Response letter for the editor and Reviewer 1 for the manuscript "Temporal dynamics and environmental controls of carbon dioxide and methane fluxes measured by the eddy covariance method over a boreal river"

Dear Aki Vähä and co-authors,
thank you for submitting a revised manuscript version. It was reviewed again by one reviewer. The reviewer raised only a few, albeit critical, points (e.g. the potential contribution from land fluxes? horizontal transport? clock offset?) which should be carefully addressed in a revised version of the manuscript.

With best regards
Hermann Bange

We would like to thank the editor for the continued interest in the manuscript. We address all the questions raised by the reviewer in this document. The reviewer comments are marked in *italics*. Our replies are directly below each question.

We have not edited the manuscript based on this discussion. If needed, we can add these figures to the supplementary information and refer to them in the manuscript.

With best regards,

The authors

Reviewer 1 remarks

We would like to thank Reviewer 1 for the on-point questions and for improving the scientific quality of the manuscript.

"It's somewhat disappointing that the authors don't want to report gas transfer velocity, as they want to save that for a future paper. But I respect that decision.

I am still not entirely convinced that the filtered fluxes they report are air-water fluxes only, and do not include some contributions from land. This is especially so during the day time when air temperature exceeds water temperature and the atmosphere probably stable. Because air-land vegetation fluxes can be so much bigger per area than air-water fluxes, even a 10% areal contribution by land to the flux footprint can cause a disproportionate bias to the flux. Have the authors looked at momentum and hear transfer from their EC system? Are their heat and drag coefficients roughly what one expects for over water?

The measured stability $\zeta = z/L_{MO}$, where z is the height of measurements and L_{MO} the Monin–Obukhov length estimated from the eddy covariance measurements, was not particularly stable during daytime. The stabilities are shown in Fig. 1 for the cases with accepted fluxes. Stabilities during daytime were mostly between −0.1 and +0.1, which is generally considered near-neutral. In addition, the accepted negative CO_2 fluxes that occurred when the atmosphere above the river was stable accounted for less than 1 % of all the instances with accepted fluxes (Fig. 2). In other words, stable stratification over the river cannot explain the majority of the negative CO_2 fluxes that were observed.

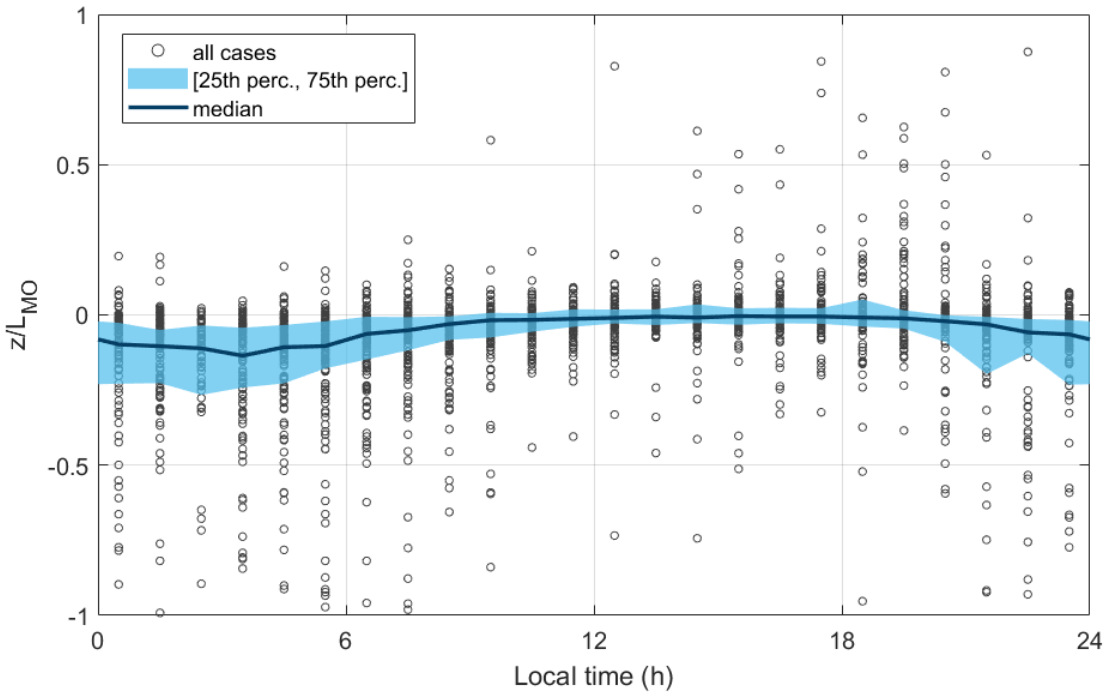

Fig. 1: Stability as a function of time of day for the cases with accepted fluxes. All cases that coincide with the accepted fluxes are plotted with circles. Hourly medians and the area between the 25th and 75th percentiles are plotted with the solid line and shaded area, respectively.

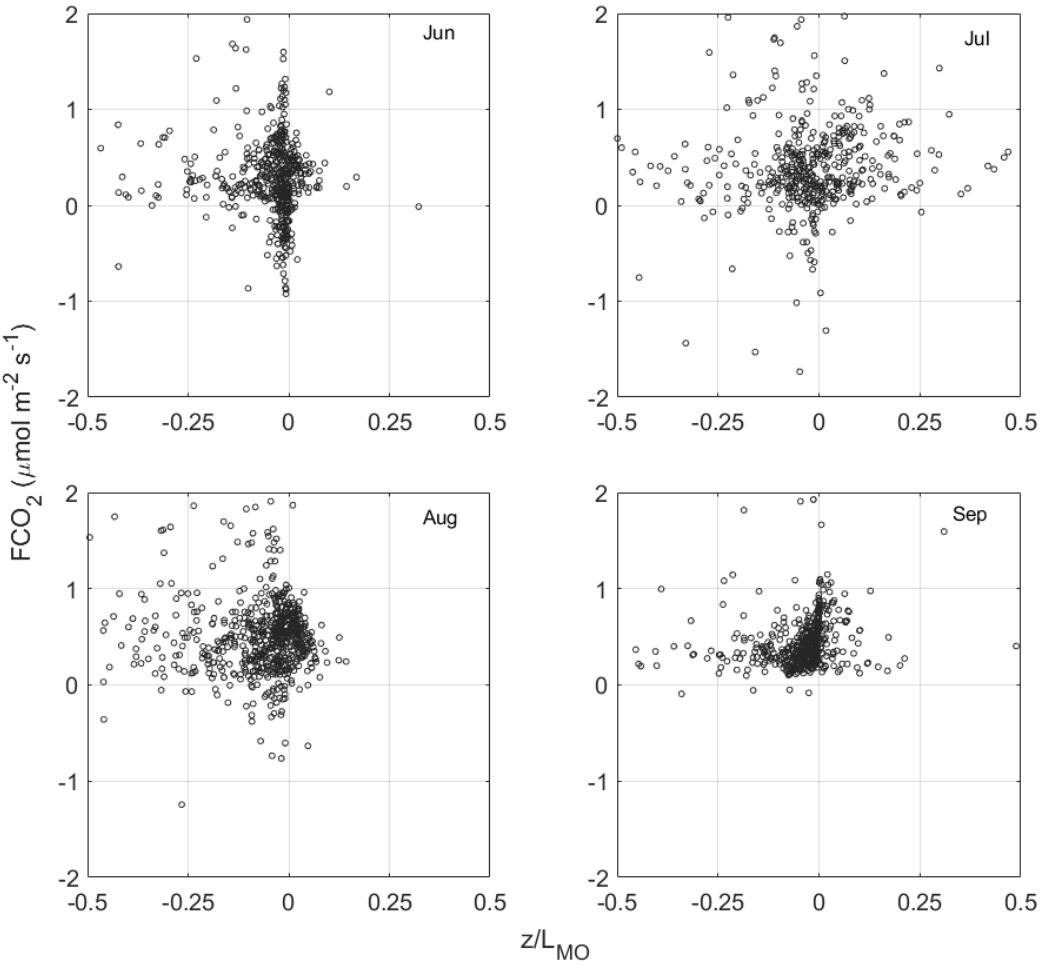

Fig. 2: CO_2 fluxes as a function of stability in different months.

In Fig. 3, the sensible heat flux H is shown against the wind speed multiplied with the temperature difference between the water surface and the atmosphere $(T_w - T_a)U$. Similarly as in the studies by Nordbo et al. (2011) and Mammarella et al. (2015) who studied trace gas fluxes from small lakes with eddy covariance, the points should sit on a straight line for fluxes originating from the same surface. As can be seen from the figure, this is the case. The turbulent heat transfer coefficient can be calculated from the slope of the line as

$$C_H = \frac{H}{c_a \rho_a (T_w - T_a) U} \quad (1)$$

where c_a is the specific heat capacity of air and ρ_a is the atmospheric density. The turbulent heat transfer coefficient is in our case 1.5×10^{-3}, which fits the theoretical value of $C_H \sim 10^{-3}$ for heat exchange over water (e.g. Wei et al. 2016). The slope of our fitted line is 1.87 which is close to those observed by Nordbo et al. (2011) and Mammarella et al. (2015), whose results were 2.23 and 1.97, respectively.

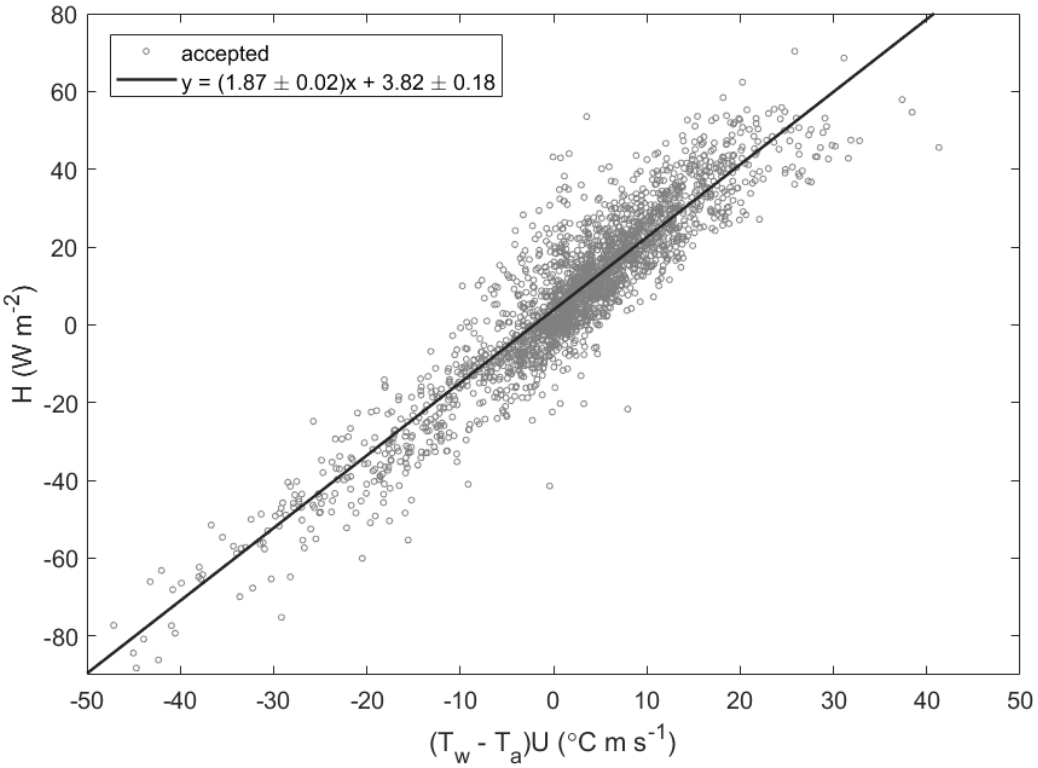

Fig. 3: Sensible heat flux against $(T_w - T_a)U$.

The drag coefficient is calculated as

$$C_D = \left(\frac{u_*}{U}\right)^2 \quad (2)$$

where u_* is the friction velocity and U is the mean 30-minute wind speed. The drag coefficients are shown as a function of U in Fig. 4 for the cases with accepted CO_2 fluxes as well as for the cases where the fluxes were not accepted, i.e., when the wind was blowing from land sectors. We only used here the accepted cases from the southern sector because the platform caused additional turbulence to the measurements when the wind was from the north. The drag coefficient over open water is of the order of 10^{-3} (e.g. Guseva et al. 2023) which we also observe, albeit slightly larger than in open ocean. Still, the drag coefficients for the water sector were smaller than when the wind was coming from the land sectors.

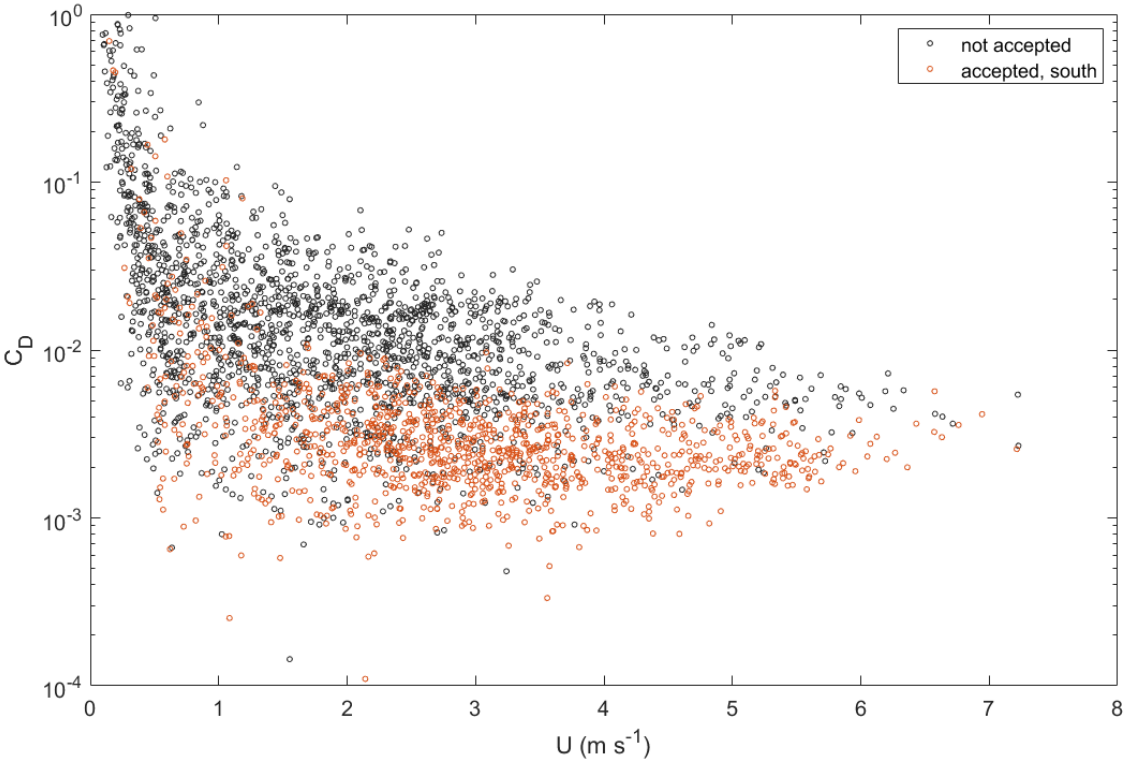

Fig. 4: Drag coefficient against wind speed. Shown here are the drag coefficient from the land sectors ("not accepted") and from the southern wind sector from cases with accepted fluxes.

The low frequency part of CO_2 spectrum looks odd, and suggests possible influence from horizontal transport.

This is due to the large variability in the single spectra. Note that the spectra were chosen from a period with the highest fluxes but still, the fluxes are small in magnitude compared to those from terrestrial ecosystems where the spectra would be clearer. As a consequence, the individual spectra have relatively large scatter.

A few more specific comments:

Zo of 0.01 m is more than an order of magnitude larger than what's typical over the ocean, where waves are much larger.

Following Aubinet et al. (2012) who cite the work by Eugster et al. (2003), the roughness lengths z_0 over small water bodies can be larger than those measured over open ocean. This is caused by increased turbulence due to the vegetation and topography at the shores and that the atmospheric turbulence field over a small water body is not able to equilibrate fully with the water surface over short distances.

The roughness lengths were calculated from the two adjacent sonic anemometers with the measurement heights z_1 = 1.00 m and z_2 = 1.82 m and wind speeds U_1 and U_2. From the logarithmic wind law, assuming neutral stability and that the measured friction velocities are equal – which they are at a sufficient accuracy –, we can solve

$$z_0 = z_2 \left[\exp\left(\frac{U_2}{U_2 - U_1} \times \log\frac{z_2}{z_1}\right)\right]^{-1}. \quad (1)$$

The roughness lengths for cases with accepted fluxes and for the southern wind sector have a median of 0.0145 m and their distribution is shown in Fig. 5.

The larger-than-expected roughness lengths also explain the increase in the drag coefficient from the open-water case in Fig. 4.

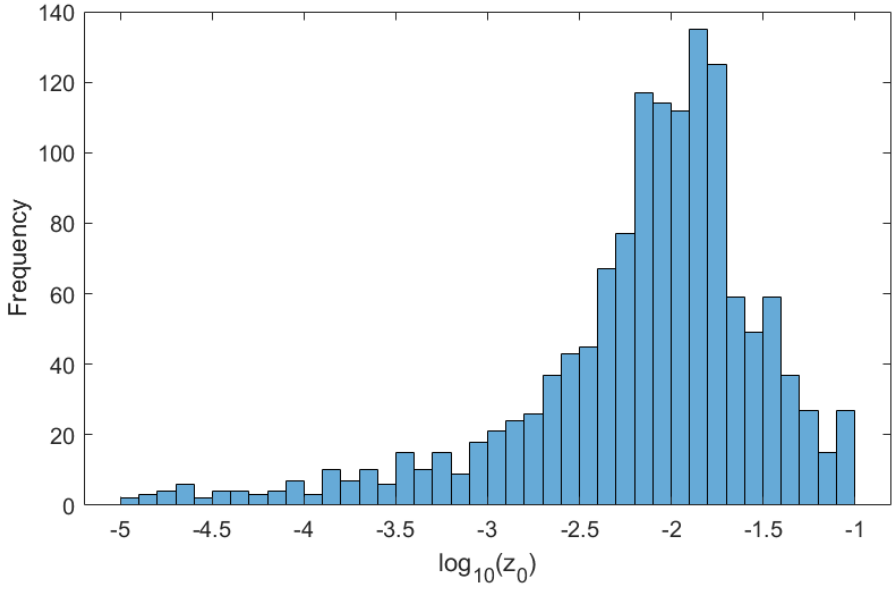

Fig. 5: Distribution of the calculated roughness lengths for cases with accepted fluxes and using only the southern wind sector.

Was 7 s delay for Picarro constant over the entire time? If yes, why not correct for clock offset in the first place? If the delay was drifting over time, was that accounted for?

The delay was constant. A histogram of CH_4 lag times is shown in Fig. 6. The distribution of lag times is centred around 70 data points, i.e. 7.0 s. Indeed, the clock offset could have been taken care of already prior to the flux data analysis and as such, the large time lag is confusing to the reader. However, the lag time determination corrects the timing difference between the sonic anemometer and the gas analyser and a 7 s lag well within a treatable range in the data processing.

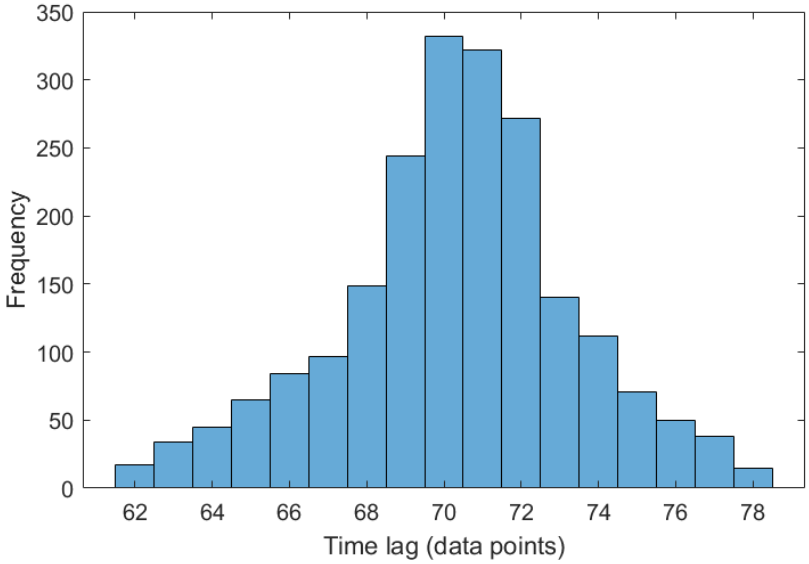

Fig. 6. The distribution of CH_4 lag times in units of data points. The mode corresponds to a lag of 7.0 s.

Fig 9. Uatm, not ppm?"

These two units for dissolved gases are identical because of Dalton's law for ideal gases. We are accustomed to using ppm as the unit. However, the unit was different in Fig. 5c of the manuscript and that has now been changed into ppm.

References

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