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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 #1

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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.

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

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(lines 11-15) seems to suggest that the physical separation of 1.5cm 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?

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 O2 uptake rates of 68 and 220 mmol O2 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 μmol L-1 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.

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?

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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.

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

Technical corrections:

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

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

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