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

A. Desai (Referee)

desai@aos.wisc.edu

Review

The manuscript develops a set of filtering algorithms to improve the quality of lake eddy covariance data. The set of filtering tests (signal strength, statistical moments, and high pass filtering) all appear to improve the comparison of EC to bulK formula type approach (for a limited set of campaigns), and make the fluxes more "reasonable" compared to published literature on lake fluxes. As someone who runs lake EC sites,

I am fully aware of the large amount of noise given that lake EC pushes several EC assumptions (topography, cold surfaces, homogenous footprints, high humidity) and so I am sympathetic for the need for this. 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 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)

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 pre-computation. 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.

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.

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?

Line 287 Please be aware that there is high uncertainty in derivation of pCO2 from carbonate chemistry in lakes (see Abril et al 2015 and Golub et al., 2017)

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.

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?)?

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 CO2 density.

Abril, G. et al. (2015), Technical Note: Large overestimation of pCO2 calculated from pH and alkalinity in acidic, organic-rich freshwaters, Biogeosciences, 12, 67–78, doi:10.5194/bg-12-67-2015.

Golub, M., et al. (2017) Large uncertainty in estimating pCO2 from carbonate equilibria in lakes, J. Geophys. Res.-G., https://doi.org/10.1002/2017JG003794.