egusphere-2025-2914
"Quantifying the farmed kelp atmospheric CO_2 uptake through localized air-sea flux in the Northern Gulf of Alaska"
Haag et al.

Author's response

We thank the editor for their encouraging and constructive comments on the manuscript. Specific concerns are addressed in detail below.

<u>Comments from Reviewer 1:</u>

Abstract

Line 14-16: This statement doesn't make sense to me because clearly there are differences between inside and outside sensors throughout the entire time series (i.e. Figure 5 doesn't have net flux that goes to zero beginning in March).
We have revised the abstract to make it clear that there is a difference in carbonate chemistry between the inside and outside mooring in the earlier part of the timeseries in lines 14-19. We also discuss this discrepancy further in the methods, discussion, and appendix to address a comment about whether the data at the inside mooring of Kalsin Bay is reflective of *in situ* conditions. Please see the first comment from reviewer 2 below.

Intro
Lines 34-36: This statement is problematic. Both references provided are old and not really useful in the context of "nearshore". They differentiate continental shelf and estuarine fluxes, but aren't specific about "nearshore". Suggest considering Laruelle et al 2018 and other more recent references that illustrate conditions are more of a mosaic related to proximity to tidal mixing, upwelling centers and freshwater sources.
We have removed the first part of this sentence to reduce the mechanisms discussed in this paragraph and to shift the attention to the effect of kelp farms.

Lines 62-64: Earlier (Line 39) it is stated that for kelp farming to have climate benefits, this activity needs to cover 90,00km2. Then at the end of the intro, the authors seem to suggest kelp farming in Alaska can have some kind of climate benefit despite the potential area being significantly smaller. Perhaps their final statement in the intro should be revised to align with the earlier statement about the importance of scale? Maybe this is simply switching "identifying" with "exploring" given the results of the study seem to only highlight how challenging it is to constrain the role of kelp farms on sea-air CO2 flux.
We have changed this sentence first by replacing "identifying" to "exploring" as suggested and also remove mention of anthropogenic CO2 emissions and instead highlight the possibility of kelp farms providing OA refugia to marine organisms. Based on reviewer 1 and reviewer 2 comments, we are shifting the narrative in the intro to highlight the importance of scale.

Methods:

Line 83: delete "accurately" since your estimate only provides a sense of the magnitude and direction given other issues with the calculation.
Removed the word "accurately."

Line 104: how do you do a 3-point calibration with a single CRM (Batch 172)? Typically the spread of calibration solutions covers the dynamic range of observed values.
The DIC Analyzer takes three replicate samples from the same CRM of three different volumes for a total of 9 samples which it then uses to determine accuracy and drift. This is an automatic feature of the instrument. Please see this short document to get further information: https://www.oceantech.co.kr/data/file/OT31/1889717392_npVDZNu6_56e1c109d13bf048c4e2f165239d8f9a6c9d385e.pdf. To clarify, we removed mention of calibration when it comes to these triplicate samples.

Line 110: replace "translate" with "adjusted"
Removed entire sentence because we removed the translation/adjustment after the last round of edits with the reviewers.

Results:

Line 204: I'm not sure how you got a transition period of 0 days. The definition of transition period seems to be (1) starts with the first 24-hours of a sign switch in AOP and then ends after the AOP is in a consistent state for 7 days. Seems arbitrary and poorly defined. What drew my attention to this is the long transition time for Windy Bay. This seems like an artifact where there was a transition from winter to strong bloom conditions, then a wind event, then a transition again, then another wind event or storm. The excursions in April might have similar forcing and makes me wonder about the utility of "transition period".
To improve the definitions surrounding the use of these terms, we have introduced a new method of using the cumulative average of AOP over the timeseries and used the minimum cumulative sum of AOP to determine the shift from heterotrophy to autotrophy, with no transition period. This slight change in the dates have also made us recalculate the averages for pCO2 and air-sea flux during the heterotrophic and autotrophic periods but it did not change the results (see new methods in lines 165-166).

Line 213: delete "ambient"
Removed the word ambient and replaced with specifically saying we were referring to outside farm seawater.

Line 219: delate "total"
Removed the word "total."

Discussion
Line 309: But Figure 3 clearly shows Windy Bay had a transition period with large excursions from AOP = 0.
We have removed this sentence as we no longer have a transition period.

Line 346: The manuscript could have benefited from some representation of the winds. It is hard

to believe the large excursions at Windy Bay aren't wind-driven. Without these excursions, the net flux would have been more negative. It actually looks like the increase in pCO2/decrease in AOP was coincident at both locations.
We discussed wind as a driver of the fluxes on lines 365-370 and its potential importance in influencing the magnitude of the fluxes based on air-sea CO2 differentials. We stated "Wind speed dominates the magnitude of these fluxes, therefore an increasing differential between seawater and atmospheric CO2 would still require strong winds to drive FCO2 (Eq. 1 and 2). However, wind forcing weakens through spring, which can slow air-sea CO2 equilibration (Stabeno et al. 2004). Therefore, the timing of wind and air-sea CO2 differentials are important when considering the ability of kelp farms to draw down atmospheric CO2, as a mismatch between seasonal winds and the farmed kelp growing season would result in a reduction of CO2 uptake (see wind speed in Fig. A2)." A plot of wind is now in the appendix as Fig. A2.

Line 353: and the assumption kelp are the sole primary producers…
On line 396, we said "Estimates of other sources and sinks of kelp-derived carbon in the marine environment are needed to contextualize the effect of farmed kelp, particularly the effect of phytoplankton in controlling the seawater carbonate chemistry." The reason we utilized an inside and outside mooring was that we wanted to characterize background photosynthesis and respiration from other organisms/processes so that we could try to isolate the kelp signal. We are open about the fact that kelp are not the sole primary producers but also want to acknowledge that the impact that phytoplankton have on seawater chemistry is captured by the outside/reference mooring and has been presumably accounted for in our net flux estimate. The final sentence of the discussion does reinforce that measuring chlorophyll biomass would be important in future work (see line 406).

Line 360: I think it is hard to imagine uptake of DIC into kelp tissue at one site but a release of DIC from kelp tissue at another site despite kelp being obviously harvested at that site. How does that work? Also, define tCO2.
There is an uptake of DIC into kelp tissue at both sites, just as there is a release of DIC from kelp tissue at both sites. However, the point of this paper is to indicate that depending on site and timing of kelp growth, a kelp farm can act as either a NET source or sink of DIC. We would recommend this study which demonstrated that aging kelp became a strong net source of DIC relative to early stages of kelp growth (https://doi.org/10.1016/j.agee.2023.108824). We have substituted "tons CO2" instead of "tCO2" for clarity. To ensure the data collected from the SAMI CO_2 sensor was a reflection of *in situ* conditions, we included verbiage in the discussion that compared measured $p$$CO_2$ vs expected $p$$CO_2$ based on a 1:1 (O_2:DIC) stoichiometric relationship using the independent observations from the MiniDOT O_2 sensor, which demonstrated that our $p$$CO_2$ observations at Kalsin Bay were robust rather than a result of sensor malfunction, conditioning period, or drift (see lines 309-320). We then followed up with our reasoning as to the high observed $p$$CO_2$ values in the Kalsin Bay kelp farm, which was not a result kelp biological processes but rather biofilm that grew on the farm during the winter period.

Line 365: missing reference
Citation added.

Line 369: aragonite isn't at saturation with respect to seawater, it is at saturation with respect to

the mineral solubility product. Short-hand, one could say "at/above/below saturation with respect to aragonite".
We have included "the mineral solubility product"

Line 370: it must not have reduced omega consistently with the large pCO2 excursions observed.
In Figure 9, the aragonite saturation state at the inside mooring (in green) was lower than at the outside mooring (in purple). I have included the wording "appeared to reduce" instead of "reduced" to emphasize that it can be seen observationally.

Conclusion: The first phrase is hard to reconcile with this study: "kelp farms influenced the seawater chemistry". Where the variability was lower (Kalsin Bay), the kelp farm appeared to be a source. Yet this region is clearly unique (TA-S is wild because of proximity to the Copper River – great to include that in the appendix, and the site had high pCO2 with low AOP – noted below). The site with highest variability was a weak sink, likely due to storms or wind events. In both cases, the ambient environment played a big role in determining the background conditions for which small differences between inside/outside kelp farms could be observed. This seems to suggest that the heterogeneity of coastal settings (gets back to the first point about nearshore heterotrophy), and the site selection of kelp farms within, is critical to evaluate before one can naively think a kelp-based carbon market could be devised.
We are in agreement that the site selection of kelp farms is critical to determining the effect of kelp on seawater carbonate chemistry, especially as we observed one farm to strengthen the carbon sink in the nearshore and one weakened it. Site selection is highlighted in the conclusions. We still believe the statement stands that kelp farms can influence seawater chemistry. This statement is not a magnitude, only indicating that there is an observable impact.

Figures:
Check multi-panel convention for journal – none are labeled.
We have added lettering to multi-panel figures and edited the captions to include reference to the lettering.

Figures 3 and 4: This wasn't discussed in the paper, but how does one generate more positive AOP and higher pCO2 at the inside mooring in Kalsin Bay.
This was also brought up by reviewer 2, that at the start of the Kalsin Bay sensor deployment the inside mooring had higher $p\mathrm{CO_2}$. We would argue that in Figure 3, the AOP at the inside mooring is lower than that of the outside mooring in Kalsin Bay, if not similar, rather than higher as mentioned by reviewer 1. These combined metrics, higher $p\mathrm{CO_2}$ and lower AOP, suggest greater respiration, or perhaps decreased photosynthesis, that would in turn drive the system towards heterotrophy. For a greater discussion on why this is and how we've addressed it in the manuscript, please see the response to reviewer 2 in the next paragraph.

<u>Comments from Reviewer 2:</u> (Note: the reviewer left follow up comments on my last author response, so I have pulled the first reviewer comments in black, then my response in green, reviewer 2's new response in red, and my final response in non-italics)
Line 118: what do you mean SAMI-CO2 was translated up or down relative to the water samples? Show the data with in pCO2 time series figure. While we are able to perform calibrations for the pH sensors, the SAMI-CO2 sensors are highly proprietary so there is no way

to correct the values before they are available as a pCO2 timeseries. We could not do a thorough calibration like we were able to do for the pH sensors, we translated the SAMI-CO2 sensors relative to water samples. However, after receiving this comment, we have reviewed published manuscripts that used this instrument and found no mention of doing any calibrations to the data directly given from the instrument so we have decided to undo the translation mentioned. See below for the raw data and the discrete bottle samples overlain. This has caused changes to the results in the manuscript. While I think it's more typical not to adjust the SAMI-CO2 data using an outside "calibration" measurement, as the authors noted from looking at other articles using these sensors, it does result in the pattern shown in the figure above (and fig 4), where pCO2 is fairly consistently higher at the inside mooring than the outside mooring at Kalsin Bay, particularly at the beginning of the record. What would be causing this? Could it be calcification preferentially within the kelp farm area, which would increase pCO2? Or higher respiration in the farm than outside? The difference between inside and outside moorings is large in Jan-Mar (up to >100 µatm in the early part). The apparent O2 production curves shown in Fig 3 don't suggest any meaningful difference in primary production between inside and outside moorings, suggesting the seasonal signal is dominantly phytoplankton, as detected by the O2 sensors. I haven't worked with SAMI-CO2 sensors myself, so I don't know whether it's reasonable to believe the difference in the early season CO2 at KB compared to the more similar oxygen lines (which are not O2 concentration, so there's a bit of applies to oranges in my comparison). It looks like the same anomaly exists at the beginning of both time-series, suggesting a high respiration (or at least high CO2, low O2) event was observed by both O2 and CO2 sensors. Not sure if thinking this through might help you resolve the complexities in this story.

We have included additional tests to determine if there was a sensor issue that resulted in the observed high pCO_2 values inside the kelp farm in Kalsin Bay. In the methods, lines 149-152, we describe comparing the independent sensors that measured carbon dioxide and oxygen to demonstrate that they align well (see Appendix Fig. A4). The results of these tests were included the results section, lines 222-226 and are included in the discussion, lines 309-316. This leaves us with confidence that the inside mooring pCO_2 data is a reflection of *in situ* conditions and not the product of sensor malfunction or drift (esp. given we describe an uncertainty term for the time series). Furthermore, after consulting previous notes, we remembered that the farmer had described a biofilm on his lines in early winter and our current hypothesis is that the high respiration inside the farm is due to the respiration of that biofilm, was likely formed by microbes. We suggest this in the discussion on lines 316-320.

Figure 2 – consider plotting density and maybe spice time series instead of T/S diagrams. It is hard to see any differences with those plots. I do not that Kalsin Bay outside mooring does see a lot more fresh water than inside mooring. The reason we used the T/S diagrams is that we are trying to compare what masses between the inside and outside moorings. I think Wiley was suggesting that plotting spice and density (not sure if he meant potential density anomaly, or sigma, which you show in these plots) may show the differences in water masses more clearly, but in complex coastal environments like these, where mixing can be vigorous (as you noted for your sites), I'm not sure it would be more helpful in presenting a clear pattern.

The lack of differences with the plots from the same bay are the reason that they are presented. This supports the use of pairing the inside and outside mooring because similar water masses are passing through the two moorings.

Introduction--While many of the salient considerations are introduced here (mCDR, OA, carbon burial rates), the introduction could be streamlined to more clearly state the goals of this project. As currently written, I wasn't certain whether they were arguing that kelp farming would be a major contributor to mCDR (as implied in the 2nd paragraph) or ameliorate OA conditions more locally (as paragraph 1 seemed to imply). Some of these details would probably be better suited for the discussion, as they are a bit confusing here (e.g., they mention low natural OC burial rates in coastal sediments, but make no mention of sinking kelp to the deep ocean until much later). We hesitate to remove all mention of either mCDR or OA from the introduction because both are discussed later in the manuscript and we want to make sure that they are introduced at the beginning. We have removed the sentence describing burial rates, however, to leave that for the discussion. I don't think my comments came through clearly before—my apologies. I was trying to indicate that the purpose of the study could be clearer by streamlining the points made in the intro. The first paragraph very broadly introduces CO2 leading to OA, a few fairly specific biological effects of OA (possibly too specific?), and impacts to the poorest and most vulnerable humans (maybe not particularly germane here?), and introducing mCDR as a potential solution (good—this seems to be your main point).

We have removed the sentence about the impacts on humans as it is germane. We appreciate that the examples given of the effects of OA on marine organisms may seem specific, but we wanted to emphasize the variety of ways that it can impact the marine food web.

The second paragraph points to burial of biomass from seaweed farming, but also discusses metabolic balance of kelp farms (heterotrophy, autotrophy, timing of peak atmospheric CO2 drawdown), scaling considerations, etc., and the nutrient removal issue I commented on previously—a lot of different complex processes—not clear what the key points are perhaps. The third paragraph starts the discussion of "why Alaska and kelp farming," which is good.

The intent of paragraph two is to provide background on how seaweed can be used for mCDR and its feasibility. We have focused this paragraph to be: seaweed is considered for mCDR -> study showing that kelp can reduce atmospheric CO2 -> we need a lot of kelp farms to reduce atmospheric CO2 by any appreciable amount in a way that matters -> kelp farms could still be useful for providing a local "halo" against OA -> but kelp farms are also not completely without negative consequences. We hope this clarifies the key points.

To my reading, your first few paragraphs point in too many different directions, without setting up the conversation about "why Alaska and kelp farming" very clearly. Your results seem to point to substantial differences in source vs sink status at your two remaining sites, which is useful and interesting. I think a key point to make to this broader community is that if people are going to engage in seaweed farming as an mCDR approach, it's going to be absolutely critical that they understand a) not all sites will yield the same results and b) there may NOT be any CO2 drawdown/climate mitigation co-benefit of growing kelp (implication: it's critical that the rest of their cost-benefits analysis pencils out because this one might not). Personally, I'm concerned about a future in which people are growing kelp (for example) and imagining that the net impact is positive when it may not be, and even more concerned about the rush to give carbon credits to such efforts without them being fully and clearly vetted. While you can measure the amount of carbon in the kelp, taken up from seawater, that doesn't mean that either a) CO2 was removed from the atmosphere in a meaningful way or b) that local acidification levels were mitigated. It seems to me that one might want to do kelp farming to a) locally mitigate OA to, for instance, grow shellfish (as the Jiang et al. 2015 paper looks like it addresses) or b) to ultimately contribute to global CO2 drawdown (or c, both and more). I think it's easier to envision a

successful implementation of local mitigation of OA (which will still have very site-dependent outcomes). The scaling argument for a global CO2 drawdown contribution requires long-term sequestration of the CO2 taken up by kelp (along with the long-term removal of nutrients), and I'm not sure what the long-term reservoirs for kelp carbon are other than deep ocean, which has a lot of potential reasons not to do it. (Also noting that there could be other ways to have a positive impact on the global carbon cycle through kelp, such as by feeding cattle kelp to reduce methane emissions—not sure what papers address this, but I have heard Nichole Price talk about it.) Your sentence about Jiang et al. 2013 indicates that local atmospheric CO2 was drawn down in their study. I am not sure why local atmospheric draw down matters, when it's the global atmospheric CO2 accumulation that needs to be reversed. In contrast, local mitigation of OA conditions could be very helpful to regional efforts (e.g. shellfish co-farming). (Or in the feeding cattle kelp example above, maybe you make a different argument about the carbon benefits.) (Also want to clarify that the local OA "mitigation" might more correctly be described as an "adapation" approach, whereas global CO2 drawdown may more correctly be referred to as "mitigation"—I hope my wording above isn't too confusing for this reason.) In any case, this got longer than intended, but the breadth of your first two paragraphs sparked some confusion when I was reading this, because it was not really clear whether you were really arguing that kelp farming in AK could help the economy by supporting shellfish co-farming (through local OA mitigation) or to the global CO2 drawdown solution. It seems like you reference the latter more, but there's enough info about both CO2 drawdown and OA to muddy the waters. In my opinion, kelp as a global CO2 drawdown solution is a harder sell (less likely to make the intended difference, less directly relevant to AK economy, etc.). In either case, thinking about your main goal for presenting the differences across sites and either sticking to one of the two options (local vs global benefit) above OR just being very clear that these are two very different things, with distinct pathways and value structures, would likely make this paper more useful to your ultimate audience (kelp farming enthusiasts, I presume).
We appreciate the thorough response addressing this point and agree with the sentiment behind it. This study was part of a larger project funded by the Exxon Valdez Oil Spill Trustee Council to characterize the influence and impact of scaling up mariculture in Alaska. In the intro, we had attempted to convey similar information to what you mention here, but due to the confusion you brought up, we have tried to make it more explicit. We are clear about the impact of scale (ie the immense scale needed to have an affect on global atmospheric CO2 drawdown) and leaned into the possible local positive affect of local remediation of OA, especially as it is discussed in the discussion with the aragonite saturation state figure. We have also included a sentence on co-culturing between farmed shellfish and kelp on lines 50-53.

2) I also agree that given the importance of wind data in many places throughout the manuscript, as well as the seemingly contradictory comments regarding the role of wind (cf. line 375 and line 390-391), it's confusing that wind data aren't shown. There are some good discussion points about peak wind season not coinciding with kelp growing season in the discussion. It would be helpful to see the NDBC buoys on the maps and some plots of wind speed. It's all just alluded to in the text now. As per reviewer 1, we have now added the ID and distance between the buoys and the farms to the methods on lines 150-151. We have not added it to the figure 1 because it is too close to the farm on the Gulf of Alaska overlay and not in the view on the smaller maps. We have plotted the wind data for you to see here. There is not much information to gain from it so we could move it to the appendix if you believe it is useful. Great—thanks. I see your point about

the plot not being particularly informative in that form, although perhaps some readers would find it useful in the appendix. I don't feel strongly about it one way or another.
As reviewer 1 also had more comments on wind in this second round of feedback, we will move the figure to the appendix for readers who might want to see it as figure A2, as suggested here. I have included the reference to the figure in the methods and the discussion when the wind data is discussed.

TA in decomposition analysis: Normally the influence of photosynthesis and respiration on TA are considered negligible compared to their effects on DIC (e.g. https://www.researchgate.net/figure/Effect-of-various-processes-on-DIC-and-TA-arrows-Solidand-dashed-lines-indicate_fig2_360026150). In high productivity settings like this, I would expect you might see an effect of production (and respiration) on TA, but I think you may still not need to include TA as part of your decomposition, as the DIC effect is much larger, and they are not independent (unless, maybe, you are making an argument for CO2 vs bicarbonate uptake, in which case this needs to be explained/justified, especially why they would be independent). If you exclude TA, does it change the magnitude of your residuals (down in lines 296-299)? This is an incredibly valid point. We included TA initially because kelps can draw up bicarbonate, or there may be shifts in TA from nitrate uptake or ammonium release if there is high enough productivity. But when we reran the decomposition without TA, the residuals increased. Please see the figures below, first showing the residuals for Windy Bay inside the farm including TA and then second not including TA. Since we observed the residuals increasing without the inclusion of TA, we have decided to keep TA as part of the decomposition but we included the hourly decomposition in figure 7 instead of monthly as suggested by the reviewer. That's interesting, and to be honest, I'm not totally sure what to make of it. I agree with your assessment that keeping TA probably makes sense. This too makes me wonder about a role for calcification and dissolution by/of calcifiers in the area (as I've noted a few other places in red here). This is an area where I feel my comments in this round are limited by my not having time to fully go through this manuscript again.
We kept TA in the decomposition.

There are a lot of different results presented for each site, and it's not always 100% clear how they relate to each other across sections. For instance, line 19 says KB is a sink, WB a source (excluding JB as sink due to bad data), but then in L250-251, it says JB is a source and KB and WB are sinks. I get that both things can be true, if, e.g., you are considering different timescales, but a table may help organize and present the information in a way that is easier for readers to follow. I think the confusion lies in that we are talking about two different things in both locations. In the abstract when we say that KB is a sink and WB a source etc, we are referring to the net integrated flux as in the farm is acting as a sink or a source based on the carbonate chemistry measured at the farm relative to outside of the farm. Later in the manuscript results, when KB and WB are both sinks, that is referring to the air-sea flux at the farm site in spring demonstrating that as photosynthesis increases in spring, the nearshore acts as a sink of carbon. One is a net value comparing inside and outside the farm and the other is the actual air-sea fluxes at the farm. I see. It may be clearer to describe it in terms of the farm increasing or decreasing the strength of the sink or source, relative to the reference site, rather than the difference between the two sites being source/sink. I think of the landscape (or seascape in this case) as a lateral patchwork of net sources or sinks on the chosen timescale by whether the sign

of the flux is positive or negative, rather than as a comparison between a manipulated plot and a reference location. If I understood your explanation correctly, I think using source/sink this way will confuse other readers too.
We like the wording presented here of the farms increasing/decreasing the strength of the sink or source relative to the reference site. This has been changed in the abstract (lines 19-20), the results (lines 245-246), and the discussion (line 360).

On a similar note, there are contradicting statements about net source/sink status for Alaska's coastal waters: cf. line 61 and line 37. It may require some reorganization of the topics discussed throughout to clarify how they do or don't actually (dis)agree with each other. As written now, they are both just stated and not critically compared. In line 37, we introduced the idea that the nearshore is usually a source of CO2 because it is heterotrophic and in line 61 we say that the coastal system is generally net heterotrophic which would insinuate that it is a source to the atmosphere. When we mention that the offshore ocean acts as a carbon sink, it is in reference to the continental shelf. Perhaps this is an issue of the definition of "coastal" as we intend the term to not include the continental shelf. We have changed the wording on line 62 to reflect this. I think you are correct that this is an issue of the definition of "coastal." I think folks like Chen and Borges and Cai may use "coastal" to describe the full continental shelf (others use 200 nm, which is roughly equivalent to the EEZ), but they also work on estuaries. In my mind, "nearshore" is something like from 30-50 m water depth to the intertidal (or something like that—I'm less familiar with what exact operating definitions exist), whether you are in an estuary or more open waters. It may be good to clarify the relevant language further in these lines (like I am thinking the sentence with the two references I referred to above may not be "nearshore," as it still says.
We have removed the first mention of "nearshore" and the Chen and Borges and Cai references while addressing previous comments about the organization of the intro, and when the topic is brought up on line 57 we are clear to distinguish between "coastal marine systems" and the "offshore on the continental shelf." We hesitate to include specific arbitrary definitions of nearshore versus offshore as this is the only time we discuss the distinction. We believe that this will remove the contradiction mentioned by both reviewers.

Did they correct for the changing direction of advection of water masses into/out of the kelp farms as the tides changed? They simply give 0-1 hour lag times that are a) unclear how they arrived at them and b) unclear whether these may be reversed with the tides. The lag times were calculated using a cross correlation mentioned in the methods on lines 168- 169. We did not explicitly correct for the changing direction of tidal advection, and we acknowledge that ebb and flood tides may alter the direction of water exchange. The short lag times (≤1 h) and the strong tidal forcing in these bays suggest that inside and outside moorings experienced largely coherent tidal signals, but finer-scale reversals could occur that we were unable to resolve with our current sensor configuration. To address this, we have clarified the lag-time methodology in the Methods section and added a note in the Discussion on lines 470472 acknowledging that tidal reversals may introduce additional uncertainty. Good addition in the discussion section. Maybe "could" instead of "should"? Can you get from chlorophyll measurements to phytoplankton "abundance" (not sure whether this means biomass or cell count or what have you but I thought chlorophyll amounts per cell or biomass could be pretty variable)?

We replaced "should" with "could" and have changed "phytoplankton abundance" to "phytoplankton biomass" because the reviewer was correct in pointing out that chlorophyll would not give abundance.

--L 116-117: implies T measured on discrete bottle samples, correct? We measured temperature in the lab when using it to calculate the pH and DIC from the spectrophotometer readings and doing the conversion for DIC from µmol/L to µmol/kg. This sentence is still confusing (now at L124-5). HOBO loggers are for T only, yes? (Haven't used them myself.) You wouldn't be using the T from the discrete samples in the lab bottles, correct. Did you just use S from those samples? A few more words may be needed here and there to clarify.

We used the salinity from the bottles at the lab since the SAMI sensors did not measure salinity, only temperature. As advised, we have made the wording clearer in lines 112-113.

--Fig 2 caption: not sure what the parenthetical statement at the end is meant to say…typo? It is meant to denote that we are plotting the density anomaly and not the density. Still unclear as written. Can you just say "isolines of potential density anomaly (σθ)"? Possible to put theta instead of zero in the figure?

We have added "anomaly" as suggested. The parenthetical statement is a typical way of demonstrating that the values shown as the isolines are the density minus 1000 kg m-3 to get to anomaly.

--L 264: Do the net FCO2 values correlate to the biomass grown at the sites? Net FCO2 values only reflect the subtraction of inside versus outside mooring air-sea flux data, no inclusion of biomass. I think this is related to my comment above about the difference between sites vs. net flux integrated over a time interval. If so, better to frame as delta net flux (or just delta flux) maybe?

We want to retain the "net" as a reminder that it is the difference between the inside and outside moorings. We also used the term "integrated" to note the integration we did over the timeseries which is the reason for the delta. To keep the terminology the same, we have added more instances of "integrated" rather than "delta."

--Lines 269-277: Really need to see the discrete samples overlaid on the mooring data! Also I would disagree that there's homogeneity of FCO2 anywhere but Windy Bay, and the statement at the end of this section implies it's at all sites (or maybe just not clearly worded to indicate it's only about WB?). We have changed the spatial survey figure to show a histogram to better see the clustering of the discrete samples relative to the mooring data. We do imply that there is homogeneity for both Windy Bay and Kalsin Bay because the air-sea flux values at the inside mooring tended to be similar relative to the outside mooring. We chose not to display the discrete samples over the mooring data because there is already so much data on this plot and the new histogram can clearly compare the two. Fair. I was envisioning the discrete samples plotted on top of the time-series, like what you did in the plot higher up in this document. Those looked pretty good, but address only the agreement part, rather than Fig 6 addressing the spatial variability. Personally I don't like the histograms as much as the original figures. I think my takeaways from these figures are that spatial variability is reasonably high, particularly at KB, in terms of both within patch variability and the overall range of values (i.e. comparing the two color scales for KB and WB in the original figure).

We went back and forth with the spatial figure and the histogram figure for Figure 6 and ended up including the spatial figure that was in the original submission. Based on the comments here from reviewer 2, we will switch the figure back to the spatial layout to demonstrate the spatial variability more clearly.