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Interactive comment on "A new procedure for processing eddy-covariance data to better quantify atmosphere-aquatic ecosystem CO_2 exchanges" *by* Tatsuki Tokoro and Tomohiro Kuwae

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Thank you for important comments. The following is the responses to your comments. We attached the replaced and added figures. Please check the detail captions and notes at the end of this document.

Comment #1 However, the challenge here is that there isn't a strong scientific justification for these. Caution has to be made in any filtering to verify that the filtering is "unbiased". For example, the Mauder/Foken criteria are based on known expectations

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of spectral response of EC sensors and properties of atmospheric turbulence. The ones proposed here are somewhat arbitrary. Thus, I would hope any revision would try to justify theoretically based on EC assumptions and also to look more systematically at the impact on fluxes. for example, diurnal cycles, cumulative NEE, nighttime and daytime averages. it would also be convincing if known relationships (for example relationship of delta pCO2 to NEE) become stronger (i.e., show Figure 10 with both filtered and unfiltered approach)

Reply #1 We agree that the analysis described in this comment would improve our study. We added figures that show the differences between the raw and filtered diurnal flux cycle data (Fig. xx1), cumulative NEE (Fig. xx2), and delta pCO2-EC flux (Fig. 9). All of these figures show that the filtering created no biases. The average shift in daytime flux was positive after filtering, but this shift was due to high-pass filtering and not to filtering by normalized standard deviations.

Changes #1 There are new versions of Figures 9, xx1, and xx2 (see attached file). We will also replace the description of the bulk formula flux calculation.

Comment #2 Further, the description of each step is a bit confusing. For example, it's entirely unclear to me if these filters are applied to the half-hourly fluxes or the 10 Hz data precomputation. I'm surprised RSSI is not used by EddyPro as criteria. The statistical moment tests are most confusing, how are the criteria determined. Is it just 1 standard deviation or something else? If all these were applied to 10 Hz data, were fluxes then recomputed in EddyPro or elsewhere? One thing that might help would be to provide sample code to apply the three tests as a supplement. If the goal is to encourage more lake EC users to use this approach (and cite this paper), sample code would go a long way toward that.

Reply #2 The filtering was applied to 30-min data; it was not applied to 10-Hz data for 30 minutes. We do not know the reason why the EddyPro does not include RSSI filtering. The calculations were so simple that they could be done with an Excel spreadsheet.

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Hence there is no programming code associated with the calculations.

Change #2 We will add a detailed explanation of the filtering in the Methods section.

Comment #3 I wonder how much some of these spikes would be alleviated by actually measuring storage flux. Storage flux is briefly mentioned at the end, but it might be worth discussing in more detail.

Reply #3 Most of the spikes in the erroneous data appeared at 10 Hz (for 0.1 s). We do not think that such short fluctuations can be explained by the storage flux (the concentration changes in the atmospheric boundary layer on the water surface).

Change #3 No change.

Comment #4 Line 106 I disagree that the main issue of EC open-path flux is cross-spectral sensitivity. This might be true over the ocean EC on ships, but modern IR sensors are pretty good at measuring CO2 and H2O fluctuations and I don't think fixed sensors over lakes suffer from the same degradation of CO2 in high humidity environments. Do you mean the WPL density correction? I am not familiar with PKT correction, and while mentioned here, it is not brought up again later in the paper. Why was it not applied here or compared to the PP2 approach? If this is true, why not just recommend closed-path sensors for all lakes?

Reply #4 We described the cross-sensitivity and PKT (not WPL) correction only in the context of their being among the eddy covariance problems. In the case of the closed-path type, other problems (i.e., attenuation of CO2 fluctuations due to pumping) have been suggested, and their resolution would require more electrical power and a more complex system. We decided that the open-path type was better for coastal measurements.

Change #4 We will remove all sentences about the cross-sensitivity and PKT corrections.

Comment #5 Line 287 Please be aware that there is high uncertainty in derivation of

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pCO2 from carbonate chemistry in lakes (see Abril et al 2015 and Golub et al., 2017).

Reply #5 We agree about the uncertainty of coastal pCO2 in limnetic systems.

Change #5 We will add a citation in the comment.

Comment #6 Line 354 I'm a bit skeptical of the "lowest reported" EC flux of -1.08 umol/m2/s. That doesn't seem that low. Maybe ok for ocean, but for lakes, with vegetation or eutrophic systems, fluxes could be ten times that. See literature on EC fluxes from recent papers by Jiquan Chen, Gil Bohrer, Timo Vesala, Gesa Weyhenmeyer or others who have published EC lake data.

Replay #6 This is the lowest flux measured by any other method (bulk formula or chamber method), not by the eddy covariance method. We suggested that the average of the filtered eddy covariance data in this study was consistent with the fluxes measured by conventional methods.

Change #6 No change.

Comment #7 Line 369 "Seem to agree well" doesn't seem like a strong enough claim. How about something more quantitative in terms of reduction of bias, increase in correlation, and other goodness of fit tests (maybe compare distributions?)?

Reply #7 As mentioned in reply #2, we decided to remove the comparison between the bulk formula and eddy covariance fluxes because the bulk formula flux has several uncertainties in the case of the CO2 flux measurement in coastal area. Instead, we have made a comparison with the delta pCO2, which cannot be compared directly with the eddy covariance flux but is the main parameter that regulates the lagoon flux. The comparison shows that the relationship (P-value) becomes significant after filtering.

Change #7 Figure 9 was replaced.

Comment #8 Line 420 Atmospheric CO2 gradient is not necessarily inversely proportional to temperature. Depends on the sign of the flux. Go back to flux-gradient theory!

I don't think the ideal gas law has anything to do with it - concentration (mole fraction, ppm) is independent of density. Unless you mean CO_2 density.

Replay #8 We mean CO_2 density (moles per volume).

Change #8 No change.

Detailed captions and notes of the attached figures.

Figure 9. Comparison of ΔpCO_2 (water minus air) versus eddy covariance fluxes calculated with conventional post-processing (PP1) and with our new post-processing procedure (PP2). The linear relationship was significant after PP2 (solid line; $P < 10^{-3}$) but not after PP1 ($P > 0.4$).

Note: Because of the uncertainty of the gas transfer velocity at the lagoon site, we decided to replace the comparison with the bulk formula flux to a comparison with ΔpCO_2 in this figure. According to the bulk formula equation, ΔpCO_2 and the air-water CO_2 flux are related linearly. This figure demonstrates how the filtering used in this study revealed that ΔpCO_2 was one of the main causes of the eddy covariance flux. The remaining factors are thought to be the wind speed, CO_2 solubility, and the distance from the platform.

Figure xx1. Changes in the number (a) and average value (b) of diurnal fluxes due to filtering. Some plots of data after PP1 were omitted to facilitate visualization. The ratio of the flux data after PP2 (red line) was almost the same between daytime and nighttime. In contrast, the average value in the daytime shifted to positive (efflux). The shift was caused by high-pass filtering (green data to solid line and closed circles) and not by filtering using normalized standard deviations (dotted line and open circles to green data).

Note: These figures show the diurnal bias (or absence of bias) due to filtering. The number of data was not biased, and the filtering using normalized standard deviations did not cause significant bias, but high-pass filtering shifted the daytime flux to positive values. This shift occurred because the Webb-Pearman-Leuning density (WPL) terms, which usually have positive values, were not affected much by high-pass filtering because the effect of long-term variations of temperature and vapor was smaller than that of CO2. [This figure will be placed after Fig. 9.]

Figure xx2. Cumulative EC fluxes after PP1 and after PP2. The data during the gap period due to bad weather condition have not been included. The seasonal trends of the fluxes were roughly the same except for the large change about 30 days after the start of measurements. This result indicated that the use of PP2 did not bias the seasonal trend and improved the continuity of the time series. [This figure will be placed after Figs. 9 and xx1.]

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2017-499, 2017.

Figure 9

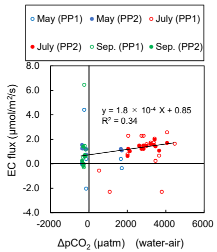

Fig. 1. Replace Fig. 9

Figure xx1

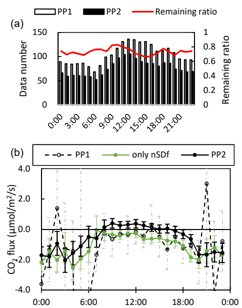

Fig. 2. Newly added Fig. xx1

Figure xx2

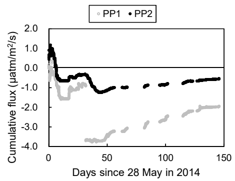

Fig. 3. Newly added Fig. xx2