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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 followings are responses to your comments. We decided to replace Fig .9 according to your comment. Please check the detailed caption and note of the figure.

Comment #1 1. The data is collected by an open-path sensor. Under conditions in the study it is more or less OK although for small fluxes a closed-path sensor is preferable. So EC measurements are probably OK, but the problem is the absolute concentration

to get to know fCO2air, which is used in the BF method, and also in the analysis assuming fCO2water zero. How was the sensor calibrated and what was the absolute accuracy for the concentration? i) In addition, related to the EC measurements, there is no information on the actual set-up, e.g. how was the sensor positioned beside the anemometer?

Reply #1 The fCO2air sensor (LI-7500A, Li-Cor) was calibrated by Li-Cor about 1 month before the measurements were made. The accuracy was guaranteed to be less than 1 %. The position of the fCO2 sensor was 20 cm to the north and 20 cm to the east of the anemometer.

Change #1 We will add this information.

Comment #2 2. The section of 3.3. BF data is very confusing. Reading it you get the impression that only data, which is reliable, is from 15 July. If yes, why the data from other periods are used in the analyses? If only 15 July remains, can you make any conclusions based on the BF data based on the so small data set?

Reply #2 We replaced Fig. 9 to enable comparison of the eddy covariance flux with delta pCO2, not the bulk formula flux. The eddy covariance flux and delta pCO2 cannot be compared directly, but they are expected to be consistent with respect to their signs (i.e., plus or minus). The new Fig. 9 shows that the inconsistencies of the plots in May and September were removed by filtering. In addition, all the data (i.e., the data for May, July, and September) were significantly correlated with delta pCO2 after filtering.

Change #2 Figure 9 was replaced. The description of the bulk formula flux calculation will be rewritten.

Comment #3 3. My biggest problem is with the whole main result of the paper: how do we know that the filtered data is erroneous? When you look the Fig. 5 it is quite clear (by subjective analysis) that two data points, the biggest and smallest ones are very probably outliers. The two other smaller (in absolute sense) data points may be outliers, maybe not, but if we accept that the most left data point is outlier based on the criteria presented in the paper, how from that stems the conclusion that all data points with higher nSD, Sk or Ku values are outliers? This is not explained/demonstrated in the paper so that I could understand and digest it. How do you know that so high nSD, Sk or Ku values are "wrong"?

Reply #3 We defined erroneous data to be data with extreme spikes, jumps, and shifts during 30-min intervals. Such data tended to have large nSD, Sk, or Ku values. The filtering used in this study should be valid quantitatively. As for the validity of the threshold, we calculated an arbitrary value, but we estimated the threshold based on criteria that were clear (determined by the largest and second-largest outliers) and efficient (as many data as possible remained). Although there is no way to tell whether all data above the threshold were wrong or vice versa, the filtering should efficiently decrease the ratio of wrong data to right data. We agree that perfect filtering based on fundamental theoretical criteria would be more realistic, but the establishment of such criteria is impossible based on the present state of knowledge.

Change #3 We will add the foregoing description to the Discussion section.

Comment #4 i) The argumentation based on the flux values found in the literature is lot fully sound; you have quite special case, the lagoon with vegetation, which can cause quite high uptake. ii) I do not understand why the 1 day and 1/2 frequencies show up in the filtered data; iii) The argumentation using Fig. 9 is not very strong: in July you can't say that the PP2 data essentially differ from PP1 data, neither in September, excluding one single point (flux value c. 6). iv) L. 382: only 3% data indicated "too low flux values", is this really a strong argument? v) L. 394-396: I understand that the convection discussed is related to very shallow layer in the water-side close to surface, and the depth of 2 m is not shallow at all in that sense.

Reply #4 i) We agree that our site was a special case. Therefore, we replaced the comparison of the eddy covariance flux with the bulk formula flux to a comparison of

the former with the delta pCO2, which should be one of the main factors regulating the air-water CO2 flux. ii) Please note that both spectra were normalized. The spectrum before filtering also contained a frequency peak. However, the peak was far smaller than the peak in the high-frequency region before filtering. iii, iv) We have replaced Fig. 9 because of the uncertainty in the bulk formula flux. In the new figure, the eddy covariance flux is compared with delta pCO2 instead of with the bulk formula flux. Please see our reply to #2. v) The argument concerning the shallowness was based on the equation in Rutgersson and Smedman (2010) (Eq. 10). They suggested that the enhancement of the gas transfer velocity could be estimated by using the buoyancy flux and the depth of the mixed layer. We estimated that a depth of 2 m was too shallow to calculate any enhancement using that equation.

Change #4 i, iii, iv) Figure 9 has been replaced. v) We will add the foregoing description.

Comment #5 4. Fig. 11 and the whole discussion on that (L. 414-437) is very obscure; I don't understand the arguments based on the ideal gas law, in my understanding the concentration is governed by the strength of the sink/source on the surface and the atmospheric transport (turbulent or advection); thus the message of Fig. 10 is also obscure; what is the discussion related to the Fick's law? The Fick's law becomes important in small scales, sub millimetre or so?

Reply #5 We agree that the concentration is governed mainly by turbulence and advection. Figure 10 extracted only the effect of the temperature gradient; it did not include any other factor, such as turbulence. We think that Fick's law can be applied at a scale of several meters.

Comment #6 5. Problems in the structure: i) Fig. 4 is introduced (L. 301) before Fig. 3b was introduced/discussed. ii) There are three Figs. introduced in Discussion although they present basic information, especially Figs. 9 and 10; I would put at least Figs. 9 and 10 in Section 3 Results.

Reply #6 Figure 3b was presented before Fig. 4 to facilitate comparison with Fig. 3a. Figure 9 (replaced with attached file) has been moved to the results section, but Figure 10 was not be replaced because Figure 10 is the basis of the argument in the discussion.

Change #6 Figure 9 has been replaced.

Minor: Comment #7 1. Line 87: what is "vide infra"? 2. L. 194-195: what was the 1-D extension of the footprint? From what distance e.g. 90% of the flux signal was originated?

3. L. 216: what is "objective gases"? More generally, why there is discussion on methane although it wasn't studied? 4. L. 281: "up to 20 cm"? So the closest distance to the water surface was 20 cm? I know that it is difficult to measure very close to surface but do you know how well the depth of 20 cm represents the surface concentration? 5. L. 282-284: what was the number of the sampling sites? 6. There is no information how the possible movements of the platform were handled/corrected? 7. The reference Vesala is incorrect: it should be Vesala T, Eugster W and Ojala A 8. Explain RSSI and HP in Fig. caption. 8. L. 630-631: what is "long-term effect of CO2 change"? 9. Fig. 9: Explain all abbreviations in Fig. or give info where there are found.

Reply #7 1. Vide infra means “see below” in Latin. 2. The “square meter” in the sentence was a mistake. 3. We could find a reference to RSSI filtering for methane measurements, but not for CO2 measurements. However, both gases were measured by the same methods (NDIR absorbance). 4. This means that the averaged depth of water sampling was 20 cm below the water surface. Strictly speaking, the water at a depth of 20 cm was not the same as the water at the surface. However, this difference was the technical limitation of the field survey. 5. The details are described in the appendix. 6. We did not measure any movement of the platform. 7. We have corrected the citation in accord with the comment. 8. The effect of a CO2 change during a time interval of several minutes is shown in Fig. 7. 9. We have replaced Fig.

9.

Change #7 2. The word “square” will be removed. 3. We have removed the text about methane in accord with the other reviewer's (Comment in pdf by the reviewer #3) comment. 5. We have added the site number in the main text.

Detailed caption and note of the attached figure Figure 9. Comparison of ΔpCO2 (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 ΔpCO2 in this figure. According to the bulk formula equation, ΔpCO2 and the air-water CO2 flux are related linearly. This figure demonstrates how the filtering used in this study revealed that ΔpCO2 was one of the main causes of the eddy covariance flux. The remaining factors are thought to be the wind speed, CO2 solubility, and the distance from the platform.

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Figure 9

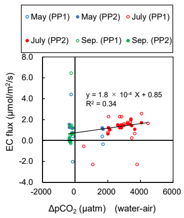

Fig. 1. Replace Fig. 9