that eddy covariance CO₂ flux became similar to that for enclosure values and you said it is good. It seems that enclosure values are real (true) CO₂ flux values. My impression is that enclosure method evaluated the underestimate CO₂ flux over sea ice because this method does not consider the wind effect. Sea ice environment is basically high wind and there is no barrier to reduce the wind (not likely tree in the forest).

Response: Without a simultaneous *undried*, closed-path LI7200 we do not have an adequate method for quantifying the water vapor impact to correct previous studies.

Because we did not collect enclosure measurements we are not claiming that our EC measurements are an exact match to enclosures, or that enclosures represent real/true values. This data simply highlights that using the best practices of EC (dried, closed-path) reduces sea ice flux measurements several orders of magnitude from suspiciously high open-path EC measurements. This moves sea ice EC measurements into a range that is more theoretically plausible. Future studies comparing the two methods simultaneously may provide insight into the degree to which enclosure method is influenced by its previously-documented shortcomings.

Action: We felt that this comment showed we did not adequately emphasize the advantages of EC over enclosures.

Therefore, we changed Page 2, Line 22:

“Additionally, the measurements are spatially limited to the region enclosed by the chamber, making it challenging to investigate ecosystem-level questions.”

To read:

“Additionally, the measurements are spatially and temporally limited. Measurements are confined to the region enclosed by the chamber (cm scale), making it challenging to accurately measure fluxes over whole ecosystems (m to km scale), which typically contain heterogeneity on scales larger than the footprint of the chamber. Additionally, long-term measurements are not feasible for enclosures due to the fact that they alter the underlying environment and the degree of manual intervention they require.”

Comment: Page 1, lines 1-2, Title: You should appeal your technical advancement with respect to the traditional method for CO₂ flux over sea ice. For example, “The dried, closed-path EC method for CO₂ flux measurement over sea ice”.

Response: We agree that this would improve the title.

Action: Title now reads:

Dried, closed-path eddy covariance method for measuring carbon dioxide flux over sea ice

Comment: Page 2, Line 3: Takahashi et al (2012) is focused on the Southern Ocean. Therefore, 2009 may better.

Response: Okay.

Action: Changed in line citation (Page 2, Line 3). Updated references to reflect the change.

Comment: Page 2, Line 7: “CO₂ fluxes over sea ice are small”. As compared to what? If we check ice eddy covariance data, it is very high magnitude.

Response: CO₂ fluxes over sea ice are small compared to terrestrial fluxes and often small (though not always) compared to open ocean fluxes. The fact that sea ice EC has previously been measured at high magnitudes is addressed in this paper. We believe our position is clear that these measurements are not believable based on the literature over the past 5-10 years.

Action: On Page 2, Line 8 we changed “Generally, CO₂ fluxes over sea ice are small, but there are...” to read “Compared to terrestrial environments CO₂ fluxes over sea ice are small. However, there are...”.

Comment: Page 2, Lines 22-23: “Ecosystem level question” I cannot understand. Enclosure method can also detect the ecosystem response.

Response: We refer reviewer to the following paragraph in Miller et al. (2015):
“The greatest advantages and disadvantages to using enclosure methods are both due to spatial variability. Chamber enclosures only integrate the signal from the area they cover (generally, a few hundred cm²); if the exchange is governed by factors that vary on larger horizontal scales (*i.e.*, the thickness and wetness of the snow cover, melt ponds, leads, under-ice hydrology, *etc.*), a prohibitive number of individual chamber measurements over a large area may be required to estimate the flux accurately (section 2.1). On the other hand, the method is ideal for studying specific, small-scale processes influencing variations in the flux (*i.e.*, brine channel distributions, ice algae respiration, *etc.*), and enclosure methods are the only technique available to determine fluxes on the same scale as most sea-ice biogeochemical measurements. In contrast, the micrometeorological techniques (sections 5.1.2–5.1.6) cover areas several orders of magnitude larger than chambers, integrating fluxes from different ice types and any open water in the footprint; micrometeorological results can, therefore, be difficult to interpret over heterogeneous surfaces.”

Action: This comment was addressed with the action taken to the first comment (see above).

Additionally, on Page 15, Line 14 we changed “ecosystem-level processes” to “ecosystem-scale processes.”

Comment: Page 2, Line 31: Why is it good agreement on terrestrial environment? No water vapor? We expected that in the polar area, water vapor amount is basically small.

Response: We can't say for sure why the terrestrial studies showed better agreement. But we believe that it stems from the larger terrestrial CO₂ fluxes resulting in a reduced impact (proportionally) of bias from interfering variables (e.g., water vapor cross talk) in the EC measurement.

The interference of H₂O on the CO₂ concentration (in LI7200) appears to be greatest when the CO₂ fluxes are small. Over sea ice the CO₂ fluxes are very small compared to terrestrial

environments. Therefore, even small water vapor fluxes (when not preconditioned out of the sample air) cause a relatively large impact on the CO₂ measurement.

Action: Because we can't elaborate on the quality of the terrestrial flux measurements, commenting on them here appears to be a distraction. On Page 3, Line 1 we changed: "While the enclosure and EC methods have shown good agreement in terrestrial environments (Laine et al., 2006; Wang et al., 2013), they have not shown good agreement over sea ice..." to "The enclosure and EC methods have not shown good agreement over sea ice..."

We removed the associated citations from the references.

Comment: Page 4, Lines 26-: Why you did not prepare both dry/not dry LI-7200. In order to check the effect of dry or not dry air, same analyzer should use.

Response: We agree that this proposed setup could have yielded some additional information. We did not do it due to cost. Funds were only available to purchase one LI7200.

Action: No action taken.

Comment: Page 9, Lines 18-19: CO₂ mixing ratio should indicated as ppmv?

Response: Yes, this is umol/mol.

Action: We have changed it to 'ppm' in the following places (Page 9, Lines 22-23 and Figure 6 caption). Figure 6 was updated so the ylabel for subplot g now reads 'ppm'.

Comment: Page 12, Line 1, 4.4 gas transfer velocity: You compared for open ocean data. I am not sure this is suitable because this paper focused on the CO₂ flux measurement over sea ice. Therefore, you should do this for ice pCO₂. For open water condition, we do not need discussion.

Response: We believe this is an important section. The premise of this paper is that we don't really know what fluxes over sea ice should be, since enclosures have problems, and all previous EC attempts have had problems. The only way to check if our system is working is to compare it to something we do know, and that's the open ocean.

Action: No action taken.

Comment: Page 13, Lines 16-24: I cannot understand. You did not use zero air Nafion in this study. And, I think that H₂O values become same for sometimes. Without a dry air counter flow, how to drying? Therefore, as indicated above, you need to check dry/not dry LI-7200 in order to clear up the effect of dry/not dry air to flux. Or you should remove/attach the desiccant for LI-

7200 to check the water vapor effect.

Response: Just to clarify, when the desiccant is active it fully eliminates water vapor going to the Nafion *counterflow*. But, the Nafion does not completely remove water vapor from the *sample* air. It reduces the magnitude of water vapor in the sample air and (this is the critical point) reduces fluctuations in water vapor. The important part with respect to the Nafion providing benefits even when the desiccant's capacity was exhausted, is that the Nafion still provides a smoothing effect on the water vapor fluctuations, even when not providing any reduction in the mean concentration of water vapor in the sample air. In this scenario, a spike of moist air will still exchange H₂O with a counterflow that is at mean ambient humidity. Because the EC calculation relies on the fluctuations and not the means this smoothing of the water vapor fluctuations still provides benefits. Tubing itself (with no Nafion) does provide some smoothing to water vapor fluctuations as water vapor is 'stickier' than CO₂. But previous results from an undried LI7200 (Butterworth and Miller 2016b) showed this did not fully remove the influence of water vapor on the CO₂ flux, instead showing up as spurious low frequency contribution to the flux, which was visible in the flux cospectra. In this study, when the desiccant was exhausted, we did not see contribution in the cospectra suggestive of water vapor interference, suggesting that it is the Nafion that is helping.

A simultaneous measurement of undried LI7200 would likely provide some additional information and will be considered in the future if funding allows.

Action: On Page 13, Line 29 added:

This makes sense because spikes of moister or drier air will still exchange H₂O with a counterflow that is at mean ambient humidity. Without a parallel, undried LI7200 it impossible to quantify how much of the smoothing is from the Nafion compared to the natural 'stickiness' of H₂O on tube walls. However, the impact of H₂O smoothing from tube walls alone was tested in Butterworth and Miller (2016b) with an undried LI7200. It was found that tubing did not fully remove the influence of water vapor on the CO₂ flux, and instead showed up as spurious low frequency contribution to the flux, which was visible in the flux cospectra. In this study, when the desiccant was exhausted, the cospectra did not indicate interference from water vapor, suggesting that that the Nafion played a critical role.

On Page 14, Line 4 changed "(particularly ones with power constraints)" to "(i.e., a system could be designed that uses a Nafion and counterflow, but without a dry air source)".

Comment: Page 13, Line 25: For future work, do you have idea to correct the vapor effect on CO₂ concentration measurement for previous EC data (then CO₂ flux data)? It is most important.

Response: Previous studies have compared dried and undried LI7200 in parallel (Landwehr et al. 2014, Blomquist et al. 2014, Butterworth and Miller 2016b). Quantifying a relationship for correcting CO₂ flux bias from water vapor in undried, closed-path systems does not appear to be straightforward due to large degree of scatter and inconsistent bias across different studies and across different IRGAs in the same study. Such work would constitute a full paper in and of itself and cannot be addressed here. It is something we plan to investigate in the future.