Author's response to comments by Reviewer 2, Moritz Holtappels (Biogeosciences Discuss., 12, C3406–C3410, 2015).

By

P. Berg, C. E. Reimers, J. H. Rosman, M. Huettel, M. L. Delgard, M. A. Reidenbach, and T. Özkan-Haller.

We thank the Reviewer for the positive and constructive comments. Below, all comments from the Reviewer are written with '*italic*' font. Our responses are written with 'normal' font.

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?*

We agree, it adds merit to the new time lag correction to ground truth it against all available information. Although in reverse order from what the Reviewer suggests, we believe we did this to the extent possible for our optical sensor, which is the only oxygen sensor used in the study for which we know the response time. Our assessment is described in the paragraph starting on page 8409, line 23: 'The wave-driven periodic variation in $\widetilde{O}_2$ and $\tilde{z}$ (Fig. 4A) produced a clear minimum in cross-correlation of these two variables that could easily be located (Fig. 4B). This minimum occurred at a time shift of 0.78 s, which gave a corrected flux that was 42% of the uncorrected flux (Fig. 4C). For reference, the optimal time shift found for the same oxygen sensor in unidirectional river flow was on average 0.83 s (Berg et al. In press) and 0.88 s in the example shown here in Fig. 1. The sensor's own response time ($t_{90\%}$) was measured in lab tests to be 0. 51 s, when inserted from air into water baths. The somewhat slower response found in the field, where the sensor was permanently under water, likely represents the equilibration time for the oxygen concentration through the thin boundary layer that forms on the oxygen sensing foil (Berg et al. In press).'

Due to the relatively short but unknown equilibration time through this boundary layer, and the changing velocity direction due to waves, we cannot produce a 'tighter estimate' of what the time lag correction should be as was done in the more controlled flume study by Donis et al. 2014. Thus, we suggest leaving this paragraph as it is.

Negative time shifts (moving the oxygen data forward in time relative to the velocity data) do make sense. For example, a microelectrode with a response time of 0.2 s positioned 1 cm upstream from the center of the ADV's measuring volume (~0.3 cm from the edge of the measuring volume) in a unidirectional flow with a velocity of 2 cm s^{-1}, should give a negative time shift of around -0.3 s.

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 = ¼ 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.*

We believe the Reviewer is correct; the outlined modification of our correction should give the identical results. However, overall, we do not see this as a significant simplification. Firstly, the calculation of *z'* is very simple:

1) Compute $z_i = z_{i-1} + w_i*\Delta t$, where Δt is the time between data points.
2) De-trend z_i to get z_i', using the same de-trending routine as for O_2.

Secondly, the modification is difficult to explain, therefore more challenging for a reader to comprehend. As a result, we suggest not including this modification in our revision. Please note, we already allocate a substantial amount of space for discussion of variants of our correction and also two other very different ones (page 8407, line 10 to page 8409, line 11).

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.*

Although not reported, we did perform an analysis along these lines for the data shown in Fig. 5. The corrected fluxes showed very little sensitivity to the width of the filter window. For filter widths ranging from 1 to 6 times the wave period, the same flux was calculated within ±4%.

We have now also performed this analysis for the data shown in Fig. 6 and suggest including the results of both analyses in the text. Furthermore, to be consistent, we suggest using a filter width of 4 times the wave period for all presented calculations. For the data shown in Fig. 5 and 6, the corrected average nighttime fluxes varied within ±4 and ±11%, respectively, for filter widths ranging from 2 to 6 times the wave period.

Related to this, the correction was not sensitive to the smoothing of the wave signal, so this operation can be omitted. We suggest adding this information to the text as well. Also, the wave period can just as easily be identified from the less noisy vertical velocity component as from the horizontal component.

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 1.2 formulates the problem to be addressed, which relates entirely to surface waves. As a result, a reference to a flume study done in unidirectional flow does not really fit in here. We suggest referencing this paper in the Introduction instead, along with other earlier papers that have addressed time lag in eddy covariance data measured in unidirectional flow.

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.

We agree, and suggest adding this information to the text and the legends of Figs. 1, 2, and 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?

It is important not to confuse nonlinear/linear waves with the shallow/deep water approximations to the linear wave theory solution. These are two different things. Typical wave parameter values in our field examples were well within the range for which linear wave theory is a good approximation. Specifically, for linear wave theory to apply, the ratio of wave amplitude to wavelength and the ratio of wave amplitude to water depth should both be <<1. For example, using the values given in Table 1 and the equations listed in the Appendix, gives a wavelength of 7 m, therefore, a ratio of wave amplitude to wavelength of 0.008 and a ratio of wave amplitude to water depth of 0.04.

Linear wave theory is therefore expected to work very well for our field examples. We suggest adding this information to the text.

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.

We chose this format because the time lag bias is proportional with the true flux. We suggest removing the 'assumed real flux' and change the *y*-axis label to 'Time lag bias as percent of real flux (%)'.

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.

The term, 'minimum in cross-correlation' is the mathematically correct term, but to avoid confusion, we suggest adding in parenthesis: '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.

We agree that a more detailed step-by-step outline of how to compute the new correction is warranted, and we suggest describing this in a new appendix (Appendix B).

We also agree that the data in Fig. 4b can be used better. They can also be used to illustrate how and why the traditional time shift correction does not give meaningful results in the presence of waves. We suggest adding this explanation to the text.

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.*

The time lag bias, not the time lag itself, increases with decreasing mean velocities.

The time lag bias scales with the true flux, so if everything else is unchanged, the ratio between the bias and the true flux is constant. We agree with the Reviewer, that the moderate decrease in time lag bias with time in Fig. 5c is caused by the combined effects of the moderate decrease in mean velocity, the moderate decrease in wave height, and the increase in water depth.

We suggest strengthening the discussion of what affects the time lag, and the time lag bias, in the text.

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?

The average time shift for the data in Fig. 6 was 1.1 s, which is not in line with the typical response time ($t_{90\%}$ < 0.3 s) for the Clark-type microelectrodes that we use. Yes, the most likely explanation is that this sensor was damaged or coated by phytodetritus. In unidirectional stream flow, we have seen Clark-type sensors deteriorate gradually through deployments as indicated by step-wise increases in time shifts found using the traditional time lag correction (Fig. 1).

While a sensor response time of around 1 s is not optimal, we disagree that this by default should have a large effect on the total fluxes as the ones in Fig. 6c. Co-spectra of the oxygen concentration and vertical velocity show that typically only a small fraction of the flux contribution is associated with frequencies higher than 0.5 Hz. So, even if a larger portion of the flux signal is lost at high frequencies, it should not affect the total flux significantly.

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.

We believe that our detailed arguments stand for why stirring sensitivity was not a dominant part of the signal recorded with the Clark-type microelectrode (Fig. 6). Again, the typical signs of stirring sensitivity that are easy to identify (see text for details) were not observed.

However, we acknowledge that our arguments are indications, and do not demonstrate that stirring sensitivity had no practical effect on the extracted fluxes. We have investigated this question further for the data shown in Fig. 6 as follows. We assumed that our microelectrode had a stirring sensitivity as found by Holtappels et al. (2015), using their fitting function and values for fitting constants (S_{sen} = 0.7%, n = 0.65, and B = 30). This particular dependency was found for an electrode pointing into the mean current which represents the orientation that gives the largest stirring sensitivity (Holtappels et al. 2015). We then applied this function to our data assuming this maximum sensitivity for all horizontal velocity directions. For each time point in our data, we calculated the size of the horizontal velocity, from that, the associated stirring sensitivity, and finally, the oxygen concentration as it should have been measured in the absence of stirring sensitivity. Fluxes calculated using the same velocity data and the uncorrected and corrected oxygen concentrations were then compared as a first order assessment of stirring sensitivity effects for this particular data set.

The average nighttime flux for the original data was -368.0 ± 20.6 mmol m^{-2} d^{-1} (SE, n = 45), whereas the oxygen data with stirring sensitivity removed gave a flux of -370.0 ± 20.5 mmol m^{-2} d^{-1} (SE, n = 45), or a difference of 0.6%. The equivalent calculation for the data with the time lag correction applied, gave averaged fluxes of -184.1 ± 11.2 and -185.2 ± 11.1 mmol m^{-2} d^{-1}, respectively, or as before, a difference of 0.6%.

We believe that this insignificant effect of stirring sensitivity should, at least partly, be explained by the moderate current velocity (2.7 ± 0.1 cm, SE, n = 45) which, as also pointed out by Holtappels et al. (2015), leads to small current-driven Reynolds stresses, thus reduced effects of stirring sensitivity on calculated fluxes.

We can add a description of this detailed calculation to the text, but also acknowledge that it will increase the length of the manuscript. We seek the Associate Editor's advice on this matter.

References:

Berg, P., D. Koopmans, M. Huettel, H. Li, K. Mori, and A. Wüest. In press. A new robust dual oxygen-temperature sensor for aquatic eddy covariance measurements. Limnol. Oceanogr.: Methods.

Holtappels, M., C. Noss, K. Hancke, C. Cathalot, D. McGinnis, A. Lorke, and R. N. Glud. 2015. Aquatic Eddy correlation: Quantifying the artificial flux caused by stirring sensitive O2 sensors. Plos One **10(1):** e0116564. doi:0116510.0111371/journal.pone.0116564.