Biogeosciences Discuss., 8, C6118–C6123, 2012
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Biogeosciences Discussions

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Interactive Comment

Interactive comment on "Oxygen exchange and ice melt measured at the ice-water interface by eddy correlation" *by* M. H. Long et al.

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Received and published: 16 March 2012

Our response to referee F. Meysman: The referee's comments are in *italics* and our responses are in normal font.

General assessment

This paper presents a novel application of the recently developed Eddy Correlation Technique (ECT) for flux measurements in aquatic systems. I this study, the technique is applied in a quite challenge setting, namely under sea ice in Greenland. The study is innovative, because it combines a geochemical application ECT, where one looks at O2 fluxes (typically towards sediments), with a physical application, where one looks at heat fluxes (as under sea ice). As the authors mention, both applications have been *done before, but never combined. The manuscript is well written, reads fluently, has carefully prepared figures and is of the right length. In fact, I have only a few (mostly minor) comments, and so, after adapting these, the manuscript is ready for publication.*

General comments

One problem with the ECT is that datasets are cleaned up and trimmed by visual inspection before fluxes are reported. In the present study, it is reported that only during 34% of the measurement period, there was enough turbulence under the sea ice to allow flux estimates. However, even if there is enough turbulence, EC flux calculations can be compromised because of other reasons (here called "anomalous variations due to sensor malfunction" like shifts in the O2 signal because detritus attachment etc). These data are typically removed based on judgment and expertise of the data collector. In my view, an important challenge for the ECT community is to make this data processing and data cleaning step more quantitative and objective. At least to start with, it would be great of ECT studies would report how many of the total data bursts were removed, because of being judged as "bad data sectors". Would it be possible to specify to report the percentage of data bursts that was rejected even when there was enough turbulence?

We agree with the referee that the evaluation of eddy correlation data can be a challenge. When we first started working with the eddy correlation technique, we tested several automated and more objective ways to separate "bad data" from "good data". However, none of the tested approaches worked to our satisfaction. As a result, we analyze data manually by inspecting each burst, often zooming in on only few seconds of data. Maybe other users of the eddy correlation technique can be more successful in automating this process?

We have re-analyzed all of our data to estimate the percentages of time with insufficient velocity (61 %), with consistent and trustworthy fluxes (26 %), and with periods where turbulence is likely sufficient but fluxes were not extractable due to sensor malfunctions

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(13 %). The latter periods were defined as all erroneous periods that had current velocities greater than 2 cm s-1. We have added this information to the text.

Statistical reporting of results. This study systematically reports the Standard Error (SE) rather than the Standard Deviation (SD). The SD is an index of the variability of the measurements, while the SE is a quality estimate for the mean (how certain are we that the reported sample mean is the true population mean). The ECT induces inherently a large burst-to-burst variation in the fluxes, and hence in the derived estimated of Respiration and NPP. Therefore, in my view, it is more appropriate to report the SD rather than the SE.

Slightly reworded, we agree with the reviewer that SD quantifies the amount an individual measurement will, in a statistical sense, vary from the average, while SE refers to how much a value averaged from multiple measurements will vary from the true mean. We present and use mean values, and thus find it appropriate to show how well they are constrained (SE). Therefore, unless Biogeosciences has a specific preference, we prefer to continue to report SE throughout the manuscript.

Equation (3). Given that this study explicitly constrains the for the contribution of ice melt to the O2 flux, why not explicitly taking this into account in the mass balance (3), i.e., add an extra term for this? This mass balance also assumes that no major respiration and production occurs in the 22 cm between the sensor and the ice (better explain where you assume that respiration and primary production takes place).

The reviewer is correct as it was our original intention to include ice melt as a factor in our O2 mass balance. However, we chose to exclude the O2 flux due to ice melt because (1) it is relatively small, (2) we had limited data from the temperature eddy correlation instrument due to sensor malfunctions, and (3) the inability to determine the exact flux due to a number of assumptions as described in the text. For these reasons, we believe this flux contribution is not robust enough to include in the mass balance model.

We assumed that the majority of the respiration and production occur at the ice-water interface and not in the 22 cm of water between the sensor and the ice. We have clarified this in the text.

Explain the sign convention of the flux: negative flux means O2 transport towards the ice

The sign convention of the flux is now explicitly described in the text and Fig. 4 legend.

Discussion of spectra analysis (P11263). The turbulent cascade stretches from frequency range 0.1 Hz to 1 Hz (Fig 3a). However, all contributions to the O2 flux however occur in the range below 0.1 Hz (i.e. from 0.01 Hz to 0.1 Hz or time scales of 10 to 100 seconds). Are these really turbulent eddies below 0.1 HZ doing the O2 transport, or something else (eg waves)? Typically, the velocity spectrum should be closed on the left hand side (showing the band gap between turbulence and advection). This is not the case here.

Please note that the y-axis in Fig. 3C is linear while a log-scale is used in Fig. 3A and 3B. This may make differences appear larger than they actually are. However, a close inspection of the velocity and O2 spectra (Fig. 3A and 3B) reveals that the inertial subrange stretches from 0.06 - 0.7 Hz and from 0.04 - 0.3 Hz, respectively, while the flux contribution, according the cospectrum, falls in the 0.015 - 0.15 Hz range. This means that roughly half the flux contribution happens at frequencies below the inertial subrange. A similar trend was found in other eddy correlation studies at relatively low mean flow velocities by Lorrai et al. 2010 (Fig. 9) and by Brand et al. 2008 (Fig. 4). As a side remark, wave contributions show up in the cospectrum as a very steep climb within a very narrow frequency band.

The velocity spectrum is likely not closed, or does not go to zero at the lowest frequencies in Fig. 3, because our measuring periods are not long enough to resolve this. Also, the data points at these low frequencies are very sparse, and thus should be interpreted cautiously.

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There is no discussion of what causes the clear trend in heat flux (ice melt) rates over the study period. Correlation with irradiance, water temperature or flow direction? Make a figure similar to fig 5, but now for the heat flux?

We investigated these potential driving variables, but did not see any correlation. We have added this to the text. Due to the lack of correlation we have not added an extra figure.

The calculation of u and z0 is based on a single point measurement (here at 22 cm beneath the ice boundary) and therefore subject to considerable uncertainty. It would be better to measure a velocity profile in the boundary layer with the ADV.*

We agree with the reviewer that some uncertainty is associated with the estimation of u* and z0 and that this may affect the size of the footprint (Fig. 9). Thus, we consider this a first-order calculation that serves primarily as an illustration of the large area that is integrated in eddy correlation measurements. This has been clarified in the text. However, we also note that the footprint was estimated for a very stable period, consisting of 20 consecutive bursts, with a unidirectional and steady current flow of 2.5 cm s-1, which consistently gave the same footprint length as reflected in the small standard error of 1.2 % of the mean (Fig. 9). As a side remark, a new ADV model (the Vectrino II from Nortek) allows vertical velocity profiling to be done along with regular eddy correlation measurements. As the reviewer points out, this will constrain the velocity profile far better.

Figure 2. The stratification should also induce O2 depletion in the boundary layer. Given 2 mmol m-2 d-1 of respiration in the ice, and a 0.2 boundary layer, one would expect to observed a decrease of 10 mmol m-3 d-1. Was this observed by the accompanying Hatch optode?

We have looked again at the O2 data for the 6.5 h period (Fig. 2) where no heat flux was detected. A respiration of 2 mmol m-2 day-1 should for a 0.2 m thick body of water translate to a decrease of 2.7 μmol L-1. However, we do not see this in our data. This may be due to a lack of vertical mixing allowing stratification to build up right below the ice-water interface. As a result the "signal" from this consumption of O2 did not reach the optode positioned 22 cm below the ice over 6.5 h.

I'm not a photosynthesis expert, but I'm amazed that as an algae, you can make a living on 4 umol photons per m-2 d-1.

It is very common below sea ice to have such low light saturation. Light saturation points as low as 0.5 μmol photons m-2 s-1 have been reported for ice algal communities (Cota, 1985).

Specific comments Abstract. Avoid the phrase avoid "amounting to: : :". Some sentences can be shortened. Remove SE's from the abstract. 11256 L5 This study was: : : 11256 L7 revealed low rates of ice melt with a maximum of.. 11256 L8 The O2 flux associated with: : :. 11256 L12 : : :.during 66

We have addressed these suggestions and thank the reviewer for his careful reading.

References: Berg, P., H. Roy and P.L. Wiberg. Eddy correlation flux measurements: The sediment surface area that contributes to the flux. Limnol. Oceanogr. 52: 1672-1684, 2007.

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Interactive comment on Biogeosciences Discuss., 8, 11255, 2011.