Response to Gil Bohrer

We thank the reviewer for his comments. The comments have led us to examine our measurements in more detail. We reply to each comment in the following.

RC1.1 The manuscript provides a very technical assessment of the effects of a drier and a compactor on removing the spectral cross contamination of water vapor in co2 flux measurements. It is relevant, and reports interesting findings with direct implications to observation techniques of ocean co2 fluxes.

RC1.2 Given the topic, I found the narrative diverging to describing other unrelated findings (turbulence, gap filling, and roughness length). These sections should be removed, or, the direct relevance of these findings to understanding the differences between the measurement setups should be described.

In addition to the analysis of H_2O interference effects on the CO_2 fluxes, a main aim of this paper is to introduce a new eddy covariance site and study its overall feasibility for sea-air flux measurements. 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 CO_2 sea-air fluxes.*

Integral turbulence characteristics and roughness length within the footprint were analyzed in order to address the quality of the fluxes measured at the new site. These sections are important for the overall quality assurance of the new setup, so we did not remove this material from the paper. However, to clarify the paper, we moved them to the appendix.

The brief discussion of gap filling was moved to the conclusions to highlight the fact that gaps are evident in sea-air CO_2 flux measurements and that auxiliary measurements (notably dissolved CO_2) are required for the gap filling for CO_2 budgets, which is one of the primary goals of these measurements.

RC1.3 Otherwise, I only have a few minor comments: p3L15-20, I think sea breeze is worth mentioning here. Also, you should mention specifically that sea-land gradient is almost always present and violates the horizontal homogeneity assumption. Please reference Rey Sanchez et al 2017 https://www.tandfonline.com/doi/abs/10.1080/20964129.2017.1392830 which evaluated the relative effect of horizontal affection.

The presence of sea-land gradient and its effect on horizontal homogeneity are now mentioned in the introduction:

> *Horizontal CO_2 gradients develop easily in the vicinity of sea-land boundary, violating the assumptions of horizontal homogeneity and negligible advection (Rey-Sánchez et al., 2017).*

As Utö is a small island (0.81 km^2) and its distance to the mainland is more than 50 km, the influence of sea breeze alone is small.

RC1.4 Fig 1- please say explicitly where is the ec flux station. I assume it is A, but a bit confused about what B is.

The caption of Fig. 1 was clarified:

The research installations on the island consist of the Utö Atmospheric and Marine Research Station and its flux tower (A), the inlet of the flow-through pumping system (B), the Atmospheric ICOS station (C), and a weather and air quality station (D).

RC1.5 The location of the tower at the edge of a cliff is problematic. The sharp change of roughness and the physical disturbance to the flow probably generate increased turbulence and a vertical ejection flow that violate a few of the ec assumptions (0 mean vertical flow, ergodicity of turbulence). There are many papers discussing the effects of forward facing step on vertical flow. See for example our paper https://link.springer.com/article/10.1007%2Fs10546-014-9923-5 and references within. Roughness and surface heat flux transition create circulations patterns that are particularly problematic for edge-of-shore flux measurements. See Higgins et al 2013 https://journals.ametsoc.org/doi/full/10.1175/JHM-D-12-0166.1 and Kenny at al 2017 https://link.springer.com/article/10.1007/s10546-017-0268-8. These will be a problem even when the footprint is all water. Please at least discuss this issue, around where you discuss other difficulties of measuring carbon flux in the ocean.

The authors are also concerned about this problem. However, the observed vertical wind speed indicates that this is unlikely to constitute a serious problem; please see the following reply. A discussion about the possible secondary circulation due to the cliff was added to the revised version:

Because an obstacle in a flow field generates an uplift zone on the windward side of an obstacle (Chatziefstratiou et al., 2014), there should not be steep elevation changes near the flux tower in order to avoid non-zero vertical wind speed. Also the roughness and flux discontinuities on a sea-land boundary generate secondary circulation that drives advection and causes challenges for coordinate rotation (Kenny et al., 2017).

RC1.6 P7L15 in the same issue of vertical flow - how did you rotate your wind coordinates? Most rotations assume 0 vertical wind. That will affect your momentum flux. In your case with the cliff facing the wind, you should filter out cases where the unrotated mean w was large.

A double coordination rotation was used to ensure a zero mean vertical velocity for each 30-min averaging period. During the whole measurement period, the unrotated vertical velocity varied within 0-1 m/s with a mean of 0.39 m/s and standard deviation of 0.21 m/s. The vertical rotation angle within the sea sector varied within 0-5^{o} with a mean of 3.1^{o} and a standard deviation of 1.4^{o}, indicating that the flow divergence due to the cliff is limited.

RC1.7 P7L26 you cannot remove more than half of the observations based on a subjective eyeball analysis for "distortion". Please provide an objective definition for which observations should be removed and remove only and all of those that fit these criteria.

We should note first that this screening only refers to the data used for spectral analysis; it was not used as a quality criterion for flux data. The spectral analysis was carried out mainly to estimate the high-frequency attenuation in terms of the transfer function specified. For the revised version of the manuscript, we adopted objective criteria for removing poorly structured cospectra:

Observations with an appropriate wind direction (180–260 ^{o}) 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 µmol m^{-2} s^{-1}), after which we had 381 half-hour periods. Furthermore, a cospectrum was discarded if its peak was not within a predefined frequency range (0.01–0.5Hz). 283 cospectra passed

> *this frequency range criterion. Also, a cospectrum was discarded if its normalized peak size (Co_{wCO2} / $w'CO_2'$) was not within predefined range (0.1–1.0). Thus, 238 observations from a 4 month period were used for the spectral analysis.*

RC1.8 P12L5 The stationarity is an environmental property and should not vary between instruments. I do not understand why you have different stationary cases for each of your setups which are at the same location. That can only be the outcome of different observation errors in each of the setups. I suggest adopting a common criterion, when both sensors observe stationary conditions.

Yes, the difference in measured CO_2 flux relative non-stationarity values originates from observation errors. We agree that it is more logical to adopt a common criterion and thus we now require that stationarity is observed by both setups. This had only a minor effect on the results.

A sentence was added to the Materials and methods chapter (page 6 line 11):

> *Here, a 30 min period is regarded as stationary if both of the CO_2 flux measurements fulfil the condition $RN_{wc} < 0.3$.*

RC1.9 P13L19 “co2 flux station" seems odd. Maybe “co2 flux exchange at the station". This paper is not about carbon budgets. I suggest removing this section. If you insist to keep it, please provide more information on the gap filling approach you used, how you estimated uncertainty, and the resulting co2 flux budget.

Changed the sentence to:

> *Thus, continuous measurements of surface seawater and atmospheric CO_2 concentration at the Utö station provide useful additional data for the calculation of sea-air CO_2 flux exchange.*

The gap filling text was moved to the conclusions. Please see the reply for RC1.2.