GENERAL COMMENTS:

The submitted manuscript of Dobashi and T. Ho under review for journal EGUsphere presents a gas transfer velocity determination using the dual tracer technique during a six days spring experiment over a seagrass ecosystem in south Florida. Controlling factors are examined and comparisons, conclusions and implications are drawn on gas transfer velocity parametrizations and air-sea CO2 flux estimations over seagrass ecosystems.

The present study shows interesting analysis and computations on gas transfer velocity parameterizations over a seagrass meadow that will definitely give insights for the scientific community working and coastal carbon fluxes and processes. Nevertheless, as it stands, the submitted manuscript suffers from a lack of information (M&M section with in situ measurements and computations) and organization (especially the result and discussion section) which make difficult to follow and understand the approach and associated results (data set link is not mentioned in the MS sections, only at the end, maybe it can help but the link doesn't work). For instance, seagrass distribution/dynamic and other (than wind, temperature gradient) abiotic environmental parameters should be better described and linked to found gas transfer velocity parametrization. Also, some conclusions should be dampened a bit or reformulated particularly when authors compared their K600 parameterization obtained from a six days dataset in one season, to other different coastal ecosystems cited in the literature.

In this way, please see the specific comments below to help in the revision of the different sections and the overall manuscript. I would recommend further revisions to allow the publication of the present paper of Dobashi and T. Ho for the journal EGUsphere.

SPECIFIC COMMENTS:

Title:

It should be specified since very general in the present form.

Abstract:

- l.11-12: the general comparison between the present study and other coastal/open oceans is delicate since other environmental factors (than wind speeds) can be involved and controlled k.

- l.13-14 and 16-17: I would dampen this conclusion due to the limited data set and specific studied meadow compared to other seagrass systems worldwide (temperate, subtidal versus intertidal, depth, current, turbidity, rainfall, heat fluxes….).

- l.14: other settings? Please specify.

1 Introduction:

- l.21: an important part of seagrass above biomass (refractory matter) is also exported and does not sink to the bottom.

- l.22: Please update the reference adding other more recent works on carbon seagrass storage.

- l.25: high methane emissions from seagrass meadow have also been recently shown (see Schorn et al. 2021, PNAS 2022 Vol. 119 No. 9 e2106628119).

- l.26-27: the link with seagrass here is not clear, please specify.

- l.33: go further citing other involved parameters (and associated studies) in turbulence control. Also, it lacks in the introduction a review on existing works on gas transfer velocity determination according to (i) coastal ecosystem typology (lagoon, coastal ocean, estuary, seagrass presence or absence, ….) and (ii) methodology, for instance k can also be determined through simultaneous sea-air CO2 fluxes using floating chamber, Eddy Covariance techniques and spatial water pCO2, before focusing on the Florida Bay as done in the next paragraph.

- l.37: please be more specific citing for these studies the used methodology (k, atmospheric CO2 exchanges) among wind speed/gas parametrizations and ecosystem typology as well (presence of seagrasses?).

2 Material and methods:

- l.49: could authors give an idea of seagrass densities in the 2000 km2 and in the specific studied area (Fig. 1). As it stands there is not enough information on spatial seagrass distribution, phenology (carbon stocks and fluxes, …) and concrete relationships done between seagrasses and k parametrization.

- l.60-65: figure caption should be specified and better linked do M&M following sections; it is in between in the submitted MS with cited measured parameters without information on how (sensors, frequencies, …) and when (duration) it was measured. The second zoomed figure should be even more restricted to better see the sampled area. In consequence, we are a bit lost when results and discussion section comes, i.e. parameter origins (sensor used, where, when, frequencies, duration, …).

- 2.2.: when and how long were tracers injected? Why?

- 2.3.: How many? Where (specify it in Fig. 1)? When?

- 2.4., l.89-90: where? When? How? L.92: black square in Fig. 1, how far form the site and why did authors use additional wind data? L.96-97: a given range for Z0 is given without any explanations on which ecosystem typology in the Florida Bay and how it was measured, please specify. Is the used average Z0 value enough precise for calculations? Maybe I did miss something but why Amorocho and DeVries (1980) equation was not used to compute wind speed data at 10 m?

- 2.5., l.100: pCO2 measurements, used frequency? Where? L.106: “specific times”, “at regular time intervals” could you specify it? L.107: CO2 standard concentration?

- 2.7., l.158-159: the last sentence is a little bit awkward, in section 2.7 important testing was done to fit in a precise way coefficient Schmidt numbers especially according to temperature,

so why was it not done for salinity in a same way? L150-151: various salinity, you mean 0 and 35, don't you? Various temperatures from 0 to 40°C, could you specify (step, number of values?)? By the way, what are salinity range, mean values in Florida Bay in general and at your sampling area during the study? No result on it in the submitted MS?

3 Results and discussion:

- 3.1, l.183: how many k600 values were taken into account to obtain this average value? That information is lacking in the M&M section and in turn this result value is unclear and one might wonder if this result is comparable to other k600 values found in the bibliography (Fig. 2).

- Fig. 2: five stations were sampled according to M&M l.173. Four observations appear in Fig. 2, could author explain (is it 4 or 5 or anything else)?

- l.200-201: what did authors mean by "this parametrization" and "K600 between 2015 and 2019"? It is not clear. Could one have further elements or descriptions (sites, measurements, …) at minima instead of only having the two above references?

- l.202-203: and using old parametrizations, could we also have those values or at least element of comparisons (%, …)?

- l.205: I think Results and discussion section should be reorganized, presenting and fully describing first (which has not been done yet) temporal series of measured environmental parameters (grouping Figs. 4 and 6 or at least water pCO2 measurements for instance, Fig. 5) and then K600 descriptions and comparisons with plots (Figs. 2, 3) and tables and relevant controlling environmental factors on k600. As it stands, it is not possible for the reader to see how the ecosystem functioned during these six days experiments before understanding K600 calculations with controlling factors.

- l.207-208-209: idem, wind speed, air-sea temperature gradients, tidal amplitude are very briefly presented here in a K600 parametrization paragraph so it is hard to follow. Again, environmental parameters should be presented first before K600. Sub-sections with clear titles in 3.1 section would clearly help as well.

- Table 3: I don't understand well, parametrizations presented in Table 2 were applied to the same datasets (?) over the same area (?) each year between 2015 and 2019 as the present study. It is too bad because, it is not explained by the authors in a clear way in the M&M to help the reader to follow and appreciate measurements and the approach done in the present study. It should be done in the revised version.

If I understand well, the K600 equation obtained from the six days tracer experiment is then applied for each year between 2015 and 2019, am I correct? Which (stations, frequency, sensor, etc…cf. M&M comments above) wind measurements were used for these calculations? How other environmental parameters varied during each year? Variations in K600 values (min-max, …) should be presented and described along other environmental parameters variations.

- l.227: again "in four periods", nothing is explained on this choice by the authors….? Why?

- l.233-237: wind, limited fetch are potential explanations for weak K values indeed, what about other environmental factors such as turbidity, current speeds, depths, rainfall events, heat fluxes…? Authors should discuss this as well as seasonal abiotic and biotic (seagrass growth, phenology, algae, …) effects on gas transfer velocities in Florida Bay since K600 equation presented in the study was obtained during one punctual Spring experiment. Those elements should be discussed in this results and discussion section to go further.

- Fig. 4: see above comments, environmental parameter chronologies should be better described in the MS and linked to K600 analysis after. Here, there are six observation points?

- Fig. 5: it should be modified (graphs a to d separated from graphs e and f) and presented in a clearer and more homogeneous way along the text, there is everything in this figure, similarly to Fig. 4 that should be modified as well (graphs a separated from graphs b, c, d). Graph presentations and associations for each figure should be modified in the revised version.

- Fig. 6: idem and wind speed and K600 values should also be added.

- l.250 (3.2): as written l.255, authors should emphasize this aspect of further tracer or simultaneous air-sea CO2 fluxes and water pCO2 measurements to get more precise k600 parametrizations over seagrasses since (i) dataset here is short (six punctual days) and (ii) relationships between k600 determination with seagrass dynamic (density, phenology, …along with previous works in the area) are not enough shown in the present MS.

- l.256: how many air and water pCO2 values were used or measured? (cf. M&M section, information lacking).

- l.259: what about nighttime period, are there available measurements (pCO2, flux, …) from previous works? It should be discussed. L263-264, assumptions are too speculative and authors should not go too far in their conclusions. Oversaturation periods (respiration, calcification) at night probably exist at their sampling site and additional simultaneous measurements of water/air pCO2 and associated fluxes should be done to draw more precise conclusions (among cited references).

- It could be interesting to better mention in this section other K600 determinations and associated studies among tracer experiments such as floating chamber and particularly atmospheric Eddy Covariance techniques for air-water CO2 flux measurements with simultaneous water pCO2 measurements.

- l.271: cyanobacteria bloom seasonality, what about seagrass as it is the main objective of the paper focusing on seagrass ecosystem?

-l.280: the last sentence is not well formulated and should be modified instead calculating CO2 fluxes from Van Dam et al. 2021 with the 4.5 cm h-1 averaged value authors got in this study and analyzing the difference between both values.

4 Summary:

- l.284-285: again, authors should dampen their conclusion when they compare (“overpredict” word used) obtained K600 values with other from bibliography since (i) they got it over few days in one particular season, (ii) other parametrizations were obtained in very different (and so not comparable) coastal ecosystems (open ocean, rivers, estuaries) and (iii) relationships

with seagrass dynamic and distribution and other environmental parameters are not fully described in the present study.