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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.

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Summary: The authors present a study about the analysis and correction of major biases induced by wave motion on eddy correlation flux estimates. The manuscript is well written and the complex problem is well explained using a wave model and field measurements. The time lag between flow and O2 sensing is identified as the critical unknown parameter that has to be estimated most precisely to prevent strong flux biases from wave motion. The offered solution to the problem is based on wave theory and applies the predicted cross-correlation between O2 and the wave induced vertical displacement of the bottom water. The study is an important contribution to

the field of turbulent flux measurement as wave interference is an often encountered problem especially in coastal waters. However, there are a few issues that should be considered by the authors.

General comments:

1. The causes of the time lag are well explained by the authors, but they are not much considered in the procedure of the time shift correction itself. The authors concentrate on the cross-correlation analysis in light of wave theory, even though the time lag phenomenon is explained by settings that are usually known: i.e. the sensor response time, current velocity, current direction and the sensor distance to ADV sampling volume. In principle, these settings should be used to first calculate a realistic range of magnitude and direction of the time shift that subsequently can be compared to the outcome of the correlation analysis. An example for such a comparison can be found in Donis et al. 2014. I think including this step before the correlation analysis would increase the confidence in any time shift correction by identifying nonrealistic results. For example, does it make sense to have a negative time shift as shown in Fig. 4b for the old correction method?

2. The new time lag correction presented by the authors is based on 2 steps. First the vertical displacement (z) is calculated by integrating the vertical velocity over time. Then the time shift for the most negative/positive cross-correlation between O2 and z is calculated from the cross-correlation function, i.e. from a step wise shift of the respective time series. For these two steps, the wave signal is extracted by low and high pass filtering, whereas the resulting time shift is applied to the non-filtered time series of w and O2. I wonder if this procedure could be simplified a bit. The wave model gives a phase shift of 90° between w and z (compare A2 and A4 in appendix). Accordingly, the correlation functions of <O2*z> and <O2*w> in figure 4b are phase shifted by 90° as well. The new time lag correction could thus be calculated easily by traditional shifting to the maximum flux and correcting for t = $\frac{1}{4}$ of the wave period (i.e. 90° phase shift). The wave period is also found in the correlation function. This would

make the z-displacement calculation unnecessary.

3. All calculations of the optimum time shift are done for filtered time series that contain the extracted wave signal. As I understood, the filtering is based on visual inspection of the time series and needs a clear wave signal in both O2 and w. I think it could be helpful to analyze the velocity in the frequency domain and use the horizontal velocity (u) spectra to best identify the wave period, because the wave amplitude is several fold stronger in the horizontal compared to the vertical velocity. Then, with adequate filtering, the wave signal can be extracted and processed as suggested, maybe even for settings with less dominant waves.

Specific comments:

Section 1.2: The work of Donis et al. 2014 addressed the time lag problem and the specific sensor-flow settings in a flume study. This could be of value for formulating the problem in this section.

Section 1.3 + Figure 1: It would be helpful to define the time shift here. What is a positive/negative shift? This is also not clear for the figures 2+4.

Section 2.1: The wave model assumes linear water waves. However, the model parameters for wave length and water depth suggest non-linear shallow water waves. Could the authors come up with some arguments why the linear wave model is nevertheless appropriate?

Section 2.1: Figure 3 is somewhat irritating as it includes the assumed real flux at 100% of the time lag bias. Fig. 3 could be misunderstood as if the increased velocity leads to only 10% of the assumed real flux. I assume it is meant that the bias is only +10% above the real flux. This should be changed or explained better.

Section 2.2, L20: A 'minimum cross-correlation' is a bit misleading and may suggest 'no correlation'. The cross-correlation in this case is significant but negative. Maybe write: . . .corresponds to the most negative cross-correlation.

Section 2.3: This section would benefit from a more detailed and precise description of the subsequent calculations steps since this is the recipe that EC users may use in the future. This could also be the section where a first estimate of the time shift should be made based on sensor and flow settings (see general comment 1). For example, it can be seen in Fig. 4b that the time shift according to the old correction method does not make sense as it is either in the wrong direction (i.e. negative) as indicated in Fig 4b, or above 1 second when the maximum flux in the positive direction is used.

Section 3.1: The first example was measured at dusk over a sea grass meadow where the net consumption increased and the current velocities decreased over time. According to the theory presented in the sections before one would expect an increasing time lag with decreasing velocities which is not reflected in Fig. 5c. Furthermore, the flux bias should increase with increasing flux and decreasing velocities (see Fig. 3). Figure 5c suggests the opposite. The latter might be explained by the increasing water depth and decreasing wave height, which both should reduce the time lag bias. However, the constant time lag even for very low velocities should be explained.

Section 3.2: The second example was measured with a Clark-type microelectrode. Applying the new correction produced time shifts of 1-1.7 seconds, which seems too high and contradicts the previously made statement that Clark-type microelectrodes are fast and do not need a time shift correction (t90 = 0.3). The authors discuss this result (P8410, L21) suggesting that the electrode tip was damaged or coated by phytodetritus, which reduced the response of the sensor. However, such a significant reduction in response time should lead to a significant loss of the flux at higher frequencies. This is not reflected in Fig. 6c where the flux remains high after the response time doubled at minute 460. How can this be explained?

Section 4, Page 8411 last paragraph: The second example was measured with a Clark-type microelectrode that is known to be stirring sensitive and the question arises how much does the artificial flux created by the stirring sensitivity of the sensor contributes to the real flux? In the second example, the large fluxes of the order of a few 100 mmol/m2/d suggest that the relative contribution could be minor. However in a wave flume study, Reimers et al. 2015 measured for similar wave velocities artificial fluxes of the order of 100 mmol/m2/d. The authors rule out the stirring effect arguing that the O2 signal does not show typical signs found in the wave tank study and, further, that the flux corresponds to the light regime. I think these arguments only support that the artificial flux from stirring sensitivity is not dominant but it is well possible that there is a significant contribution. For example, a constant negative contribution of the artificial flux could lower positive fluxes and increase negative fluxes without changing the general correlation with the light regime. Furthermore, the O2 signal from stirring sensitivity depends on the flow direction. This was shown for unidirectional flow (Holtappels et al. 2015) and waves (Reimers et al. 2015) for sensor settings, where the sensor was either in line or perpendicular to the (oscillating) flow. The wave currents in Fig. 6 seem to be 45° (see Berg and Huettel 2008) and are on top of a unidirectional current from yet another direction. The artificial O2 signal patterns that arise from such a complex setting are not known and the combination of true wave-induced O2 fluctuations and the artificial stirring-induced O2 fluctuations are also not considered. In fact, if the stirring induced O2 signal is only slightly dependent also on the vertical component of the flow it could cause a phase shift of the max/min O2 signal expected from wave theory. This might even explain the very high time shift corrections for this data set.

References: Donis, D., M. Holtappels, C. Noss, C. Cathalot, K. Hancke, P. Polsenaere, F. Wenzhöfer, A. Lorke, F. Meysman, R. Glud, and D. McGinnis, 2014: AN ASSESSMENT OF THE PRECISION AND CONFIDENCE OF AQUATIC EDDY CORRELATION MEASUREMENTS. J. Atmos. Oceanic Technol. doi:10.1175/JTECH-D-14-00089

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