# Author's response to comments by Reviewer 1 (Biogeosciences Discuss., 12, C3268–C3271, 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.

### General comments:

This manuscript describes a study that was conducted to investigate the implications of applying a time lag correction for eddy covariance (EC) benthic flux measurements performed in the presence of surface gravity waves. To date, aquatic EC studies have opted either not to apply a time-lag correction, or have applied a correction that was adapted directly from atmospheric EC procedures, where the scalar data are shifted stepwise in time relative to the velocity data to find the maximum numerical flux. This study demonstrates that applying this latter correction in the presence of even small waves (<5cm) can bias the flux significantly. Surface waves present a problem that is unique to aquatic measurements, so it hasn't been possible to adopt a more appropriate procedure directly from the atmospheric boundary-layer literature. In this manuscript the authors present a new approach for time lag correction in aquatic environments that is effective at minimizing this bias, and critically evaluate its application using both modeled and measured data.

This study is novel and would be a timely addition to the aquatic EC literature, since EC papers are being published with increasing frequency. The paper is concise and well written, and the authors have considered the latest methodological issues associated with EC. Figures, tables, and appendices are informative and clear. I also commend the authors for making all published data publicly available.

My assessment is that the paper is appropriate to be published as a Technical Note in Biogeosciences.

Some suggestions for editing and clarification are identified below.

Specific Comments:

Section 1.1.

Lines 29-31. I suppose one could argue that McGinnis et al. 2008, Lorrai et al. 2010, Lorke et al. 2013, Donis et al. 2015, Holtappels et al. 2015 also have 'scalar flux calculation methodologies' as their main focus. The sea ice EC community has also been active in this research area (e.g. work by McPhee, Sirevaag etc.). Could this last sentence be reformulated to focus more on the issue addressed in this paper specifically? This would also link in well to the section that follows.

Yes, we suggest removing the last sentence. Then, what remains of this section will provide a better transition to the next section where we define the problem that we will be addressing.

### Section 1.3.

Lines 11-15. The traditional time lag correction accounts for both the physical separation between the sensor tip and the ADV measurement volume, as well as for the response time of the sensor. In unidirectional flows with high flow velocity the small physical separation usually is insignificant and data shifting is mostly due to the response time of the sensor (e.g., Donis et al. 2015 JAOT). As it stands, this section (lines 11-15) seems to suggest that the physical separation of 1.5 cm is the reason for why data shifting results in a 2-fold increase in the numeric flux, but I suppose that the response time could have something to do with this too? Could the response time of the new optical sensor be included here or in the Fig. 1 heading?

Yes, the response time ( $t_{90\%}$ ) of the new optical sensor was measured as 0.51 s (Berg et al. In press), and yes, it contributes to the total lag time. However, the 1.5 cm physical separation was measured from the edge of the ADV's measuring volume to the edge of the 1 cm diameter sensor tip; this is equivalent to a 'center to center' distance of 2.5 cm. This is a larger distance than what is typically used for micro-sensors (i.e. oxygen microelectrodes), therefore, it contributes more to the total time lag. We suggest adding this information to the text.

#### Section 4.

Page 8407. Lines 25-29. The authors apply the time lag bias correction to two datasets that were collected in highly reactive sites (mean  $O_2$  uptake rates of 68 and 220 mmol  $O_2 m^{-2} d^{-1}$ ). This is understandable, because the time lag bias becomes more evident in such settings. The authors note that there might not be a clear periodic wave signal in the oxygen concentration during some periods at dusk and dawn when the oxygen gradient within the BBL is largely diminished.

A question that this raises is just how low the benthic flux needs to be in order for there not to be this periodic signal in the oxygen measurements. From a theoretical perspective, the oxygen microsensors are able to capture very small changes in oxygen concentration. McGinnis et al. 2011 suggest a resolution on the order of 0.004  $\mu$ mol L<sup>-1</sup> for their 16-bit AD converter. This would translate into a really small benthic oxygen flux, and therefore the sensors could, at least in theory, resolve a periodic wave signal in the oxygen concentration during most of the dawn and dusk periods, too. Such an analysis, to first-order, would also be informative for EC measurements performed in less reactive sediments such as those present in temperate systems in winter and in high-latitudes in general.

We do not think that it is possible to define such a minimum numeric flux below which the correction would fail. It would depend on a number of parameters.

From Appendix A (Eqs. A5 to A8), the following can be recognized: It is correct that the periodic wave signal in oxygen concentration is proportional to, or scales with, the vertical flux. However, the periodic wave signal also scales with the vertical mean oxygen gradient in the water column, and because this gradient scales with the turbulent mixing, it also scales with both the mean current velocity and the sediment surface roughness. Hence, if, for example, a given flux is numerically large enough so that an adequate periodic wave signal in concentration can be defined over natural turbulent fluctuations for the time lag correction to work, this may very well change if the mean current velocity increased. This substantial dependency is reflected in Fig. 3.

Section 4. Page 8408. Lines 17-25. Have the authors applied a frequency-domain correction to the same two field datasets that are presented in this study? It would be interesting to summarize the outcome of such an analysis over here, to hint at the potential importance of wave- or pressure-driven exchange processes in permeable sediments. Also, would the authors expect wave-driven O2 exchange processes to be of lesser importance in cohesive sediments?

We will address the last part of the comment first, and clarify that the sediment type below the eddy covariance measuring point does not affect the time lag bias and its correction studied here. The bias arises from a non-perfect data recording of periodic wave motions in a water column that contain a clear vertical gradient in mean oxygen concentration produced by the underlying sediment's uptake or release. It is implicitly assumed in the correction that no substantial horizontal gradients in mean oxygen concentration exist. For example, if anoxic porewater is released locally from sandy ripple crests, then it is assumed that this local horizontal variation has been smeared out by turbulent mixing at the measuring height (further details are given below).

We did consider applying a frequency-domain correction to our two field datasets, but opted not to. This correction would remove the oxygen flux contribution associated with wave motions in the narrow wave-frequency band. We argue (page 8408, line 22) that: 'This, or similar approaches, are already widely used to remove wave contributions to Reynolds stresses for wave dominated near-bottom flows (e.g. Bricker and Monismith 2007). However, such corrections should be applied with caution here because part of the real vertical oxygen flux may be facilitated by wave motions, and thus occur at the wave frequency. Wave motions over rough benthic surfaces can give rise to eddies or water parcel ejections at wave frequencies, which expand up into the bottom water, well above the wave boundary layer (Kemp and Simons 1982; Sleath 1987; Reidenbach et al. 2007).' Consequently, a frequency-domain correction may give fluxes that are underestimated – fluxes that represent vertical transport due to current driven turbulent mixing, but exclude any real contributions associated with wave generated eddies – therefore, they are not meaningful in a biological or ecological sense.

Similarly, a point that could be discussed further is the extent to which wave-driven localized release areas of reduced pore-waters (so-called ripple 'upwelling zones'; Precht et al. 2004, L&O) could confound the interpretation of the mean oxygen gradient in the BBL. Are upwelled anoxic pore-waters expected to project upwards to the oxygen sensor before being mixed into the bottom waters of the BBL? Perhaps this is not a problem, because upwelling zones are highly localized and typically constitute <30% of the sediment surface area. But because the focus of this study is quite heavily on permeable sediments it could be good to add a sentence or two on this potential consideration.

Please see our comment above on local porewater release from sandy ripple crests. Also, this porewater release happens at 'creeping' vertical velocities, on the order of 0.001 cm s<sup>-1</sup> (Precht and Huettel 2003), which should ensure a close-to-instant mixing with the water-column water above, whereas horizontal wave velocities easily exceed 10 cm s<sup>-1</sup>. In other words, it is appropriate to assume that any horizontal variations in oxygen uptake or release at the sediment surface have been smeared out by turbulent mixing at the measuring height. However, we acknowledge that a remark along these lines will be helpful, and we suggest adding this information to the text.

Figures 5 & 6 panel B: I suggest adding the mean flow velocity magnitude.

We agree that this will be useful information and suggest adding this variable to Figs. 5 and 6.

Technical corrections:

Section 1.4. Line 22: Semicolon should be replaced by a comma.

We agree.

Section 2.1. Page 8401 Lines 24-25: Sentence should read "...for example one with a roughness of 10 mm...".

We agree.

#### **References:**

- Berg, P., D. Koopmans, M. Huettel, H. Li, K. Mori, and A. Wüest. In press. A new robust dual oxygentemperature sensor for aquatic eddy covariance measurements. Limnol. Oceanogr.: Methods.
- Precht, E., and M. Huettel. 2003. Advective pore-water exchange driven by surface gravity waves and its ecological implications. Limnol.Oceanogr. **48**: 1674-1684.