

General Comments:

This contribution from Granville and Berg covers a highly relevant and timely topic of air-sea gas exchange in seagrass ecosystems. The authors combined underwater EC with other in-situ data sources, and external data products to investigate the topic. While an impressive dataset is presented, and a number of relevant questions are discussed, I do not see a clear argument presented in the manuscript, either related to a new concept or the questioning of existing ones. Instead, the paper touches briefly on several topics, including k parameterization, seagrass ecosystem CO_2 fluxes and their drivers, as well as different wind speed products and their impacts. The cost of this broad spread is that there is limited quantitative effort placed on any of these subjects, as well as a lack of uncertainty assessments. Again, I believe the topic is relevant and the dataset of high quality, but would suggest the authors rethink their analytical framework and potentially focus on a subset of the topics. More detailed comments follow:

Technical comments:

I also have two related thoughts about the application of O_2 fluxes to derive a k for CO_2 . The first, and to me more pressing of the two, is related to chemical enhancement, which contributes to increased CO_2 transfer especially at high pH, but importantly does not exist for O_2 . The problem is that the calibration window used to parameterize Eq 3 was in the nighttime, when chemical enhancement was relatively low. As the daytime pCO_2 lows (close to $200 \mu\text{atm}$) were likely also accompanied with a high pH, this would mean a relatively high $k_{\text{CO}_2}/k_{\text{O}_2}$ that is not captured by Eq 3. Chemical enhancement of k_{CO_2} can be on the order of a few cm/hr , which is especially relevant during periods of low wind speed which were quite common for all of the sites for the first half of the study.

I also don't believe that the Sc scaling captures well the difference in gas transfer between CO_2 and O_2 during breaking wave conditions (which I assume you had, at least during the storm). Basically, my concern is based on the understanding that bubble-driven transfer contributes proportionally more for O_2 than for CO_2 (Woolf 1993; Asher and Wanninkhof 1998). This presents a problem, as the period used for calibrating the $\text{O}_2 \rightarrow \text{CO}_2$ conversion was calm, and I assume without breaking waves. My impression is that the conversion from O_2 to CO_2 as formulated in Eq 3 would only really be valid when sea state conditions are similar to the calibration window.

Taking these two points together into consideration, the approach used here to convert from k_{O_2} to k_{CO_2} would likely underestimate k_{CO_2} at low wind speed, and overestimate at high wind. A few ideas on how to improve this: one approach would be simply to only apply Eq 3 for wind speed within the range of the calibration window, and not to extrapolate beyond

that. Or, the authors could apply a range in plausible n exponents in Eq3, and use these to give a uncertainty bound for calculated k_{CO_2} . Another possibility would be to resolve this with a discussion in the text.

Line by line comments:

30: Please consider rephrasing the first sentence. CVEs don't transport terrestrial C to the ocean, but rather intercept it on their way. More broadly, since this study focuses on inorganic carbon, so I would suggest to support this opening paragraph with some statements linking OC storage and air-water CO₂ flux in CVEs.

35: The uncertainty in GHG fluxes does not directly affect Corg stock variability. Please rephrase.

47: I don't think Ollivier et al 2022 measured air sea flux directly. They just used CRDS to measure the air-water CO₂ gradient. There's also the gradient flux approach, although I can't recall a paper using it in seagrass meadows.

Figure 1: I have trouble placing where exactly the Delmarva peninsula is in broader regional context. Maybe it would be easier to see if the inlet map was zoomed into the eastern seaboard, rather than zoomed out to all of north America.

109-114: Not sure you need such detailed description of the Corg stocks here. If you need to trim length, this would be a good start.

144: It might be nice to see where exactly this ERA5 grid cell is on the map in figure 1. Does it overlap with land at all, and are the field sites within its bounds?

167: Would it be useful to say here why it only measured quality fluxes <4.5 m/s?

Equation 3: See comments above about conversion of k_{O_2} to k_{CO_2} . Basically, I'm concerned about the application of k calculated for a non-reactive gas (O₂) being used to derive k for a reactive gas (CO₂).

222: this is only the manufacturer stated precision/accuracy of the sensor, right? Still, this means an uncertainty of ca 40 ppm, which is ca 10% of the mean pCO₂, and 20% of the typical daily low around 200. I feel it might be useful to quantify the impact of this analytical uncertainty on derived CO₂ fluxes, and compare to the uncertainty coming from k parameterization.

224: How were DIC measurements used to validate (or was it to calibrate) pCO₂ from the sensor?

225: Does the NOAA carbon tracker provide locally-downscaled CO₂ values, or is this a regional or national average? Please state.

263: I don't think a "relative SD" would be the best comparison here. The absolute magnitude differences across k models is by definition *larger* at higher wind speed, but the relative difference is larger at low wind speed. The way the comparison is reported here is misleading, because it is generally the difference in k models at high wind that has the largest effect on flux parameterization.

Figure 4b: I'm confused by the reported grey SD bars in the U10 vs k600 figure. Why do they span across zero? In this context, I don't see what a negative k600 would imply.

Section 3.2.3: Is this entire section only using the 4-hr period? If so, I think a quick explanation of why only this time was used, and why it was chosen for UAEC kO₂ work. Wouldn't more data have helped with choosing a parameterization for section 3.3?

299-304: I don't think it really means anything, that the standard deviations of the modeled k600 values overlap and are "not statistically different" because they were modelled from a range in wind speeds....the larger the range in wind speed, the larger the standard deviations in k600. If, on the other hand, your calibration window had consistent wind speed, then the SDs would be much tighter and not overlap.

Section 3.3.1: Here, I think it is missing, a consideration of the uncertainty in k parameterization on modelled CO₂ fluxes. 11 k models were used earlier, so I do not see why only one is chosen for CO₂ flux derivations.

Section 3.3.2 & 3.3.4: Please clarify which k model was used for the storm fluxes. Was it only N00? If so, I again here think it would be helpful to at least consider a range in k models.

370: Where in figures 1 or 3 is this underestimation shown, and what is being compared with what? It certainly makes sense that the land-based stations would more often underestimate offshore wind speed, but I don't see where this is quantified in the paper.

375: Would it be reasonable here for you to suggest a better way to handle the modeling of fluxes when only land-based wind speed is available? Maybe some kind of roughness length scaling?