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Interactive comment on "Technical Note: Time lag correction of aquatic eddy covariance data measured in the presence of waves" *by* P. Berg et al.

Anonymous Referee #3

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The manuscript by Berg et al. presents a new method for correction of benthic flux measurements using a fast oxygen sensor with an AUV. The correction makes use of periodic vertical advection of the oxygen gradient to find the effective time offset between the velocity and oxygen data. The paper represents a novel and valuable contribution. My recommendation is for publication with response/attention to further comments listed below.

I'm a bit uneasy about the magnitude of the time offsets that are calculated. Lags of nearly 2 seconds are observed, comparable to the wave period and much larger than the response time of the instrument. It's apparent that the authors are similarly uncomfortable based on the discussion on pg 8410, where this is attributed to 'phyto-detritus'.

Presumably this would result in a slower sensor response. It's unclear whether this slow response would affect the flux calculation. Berg et al. 2015 suggests that a lag of 0.51 would have minimal effects on the flux, although a 2 sec. lag suggests a much slower sensor response.

The focus here is on temporal misalignment due to oxygen sensor response and also due to sensor spatial separation, although sensor misalignment relative to the vertical can also play a role. It appears that the flux calculation is made without any correction for sensor misalignment (i.e. Shaw and Trowbridge) (the references listed in describing the flux calculation (Line 19, p8403) all deal with cases without waves). In that case, the flux calculation may be contaminated by a wave-induced component resulting from the apparent phase shift between w and c. Of course this shift would also affect the calculation for $\tilde{z}$ in equation (1), which would result in a time offset. This offset could then contribute to the calculated offset, and would thus be addressed by the proposed method. If I'm interpreting this correctly, this may account for some part of the observed lags.

It would be useful if the authors provide wave velocities along with significant wave heights where they are describing conditions (sect. 3.1) – this is the relevant value to assess the wave strength at the measurement site. The reader can calculate this using the dispersion relation, of course, but it would be nice to have it given.

The authors discard stirring sensitivity effects based on the characteristics of oxygen variations on pg. 8411. I agree that the data in Fig. 6a appear qualitatively to be symmetric, but this could be assessed more quantitatively. The presence of a harmonic in the signal is also not readily apparent, although I expect that would be damped significantly by the slow sensor response. A spectral analysis of the concentration signal might be useful to identify higher harmonics and show the sensor response cutoff. I'm unclear on what effects this would have on the calculations though.

Interactive comment on Biogeosciences Discuss., 12, 8395, 2015.