

Response to Brian Butterworth

We thank the reviewer for his thorough comments. These comments forced us to investigate our measurement installation even further and revealed important insights. We reply to each comment in the following.

General Comments

RC2.1 The work laid out in this methods paper is thorough and up-to-date, with the current best practices in the field. The researchers designed and clearly described what appears to be a functional eddy covariance tower for measuring CO₂ fluxes in a marine environment. Which is not an easy task. With regard to scientific quality, I believe the authors have done an excellent job.

RC2.2 There are several areas that the paper could be improved. Broadly, the paper needs to stay more focused on CO₂ flux measurements. Several analyses seemed unnecessary (e.g., roughness length and similarity theory), veering away from the overriding goal. These could be replaced by a more in-depth assessment of the performance of the system with regard to CO₂ flux (e.g., gas transfer velocity or bulk flux comparisons).

While these analyses may seem obsolete, we should note that the paper has several aims, one of which is an overall assessment of the feasibility of the new marine flux station. Integral turbulence characteristics and roughness length analyses were carried out to test the fulfillment of theoretical assumptions of eddy covariance method at this particular location. For this aim, these sections provide useful information that we wish to keep in the paper, however moving them to an appendix.

In the revised version, we clarify the aims of the paper in the end of the introduction:

In this paper, we introduce a newly established and currently operating eddy covariance measurement site, located on the Utö Island in the Baltic Sea. This paper has two objectives: 1) Study the characteristics of the new site and measurement setup and 2) Analyze empirically the effect of water vapor on the CO₂ sea-air fluxes.

To improve the focus of the paper, the sections dealing with turbulence and horizontal homogeneity were removed from the main text and presented as appendices. The following sentences were added to the beginning of the Stationarity section:

In order to evaluate the quality of our measurements, several theoretical assumptions of the eddy covariance method were analyzed. The flux footprint area was found to be horizontally homogeneous (Appendix B), and turbulence was well-developed as integral turbulence characteristics could be expressed as functions of stability parameter (Appendix C). In this section, we analyze the fulfillment of the stationarity assumption and determine the effect of CO₂ flux nonstationarity on the CO₂ fluxes.

RC2.3 Also, because the method (i.e., dried closed-path IRGA) has been presented in previous papers, this paper would improve its contribution by delving a little deeper into the challenges facing the system (e.g., distorted spectra). Doing so could enable those deploying similar systems in the future to address those challenges and improve data retention.

We figured out the source of distortion. Please, see the response for RC2.13.

RC2.4 This is a good paper. The work is thorough, the methods are sound. I recommend it for publication and look forward to seeing the science papers that follow.

Specific comments

RC2.5 The paper should attempt some way to corroborate the magnitude of the measured fluxes with previous results. While we have no reason to doubt that the fluxes are good, we also have no evidence that they are good. Accomplishing this should be straightforward given the fact that waterside pCO₂ was measured. This could be done by presenting gas transfer velocity. Or it could be done by comparing against bulk CO₂ flux calculated using an existing parameterization for gas transfer velocity.

Typically, eddy covariance measurements are used for developing parametrizations, not vice versa. However, we compared the k-U relationship determined from our data to the traditional quadratic equation of Wanninkhof (1992):

As no previous measurement data are available for comparison, we validated the magnitude of the measured air-sea CO₂ fluxes roughly by calculating the gas transfer velocity ($k_{660} = F/K_0 \Delta p\text{CO}_2 (Sc/660)^{1/2}$) from our data and compared it with the universal parametrization proposed by Wanninkhof (1992). Schmidt number (Sc) and solubility (K_0) of CO₂ were calculated according to Wanninkhof (1992) and Weiss (1974), respectively. For this comparison, we only included cases in which the absolute partial pressure difference ($\Delta p\text{CO}_2$) between the sea and atmosphere was larger than 3Pa. The gas transfer velocities derived from our measurements were in good accordance with predictions (Fig. 7), which lends credence to our flux measurements.

RC2.6 The percentages of data lost due to non-stationarity (63%) and wind direction (51%) are reported separately. It's worth reporting somewhere in the paper the combined loss / total percentage of data that made it to the final analysis.

We agree that this is important information for the general representativeness of the station. The total percentage of lost data is added to the text:

Overall, 91% of data were discarded due to unsuitable wind direction, nonstationarity or a positive momentum flux.

RC2.7 Page 3 – line 13 –The closed-path design does not automatically mean increased sensitivity to motion. Miller et al. (2010) found that the LI6262 and LI7000 were more sensitive to motion. This doesn't apply to all closed-path IRGAs on the market (e.g., the LI7200 [closed-path], which has the same internal design as a LI7500 [open-path], doesn't show the same degree of motion sensitivity as those named by Miller et al. [2010])

The motion sensitivity problem was clarified:

However, this does not apply to all closed-path analyzers, since some of them have internal design resembling open-path instruments (LI-COR, 2016).

Before ending up with the current measurement system, we tested several years (since October 2012) different instrument set-ups including LI-7500 and LI-7200. After several test installations (and broken instruments due to sea spray, strong winds and wintertime icing) we finally ended up with the current setup.

RC2.8 Page 5 – I like the photograph (Figure 2). It makes the case that the virtual impactor is necessary. But it seems like a photograph that is a little closer to the tower, and that shows the instrumentation a little better,

would be more helpful here (where you're describing the physical design of the system) and that this one could potentially be moved to the Appendix. Just a thought.

The following figure shows the tower instrumentation slightly better. The anemometer is seen on the left side of the boom, and the orange sample line and its inlet are seen at the middle of the boom. Also, the previously used LI-7500 is at the top of the mast.

Before the current inlet location, we tested placing the inlet in more ideal position, but could not get it function due to the sea spray. We are still investigating a technical construction which would tolerate the environmental conditions with minimal flow disturbances.



Fig R1. Top of the flux tower.

RC2.9 Page 6 – How do you generate your purge air for the nafion? And zero gas for the LI7000?

The same sample air coming from the gas analyzer is directed to the outer cell of the drier. Thus, the total pressure difference drives the water vapor removal. This indeed does not remove all water vapour but effectively attenuates the fluctuations, as shown by the H₂O variances. Please see the figure below.

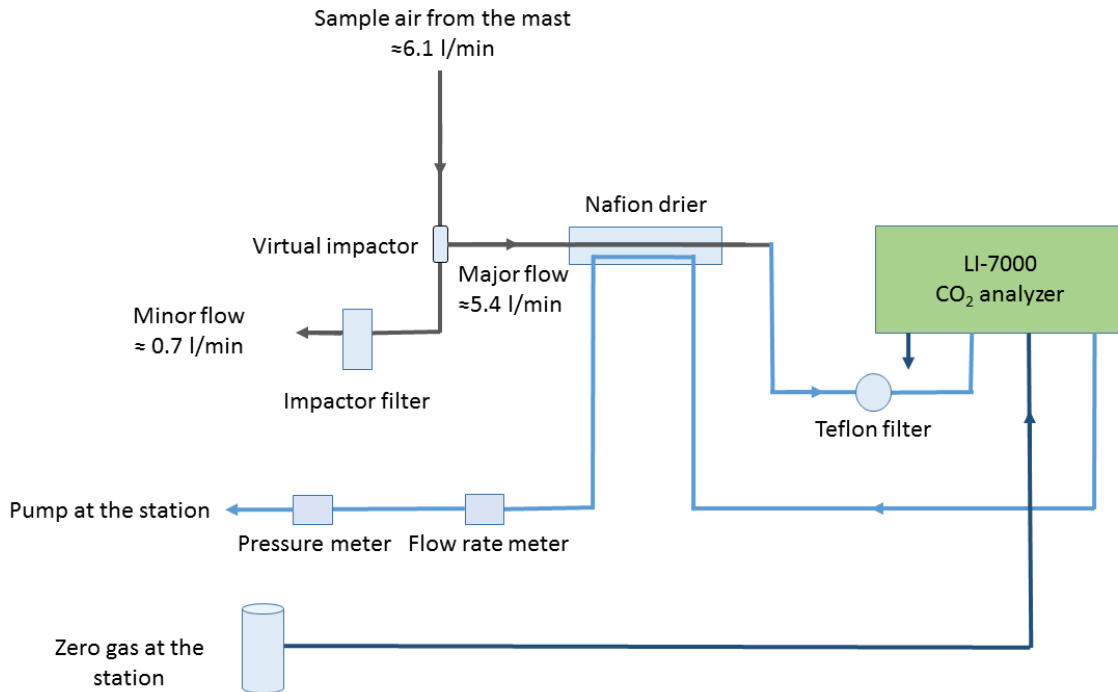


Fig R2. Schematics of the test setup. Dark lines represent steel pipes and light blue lines are Bev-A-line-tubings.

Sentences were added to the page 6 line 9:

The sample air leaving the gas analyzer of the test setup is directed to the purge cell of the drier. Thus, the total pressure difference drives the attenuation of water vapor fluctuations.

Sentences were added to the page 6 line 17:

The same zero gas flows through both analyzers. The actual zero gas bottle (40 l) is located at the station, and a small stream of zero gas flows continuously to the gas analyzers in the instrument hut.

RC2.10 Page 7 – line 14 – Suggest removing the “flow” in “stationary flow conditions”. It’s not purely flow that is involved ($w'c'$).

“Stationary flow conditions” was changed to “stationary conditions”. In addition to the page 7, line 14, this affects also page 1, line 14 and page 7, line 23.

RC2.11 Page 7 – Section 2.3.1. Spectral analysis: Here you’ve applied a limit of $|z/L| < 0.05$. But later, in Figure 8, it appears that about half of the data fall outside of this range. Why did you apply this more stringent criteria? Was there something wrong with the spectra outside this range? Or was it just so the peaks of your spectra lined up more cleanly?

The stringent stratification criterion was chosen so that the peaks of the cospectra would be located at the same frequency range. The spectral analysis procedures were updated:

Observations with an appropriate wind direction (180–260 °) and stationary conditions ($RN_{wc} < 0.3$) were accepted for spectral analysis. 607 half-hour periods met this criterion. Next, we discarded small fluxes ($< 0.1 \mu\text{mol m}^{-2} \text{s}^{-1}$), after which we had 381 half-hour periods. Furthermore, a cospectrum was discarded if its peak was not within a predefined frequency 30 range (0.01–0.5 Hz). 283 cospectra passed this frequency range criterion. Also, a cospectrum was discarded if its normalized peak size ($Co_{wCO_2} / w'CO_2'$) was not within predefined range (0.1–1.0). Thus, 238 observations from a 4 month period were used for the spectral analysis.

RC2.12 Within the more limited selection of 612 observations, over half are discarded because they are ‘distorted.’ I assume this is so you can use the ‘good’ spectra to get a reasonable/ workable transfer function. That makes sense. But then are you applying that transfer function to correct all observations that pass the stationarity and wind direction criteria, even the ones with distorted spectra? The transfer function should not be expected to fix the fluxes for these other intervals.

The spectral analysis was carried out to determine the performance of the measurement system for estimating and correcting for the flux losses due to attenuation of high-frequency fluctuations. This attenuation is specific to the measurement setup and was described by means of the transfer function shown in Eq. 4. For this function, we estimated the half-power frequency, f_0 , which characterizes the overall performance of the measurement system. As the reviewer agrees, it is reasonable to use the best-quality data for this. The distorted spectras obviously cannot be corrected this way and this is not what we try to do. Rather, we use the specified transfer function with the estimated f_0 and convolute this with the generic cospectra of Kaimal et al., which depend on wind speed and stability, to obtain an estimate of the flux loss that needs to be compensated for.

RC2.13 I think this is an area of the paper that can be developed. If the quality control for CO₂ fluxes (shown in Figures 6 and 9) consists only of the wind direction and stationarity criteria, then the distortion of spectra mentioned in this section needs to be addressed. What is causing the 365 of 612 intervals that satisfy $|z/L| < 0.05$ (and presumably some percentage of intervals with z/L outside this range) to have distorted spectra? Is the distortion in the spectra from low or high frequencies? Or a consistent spike? Does it happen under specific environmental conditions? Is there some way to measure and account for the error such distortions likely introduce into the measured flux? The shape of the spectra, especially if they’re consistent, will be useful for indicating what problems may be affecting the tower. Maybe it’s low frequency contribution coming from the residual water vapor, maybe the tower is swaying, maybe there is high frequency noise from the IRGAs. If you can give the readers more information here it will help those who plan to deploy similar systems to address those challenges.

We calculated $u'w'$ cospectra and $u'u'$ and $w'w'$ power spectra and found that these followed expected shapes. This implies that the mast does not disturb the flow. However, the position of our gas inlet close to the base of the anemometer was found to be non-ideal for some wind directions. This placement was chosen to minimize the accumulation of sea spray in the tubings (please also see the answer to comment RC2.8 above). Because of this, the $w'c'$ -cospectra of the northernmost wind directions showed a disturbance in high frequencies that grew with increasing wind speed. Thus, we decided to use only southwestern wind directions in this paper. This is now stated in the beginning of Instrumentation chapter:

In this paper, we utilize data from the open sea sector towards the Baltic Proper (180—260 °). The fetch in the sector 260—340 ° may be limited and, as the anemometer base potentially disturbs the flow, the data from this sector were not included in this study.

RC2.14 Page 9 – Figure 3 – When printed the dashed cyan line on subplots d and e is not visible when it overlaps with the blue line. Might want to consider some way to increase the contrast.

The cyan color was changed to yellow in Fig. 3.

RC2.15 Page 10 – Section 3.1.2 – With respect to z_0 , no one was going to complain that an extended open water surface does not satisfy horizontal homogeneity conditions for EC measurements. Yes, the figure does show that you've successfully selected the right wind direction window. But that seemed obvious just from your map. And you could always just include one sentence that says it was confirmed because z_0 was consistently below 1 mm in that window. There isn't anything wrong with this section, but it is not particularly necessary.

Yes, the result is trivial – this calculation was part of our overall QA/QC and site analysis. The Horizontal homogeneity section was moved to the appendices.

RC2.16 Page 7 – line 3 – Here you've handled the tube delays by taking the maximum covariance. I know this is a common practice. But my experience has been that it often results in the selection of fluxes which have large contributions from frequencies outside the expected range. Have you calculated the expected tube delays based on your known tube lengths, tube diameters, and flow rate? How well do the maximum covariance lags match the expected lag based on this calculation? If you base the tube delay on this calculation do you see improvement in the percentage of spectra that are distorted (page 7 – line 26)? I don't think this necessarily needs to be reported on in the final paper, but it's worth checking.

The maximum absolute covariance is calculated within a predefined lag range, which is selected on the basis of the expected delay. If no maximum is found within this lag window, a default value is used. As the reviewer points out, this is a common practice. However, the default value is derived from the statistics of the calculated lags rather than tube geometry and flow rate.

The setups are complex, especially the one with a drier and a virtual impactor, and the effective tubing volume cannot be evaluated accurately. Using the dimensions of the outer and inner tubings and the measured flow rate, the lag would be 2.6 s, whereas the maximum covariance is found using the lag of 3.0 s on average. The cospectra become clearly disturbed if a non-optimal lag is adopted.

We have checked the cases where the $w'c'$ cospectrum is distorted and tested a wide range of different lags. The distortion is seen with all lags. Assuming the 'expected' lag does not improve the cospectra; it only affects the frequency at which the deviation from the ideal cospectrum appears.

RC2.17 Page 11 – line 2 – What is the average high-frequency correction?

The sentence was changed to:

The average high-frequency correction of the CO₂ flux during the measurement period was 17 % for the test setup and 13 % for the standard setup.

RC2.18 Page 11 – line 15 – How do you handle the range of f_0 (e.g., 0.04 to 0.22) in correcting the attenuation of latent heat flux? Did you calculate the different transfer functions for different relative humidities based on

their f_0 ? Your assertion that the flux attenuation (for RH=80%) is 44% of the real flux would be strengthened by a comparison of your result to an empirically-based model (e.g., COARE [Fairall et al. 2003]).

We plotted f_0 values as a function of RH, and fitted a power law curve on it.

The H₂O flux attenuation is mainly produced by absorption and desorption of water vapor on the surface of the tubings. Tube aging plays a role in the size of this attenuation (Mammarella et al., 2009). In our case, this aging is likely caused by the accumulation of sea salt in the tubings. Thus, this attenuation is setup-specific.

C2.19 Not critical, but since you are using these latent heat fluxes to interpret your CO₂ fluxes it wouldn't hurt to show the CowH₂O cospectra of the undried system.

Yes, this would give a better expression of the size of the H₂O flux attenuation in the tubing in marine environment. We added a median of w'H₂O' cospectra in the Fig. 4.

RC2.20 Page 12 – line 8 – So the reader doesn't have to flip back several pages to Figure 3 it would be helpful here to place a reminder that all the CO₂ fluxes in July were expected to be negative. Maybe insert something like “ (all of which were expected to be negative) ” after “July 2017”.

Added the suggested sentence to page 12 line 8:

all of which were expected to be negative

RC2.21 Page 14 – Section 3.1.5 Turbulence: I think this section is unnecessary. It distracts from the main purpose of the paper (CO₂ fluxes). In reading it felt like a distinct transition to a new subject. One which required more effort than the payoff was worth. I think this section would be better left to its own paper, when the authors have developed it further and have the space to discuss the implications. For this paper, there was no practical application. If there is a reason that this relates to the CO₂ fluxes that you've shown then that reason should be made more clear.

We feel that this section is important for the overall quality assurance of our new measurement setup but to clarify the text, we moved it to the appendix C.

Technical Corrections

RC2.22 Page 3 – line 26 – Word choice on “effortless”. Might want to go with something like more feasible, practical, or straightforward (or conversely ‘not as logistically challenging’).

Agreed, these measurements indeed are not completely effortless. The word was changed to “more feasible”.

RC2.23 Page 3 – line 31 – Grammar: “and the drying of sample is straightforward to implement” to “and allows for straightforward implementation of sample air drying” (or similar)

Changed the sentence to:

and allows straightforward implementation of sample air drying

RC2.24 Page 5 – Citations at the ends of each paragraph should be moved inside the period.

The citations regarded the whole paragraphs.

RC2.25 Page 5 – line 17 – Change “A : : : tower is placed” to “A : : : tower was placed”

The grammatical tense was fixed.

RC2.26 Page 7 – line 6 – “was considered” to “were considered”

Done.

RC2.27 Page 7 – line 23 – “based on the criterion that $RN_{w'c} < 0.3$ ” would read better if it were enclosed in parentheses rather than commas. This would match the way you’ve presented the wind direction criterion.

True. Changed the sentence to:

Observations with an appropriate wind direction (180—340°) and stationary conditions ($RN_{w'c} < 0.3$) were accepted for spectral analysis.

RC2.28 Page 8 – line 3 – Consider reorganizing the sentence that begins with “To correct for: : :”. It is difficult to read.

The sentences were modified:

To correct for the high-frequency attenuation of fluxes, the universal Co_{wT} equations reformulated by Horst (1997) from those originally presented by Kaimal et al. (1972) and Kaimal and Finnigan (1994) were used as a reference. As the shape of this universal cospectrum depends on stability and wind speed, the attenuation for each 30 min flux was calculated as a function of these meteorological parameters.

RC2.29 Page 8 – line 26 – “seasons of carbon cycle” to “seasons of the carbon cycle”

Done.

RC2.30 Page 10 – line 13 – “Especially, a large swell : : :” is an incomplete sentence.

The sentence was removed.

RC2.31 Page 14 – Figure 8 – “f” in the xlabel of subplot b should be “fc” for consistency with how it’s written in the text.

Done.

RC2.32 Page 16 – line 25 – “the difference between the dried and undried sea-air CO₂ flux measurements were very similar” needs to either become “the difference between the dried and undried sea-air CO₂ flux measurements was small” or “the difference between the dried and undried sea-air CO₂ flux measurements were very similar.”

Page 16 line 25 was changed to:

the difference between the dried and undried sea-air CO₂ flux measurements was very small

RC2.33 Also, the second part of this sentence doesn’t work as it’s written because the subject of the sentence changes.

The sentence was broken down to two sentences:

This also proves that a Nafion drier (in our case combined with a virtual impactor) does not disturb the CO₂ flux measurements.

RC2.34 Page 18 – Figure 11 caption – move “(standard setup – test setup)” to immediately after “CO₂ flux difference”

Done.

References:

Mammarella, I., S. Launiainen, T. Gronholm, P. Keronen, J. Pumpanen, Ü. Rannik and T. Vesala. Relative humidity effect on high-frequency attenuation of water vapor flux measured by closed-path eddy covariance system. *Journal Of Atmospheric And Oceanic Technology* 26:1856-1866. 2009.

Wanninkhof, R. (1992), Relationship between wind speed and gas exchange over the ocean, *J. Geophys. Res.*, 97(C5), 7373–7382, doi: 10.1029/92JC00188.