

The study conducted by Granville and Berg examined air-sea gas (oxygen (O_2) and carbon dioxide (CO_2)) exchange in a shallow seagrass meadow in the South Bay. They compared wind speeds measured at different locations, as well as those derived from reanalysis data, which are commonly used in gas transfer velocity (k) parameterizations. They measured air-sea O_2 flux using the upside-down aquatic eddy covariance (UAEC) to determine air-sea O_2 flux and k . They also evaluated appropriate k parameterization in the environment. The authors applied the best-fitting k parameterization to calculate air-sea CO_2 flux. They found that offshore wind measurement site and reanalysis data were similar to the wind speed at observation site. They also found significant differences in parameterized k , especially at lower wind speeds, which is often observed at nearshore regions. They also showed that air-sea CO_2 flux is strongly affected by the storms.

This paper shows the concerns about using wind speed data at measured at different conditions, applying k parameterizations without considering environmental factors, and neglecting the effects of storms on biogeochemistry in seagrass meadows, with observational evidences. Since this ecosystem is playing an important role in biogeochemistry yet the existing studies are not enough, this study is important to be published.

Major comments:

Line 167:

Why is the data period so short (4 hour) for determining k ? I think usual eddy covariance paper has more data than 4 hours. Since authors points out the short period of observation as a disadvantage of floating chamber in Introduction (Line 47), I assumed that UAEC can provide longer observation period. Authors mentioned that the 4-hour period on August 26 had consistently low wind speeds ($\leq 4.5 \text{ m s}^{-1}$). Rather than requiring the wind speed to remain consistently below this threshold, how about using all 15-min data points with wind speeds $\leq 4.5 \text{ m s}^{-1}$?

Methods section:

I am concerned with the fact that there is no description about calculation of uncertainty in the derived k_{600} . There is no uncertainty in figure as well (Fig. 6a). When authors try to validate parameterizations by comparing with the observed k , authors need to take the measurement uncertainty into consideration.

Line 240:

It would be better to show the wind direction as well, since authors point out that wind

speed is reduced when wind blow over the land. The relationship between wind direction, measurement-site locations, and nearby land can be discussed in the Discussion section.

Minor comments:

Line 47:

I agree that floating chamber can directly measure air-sea gas flux. However, I do not think “cavity ring-down spectrometry” can measure air-sea gas flux directly. I think it can be used to measure partial pressure of a gas. Since partial pressure of a gas is combined with k to determine air-sea gas flux, this method does not measure air-sea gas flux directly.

Line 52:

It would be better to explain the advantages and disadvantages of UAEC more. Can it provide long-term measurements? Can it measure at a high temporal resolution? Is the system cheap to buy and deploy?

Line 54:

I think the phrase “As a result,” is not needed.

Line 74:

Since authors already defined k as gas transfer velocity, authors do not have to write “gas transfer velocity” again.

Line 124:

The final station, “Hog” Island, is located..... Figure 1 caption writes it is Hog station, while Line 139 write it is Hog Island station. They should be consistent.

Line 133:

How is roughness coefficient of 0.002 m determined? If there is any citation, it should be explicitly written.

Line 136 through Line 141

The instruments used to measure wind speed should be described.

Section starting Line 150:

How did the authors calculate the uncertainty in k_{600} ? The method used to calculate the uncertainty in k_{600} should be described in this section.

Line 173:

I think equation (3) and (4) are basically same. They should be generalized and showed as one equation.

Line 175:

Sc is the kinematic viscosity of water divided by the diffusion coefficient of a gas. So I am confused why solubility is involved in its calculation.

Line 190: It should be explicitly written that only 4 hours data was used. Currently, “during August 2021” implies that there is data for 1 month.

Line 271:

I recommend presenting the observed k first, followed by the estimated k, because the observational data are more valuable than the estimated values. Currently, the observed k are presented in Section 3.2.3. I recommend moving the relevant sentences to Section 3.2.2.

Line 310:

I thought that (Ho et al., 2011, 2016) are not appropriate citation. They might use rRMSE, but I thought they did not develop rRMSE. I think citation is not needed here.

Line 322 and elsewhere: p_{CO_2} should be pCO_2 . Also, I think pco2 is not concentration. its unit is atmospheric pressure.

Line 337:

I thought that “mean $\pm$ standard deviation” is not needed since authors mentioned it at Line 232.

Line 377:

I think the authors do not necessarily need to cite a previous study. They can estimate the potential bias themselves by examining how the parameterized k changes when the wind speed is increased by 1 m s^{-1} .

Line 390:

I thought that the corresponding figure should be Figure 5 and 6, not 6 and 7.

Line 413:

Figure 6 does not provide plot for $u_{10} > 5 \text{ m s}^{-1}$, so I guess the corresponding should be different figure. Looks like the figure number is sometimes not appropriate hereafter. Please check them.

Line 436:

“during peak seagrass density,” -> “during the period of peak seagrass density,”

Line 450:

kco2 is not only affected by wind, but also by temperature through Sc calculation. Was there any effects from the temperature?

Table 1:

Authors need to to explicitly write that the data period was 4 hours, not “during the August 2021”.

The parameterization of Ho et al. (2016) involves “w”. Is it “v”?

Figure1:

The two figures are not clear. Please use more high resolution map, especially the right bottom figure. Is “RPLV” in the figure “RPLV2”?

Also, I recommend adding the locations for which ERA5 data were used.

Fig. 4a:

It looks like there are small local maxima in the wind-k parameterizations, such as M04 and W09 at wind speed of 6 m s^{-1} , which seems unusual to me. The wind-k parameterizations are generally smooth, continuous curves without such jumps (see, for example, Figure 6 of Wanninkhof et al. (2009)). Could authors please check the figure again?

Figure 4b:

Please specifically write what the black dots and gray lines are, in the figure caption. The figure caption writes that this figure shows “The relative standard deviation of the mean k600 value across measured U10”. Therefore, I thought that the gray line is the relative standard deviation. However, relative standard deviation should not have unit, which contradict with the y axis saying “ cm h^{-1} ”.

Figure 5:

It would be better to add error bars in measurements.

Also, why does the x axis range from 27 and 30 with units of Time (h)? I think time (h) should range from 0 to 24.

Figure 6a:

Looks like there is no errorbar in observed k600, so the errorbar should be added.

Figure 6b: The figure caption writes that “The mean and relative standard deviation of the k600”, but I am confused since relative standard deviation should not have units and y axis says the unit is “cm h⁻¹”.