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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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This paper is an interesting look at possible corrections to the eddy covariance approach when used over open water. The manuscript uses several other approaches alongside the EC method to validate their corrections. The highlighted post processing approaches are a despiking and high pass filtering methodology. While I find the paper generally well written enough to follow, I have what I consider to be several major concerns with the paper.

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 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. 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).

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.

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).

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

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2017-499/bg-2017-499-RC3-supplement.pdf

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