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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 the responses to your comment. We attached the replace Figure 9 and pdf file for responding to the comment #4. Please check the detailed caption and note of the replace Fig. 9 at the end of this document.

Comment #1 First, I am not sure I agree with the choice of journal given the results of the study. I find the results a little unsatisfying since they don't give any real insights into ecosystem function or boundary layer atmospheric anomalies, really. It's really a

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deep dive into EC nitty gritty, which is fine, but in my opinion, doesn't belong here. This is primarily a methodological paper to me, so would fit better in a journal which encourages methodology manuscripts. Even so, I don't really know that I agree that the post processing package would be groundbreaking enough to publish on its own without significantly improving the proof that it actually makes a necessary correction to the EC signal.

Reply #1 We disagree with this comment. Comprehensive measurements of coastal CO2 fluxes have been among the most important research foci in the biogeosciences, and the noise of eddy covariance measurements and their inconsistency with results obtained using other conventional methods have confounded analyses. In this study, we hypothesized that filtering would enable more informed comparisons of measurements. We agree that the discussion about the temperature gradient was qualitative. The temperature gradient topic will require further research, but the present analysis should be valuable for biogeoscience studies.

I think all three steps employed are used in some form or fashion already in many EC processing setups (not sure about Eddy Pro since I don't use it, which is the main reason I think it might have merit. That said I'd be a bit surprised if they didn't account for at least the RSSI and despiking).

Comment #2 I have some methodological concerns as well, which I'd like to highlight. My main concern has to do with the high pass filter. It seems to me the high pass filter is actually working as a low pass filter (i.e. is allowing the low frequency signal to pass through while attenuating the high frequency signal). On figure 7, for example, what (to my understanding) should have been removed is actually the gradual shift from above 16 mmol m-3 to below it, but instead it seems to me that the high frequency 'noise' has been removed. To my eye, this is actually a fundamental error with the EC approach since the high frequency signal is what we actually base EC measurements on. I'd actually agree the gradual shift should be removed, but that doesn't appear to be what's been done here. It could be that the red line is actually the signal that was removed, but I don't think that's the case since figure 8 also shows that the high frequency signal has been attenuated. Indeed, the low frequency signal actually appears to have been amplified based on figure 8, which is the opposite goal of the HP filter. These issues had me concerned enough to take a deep look at equation 2, which I think might be the root of the problem. Eqn 2 (the HP fitler), takes the general form of a HP filter, but the choice of independent variables seems off to me. If you expand the formula it boils down to something really simple that I don't think is accomplishing what the authors intended.

Reply #2 First, the description of Eq. 2 was misstated. We apologize for the confusion. We think that this comment reflects some misunderstanding. The red line in Fig. 7 (a) shows the filtered trend, not the data after filtering. The difference between the trend (red line) and the raw data (black line) was used for the calculation. In addition, the high-frequency attenuation in Fig. 8 has no direct relationship with the high-pass filter. The high-pass filter was applied to the 10-Hz raw CO2 concentration data or wind velocities, and the spectrum in Fig. 8 was calculated from 30-min CO2 fluxes. The decrease at high frequencies in Fig. 8 resulted from the removal of outliers by filtering. The outliers reflected large, discontinuous fluctuations that affected the spectral analysis like white noise.

Change #2 We have corrected Eq. 2. Please see the attached pdf (Responses to reviewer #3.pdf)

Comment #3 While the HP filter is my main concern, I don't really love the despike section either, but it's more how it is presented than a concern that the method is valid. Figure 5 and the text don't really seem to show how any one of the criterion examined is better than another. Actually, looking at figure 5, if I were to come up with a rule to try to highlight the four points, the best I could do in each panel appears to be setting a limit to the maximum and minimum carbon flux. Since you could do that before sorting the data by the new criterion, I don't think that it makes things clear. That said, I have enough experience with despike routines that I understand more or less what they're trying to accomplish, I just don't think the figures are doing justice to the work. Figure 5 needs to be completely overhauled to show your results more clearly, since the text and figure don't currently support one another properly (in my opinion).

Reply #3 We agree that the threshold value for de-spiking was arbitrary and was not perfect. The theoretical basis for the threshold was hypothetical and could not be identified. Alternatively, we decided to use an empirical but simple way to filter the data. We can argue that filtering using normalized standard deviations or other parameters was qualitative correct, and filtering should improve the eddy covariance data. It is likely that a more theoretical (and complex) threshold would more accurately correct the eddy covariance data, but we feel that this quick albeit rough approach facilitates evaluation of the carbon cycle in coastal ecosystems, an issue that requires prompt action.

Comment #4 More detailed line by line comments are included on the attached PDF.

Reply #4 The replies are described in the attached file.

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.

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2017-499/bg-2017-499-AC3-

supplement.pdf

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

Figure 9

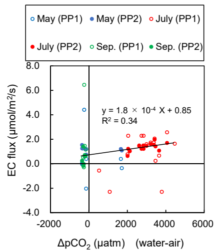

Fig. 1. Replace Fig. 9

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