Dear Dr. Humphreys and the authors:

I apologize for my great delay in returning comments to you. I'm afraid I've not been able to go through everything in as much detail as I would have liked, so there could certainly be things that I missed or took out of context in terms of completely addressing the authors' changes to the manuscript.

I didn't see where to enter my comments online, so am returning this Word file to the editor, which is an exported version of the author response file with my comments in red where they seemed warranted. I did not systematically comment on R1's comments, nor have I deleted anything from this file. I just added my own reply where it seemed germane to my earlier comments. I have also added additional comments at the end.

All the best,

Simone Alin (R2)

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egusphere-2025-2914

"Quantifying farmed kelp atmospheric CO_2 uptake through localized air-sea flux in the Northern Gulf of Alaska"

Haag et al.

Response to the Reviewers

We thank the reviewers for their encouraging and constructive comments on the manuscript. Specific concerns brought up by individuals are addressed in detail below.

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

Data quality: The data from Jakolof Bay in April are clearly bad. The values of these data are well beyond what is typically seen in nearshore settings in Alaska and other areas along the NE Pacific coast. The authors could have instilled a lot more confidence in these data by showing how the discrete measurements compare. This could be done by simply plotting the discrete pCO2 values with the pCO2 time series in Figure 4. The fact that the fluxes estimated for this region exceeded 200 mmol m^-2 ^d-1 (Figure 5), yet the fluxes from the discrete samples from April 23 were ~4 mmol m^-2 d^-1 is a big hint the pCO2 is wildly different between the two measures. The authors seemed to miss this when they say on Line 275 that "mooring values fell below the range measured discretely at the farm". The figures show the opposite. Clearly both the inside and outside moorings were fouled at this site (indicated by the photo in the appendix) and, given how important the results are from this site for the conclusions of the paper, I feel the authors need to either make a much stronger argument for the validity of the data or remove the bad data from the analysis and re-evaluate their conclusions.

Given the consensus between the two reviewers, we have decided to remove all mention of Jakolof Bay and only include the other two sites in the manuscript.

Glad to see the authors have removed the Jakolof Bay data. It was difficult to see how they could be recovered and yield reliable information. Honestly, I think the two-site comparison is much easier to digest too.

Computing fluxes: On Line 390 the authors state how important wind speed is in dominating sea-air exchange rates (and clearly this is not the case given the above statement about Jakolof Bay – the very high deltapCO2 there – and the resulting extreme fluxes) yet there are no wind data presented in the paper. The authors claim to have used NDBC buoy data but I'm not sure there are buoys in each of these areas. Was it one buoy that was used? Multiple? Can the NDBC buoys be indicated on the maps in Figure 1? If the authors are using the MacIntyre relationship, then they should be clearer about that maybe refer to the fluxes as "synthetic fluxes" and qualify that these estimates are meant to give a sense of the exchange rates and are not actual estimates of the flux (which would include local winds).
We have added to the methods the IDs for the buoys used and the distance from the farms. We have also added wording to the methods describing that we cannot use local winds and therefore it gives a sense of the exchange rate instead on lines 105-109.

Good text additions in lines 105-9.

Calculation of "net" flux: I don't think differencing the instantaneous flux (hourly) from inside and outside the farm site and calling that a "net flux" makes sense. The flux is kind of a net parameter to begin with since it uses the sea-air gradient in the computation. A clearer way to do this computation might be to integrate the flux from inside and outside the farm site, and then difference the integrated values to report a flux difference between the inside and outside of the farm. I assume the start of the "transition time" would be when you would start the integration, but this part wasn't clear in the manuscript.
Whether we integrate before or after differencing the inside and outside, we get the same final number. But we maintain that the net calculation prior to integration is still useful because it allows for a visualization of short-term differences between the inside and outside moorings that cannot be seen when seasonally-integrating the timeseries. However, we had previously used the entire timeseries and when we shortened the integration window to start at the transition time, Windy Bay turned from a source of CO_2 to a sink. We have gone over these values again and noticed a conversion issue with days and hours which overestimated previous calculations. This has been resolved along with the resulting carbon credit estimations.

Integrated signals: As a community providing mooring CO2 system measurements from settings like this, I think we need to be very careful to recognize that pH or pCO2 measurement we are making and reporting on integrate signals reflecting all the processes encompassed in the decomposition they present. Authors attempt to do this but nowhere did I see mention of the local phytoplankton community potentially contributing to the deltaDIC change and the resulting pCO2 values driving the fluxes. This study cannot discount the role of phytoplankton within and around kelp beds, so I suggest the authors re-examine how they are presenting the "role of kelp". Kelp are part of the autotrophic community growing during spring and driving changes in the CO2 system.
We recognize our lack of acknowledging phytoplankton in its role in affecting carbonate chemistry. We have gone through and ensured that when we are discussing the biological effect on carbonate chemistry, we use the general term biological processes and not specifically kelp. Further, we emphasize this with line 462 in the discussion.

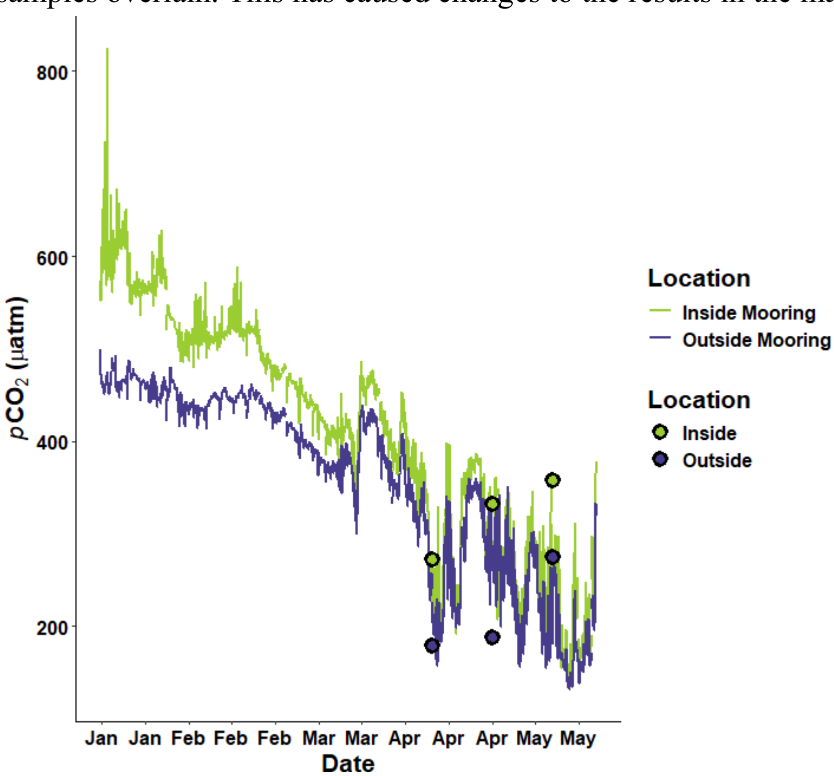
Some minor comments:
Intro – consider some statements of the potential negatives and unknown consequences of using kelp for mCDR. Should reference the National Academies report.
A sentence for the potential consequences of mCDR has been added to the introduction on lines 47-50 and referenced the National Academies report.

Lines 39-41: Poor wording, please rephrase. Disequilibrium refers to the sea-air pCO2 gradient which can be positive (source) or negative (sink).
We have removed this sentence because the paragraph works well without it and to reduce the discussion around the wind data that created issues in other comments.

Lines 49-57: The authors are referring to the northern Gulf of Alaska but then mention the state economy. Here they are referring to the State of Alaska so perhaps should be clearer and refer to Alaskan state of federal waters in the Gulf of Alaska.
We have now mentioned that once in the paragraph indicated, but we will keep the reference to the NGA as the acronym for simplicity.

Methods – could add caveat that 3m measurements may mis-represent the flux in highly stratified settings. If you believe these settings aren't highly stratified then make that point clearer in the results.
We have added the caveat to the methods on lines 105-108. During the spatial survey conducted in spring, we also collected three discrete samples in a vertical column at 3m, 2m, 1m, and the surface to determine if it was well-mixed and the pCO_2 values did not differ between those samples. However, now that we are not using Jakolof Bay in the manuscript, we didn't see fit to include it to aid in supporting our argument.

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 pCO_2 timeseries. We could not do a thorough calibration like we were able to do for the pH sensors, we translated the SAMI-CO_2 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.

Line 154: there is mention of lag time calculations but I don't see how this was used.
The lag time calculations were used at the end of the first paragraph in the results when we indicated what the cross-correlations were for salinity and temperature between the inside and outside moorings to demonstrate the lag in time between the two locations and possible water mass movement.

Flux units: I believe you are reporting hourly data but the flux units appear to be mmol m^-2 d^-1. If you want to keep hourly data, suggest reporting mmol m^-2 hr^-1, otherwise average to daily for d^-1.
We have converted the data to hourly. We hadn't averaged the data to make it daily but had taken the hourly data and multiplied by 24 to put it into units comparable to other studies. But we agree with the suggestion made to keep it as hourly.

Decomposition calculations Line 180, here H is meant to be the depth of the mixed layer and not the sampling depth. This equation is meant to show the change in TCO2 over the mixed layer due to sea-air CO2 exchange. Suggest author's add a caveat that they are exchanging sampled depth for mixed layer depth which may drive larger deltaDIC in areas/times of weak stratification and high flux. Is there a reason the decomposition was done on monthly averages instead of daily (comment above) or hourly data? With monthly, t2-t1 should be month2-month1 and not days and units should match.
We used sampled depth for H because Garcia-Troche et al. had done it that way and we adapted our equations from them. As we are assuming a well-mixed water column with no stratification, we have changed H to reflect the entire water column, as now described on line 193. As per the comments regarding hourly vs daily vs monthly data, we calculated the decomposition for hourly data.

Results – consider starting out with a statement about the time span, volume of data recovered, number of discrete sample events, etc. Why is the inside mooring record shorter than the outside mooring record?
The inside mooring timeseries ended when the kelp was harvested but the outside mooring continued over the summer for AOP to get a sense of what the system might look like past the harvest time. However, we understand the confusion here and have removed the timeseries for the outside mooring past harvest.

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.

Side comment here – the outside farm site moorings should be referred to as reference sites and not control sites. These time series aren't a control of anything but provide some baseline, or reference, to judge the inside farm site time series against. Throughout – remove "control" and replace with "reference" when discussing outside farm moorings.
All mention of "control" has been changed to "reference."

There are grammatical errors in a number of places throughout the manuscript.
We have done a thorough readthrough of the manuscript and believe to have found the grammatical errors referred to here.

<u>Comments from Reviewer 2:</u>
Major points:

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). The second paragraph points to burial of biomass from seaweed farming, but also discusses metabolic balance of kelp farms (heterotrophy, autotrophy, timing of peak <u>atmospheric</u> 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.

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 CO_2 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) CO_2 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 CO_2 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 CO_2 drawdown contribution requires long-term sequestration of the CO_2 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 CO_2 was drawn down in their study. I am not sure why *local* atmospheric draw down matters, when it's the *global* atmospheric CO_2 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 CO_2 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 CO_2 drawdown solution. It seems like you reference the latter more, but there's enough info about both CO_2 drawdown and OA to muddy the waters. In my opinion, kelp as a global CO_2 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).

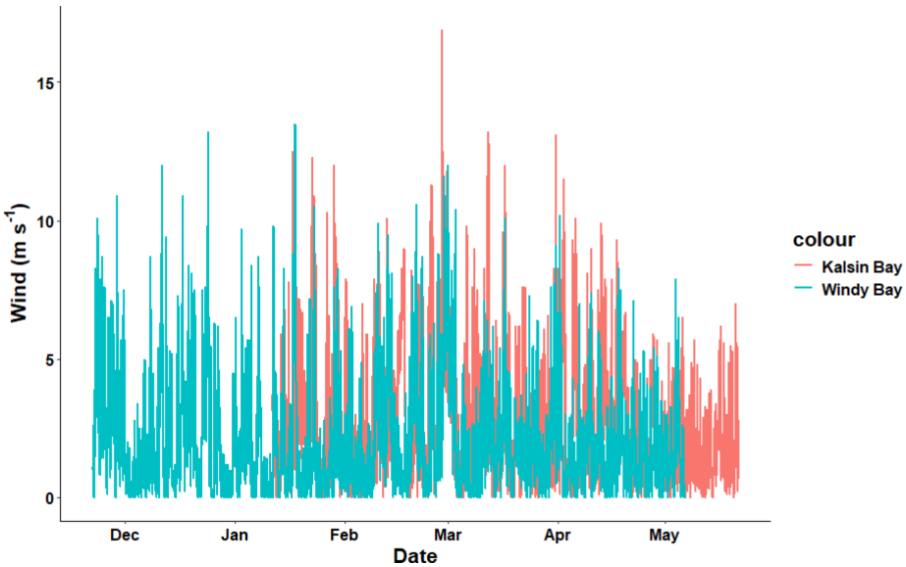
[[By the way, the comment about seaweed needing to absorb 4 Gt CO_2/yr is incorrect—the statement in the paper cited is that that 4 Gt CO_2/yr needs to be removed from the atmosphere to achieve net zero by 2050, with seaweed being one major potential contributor if it can be scaled up to Gt scale.]]
This sentence has been removed for accuracy but also for clarity as the sentence before it already emphasizes the point we wanted to make, that kelp farming would need to upscale dramatically.

1) I wholly agree with Dr. Evans' comments generally, and specifically about data quality being bad at Jakolof Bay. I concur that a much stronger justification would be needed to keep that site

in the published results, otherwise remove that time-series.
We have removed that timeseries from the manuscript.

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.

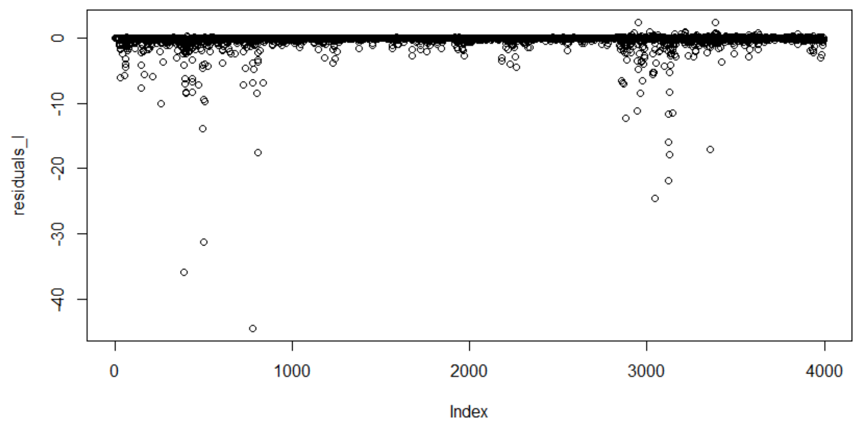


3) Confusing mix of units in the decomposition calculations (monthly vs daily or hourly values)—needs clarification.
We are changing all the data to hourly to avoid confusion.

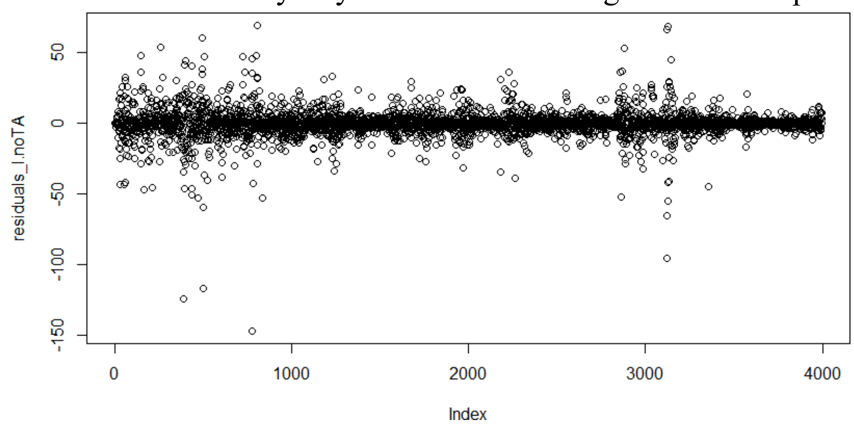
TA in decomposition analysis: Normally the influence of photosynthesis and respiration on TA are considered negligible compared to their effects on DIC (e.g. https://www.researchgate.net/figure/Effect-of-various-processes-on-DIC-and-TA-arrows-Solid-and-dashed-lines-indicate_fig2_360026150). In high productivity 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.

Residuals from Windy Bay farm including TA in decomposition:



Residuals from Windy Bay farm NOT including TA in decomposition:



Contradictory(?) quantitative statements:
1) 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.

2) 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 CO_2 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.

Circulation: Having been the lead carbon assessment scientist on an early seaweed uptake experiment in Washington state (to look at local OA amelioration), I have an understanding of what a critical role circulation plays in the results for these sorts of studies. A few questions:
1) The authors state in L 189-190 "Comparison of water mass movement at the inside and outside moorings confirmed that both sensor arrays detected similar water masses, allowing for a calculation of net air-sea CO2 flux when paired with the inside sensor array." but they did NOT measure water movement or really describe how they made these inferences. How was this conclusion reached?

We did not deploy current meters at the sites during this study so we did not quantify circulation directly. The statement that is pointed out from line 189-190 was made based on the similarity of the T-S diagrams at the inside and outside moorings (Fig. 2) and the observed coherence of major tidal signals across sites (Fig. 8). This suggests that both moorings were sampling the same regional water mass and the cross correlations demonstrated that the lag time is within an hour or two.

Reading it again, I guess it's clear enough for these purposes. Our study involved a more detailed model, etc., so it was probably more critical to have current measurements.

2) 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 470-472 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)?

3) Dr. Evans asked about whether the 3 m depth of the kelp and sensors is relevant to air-sea gas exchanged. I have the same question. It's hard to tell to what extent O2 and pCO2 covary temporally across the figures, as the timescales of the plots are different. If they covary strongly, that may suggest that gas exchange is not having a large effect, as O2 should be exchanged much more rapidly than CO2. The authors state in lines 345-346 that WB and KB had well-mixed water columns, but with sensors at a single depth, I'm not sure how they inferred the well-mixed status of the water columns.
I am hesitant to use CO2 and O2 comparisons to infer gas exchange. While there should theoretically be a 1:1 conversion between the two for respiration, many of the photosynthesizers in Alaska (kelps and phytoplankton) can also utilize bicarbonate which would decouple that signal. While we cannot prove using data we collected that the water column is fully mixed, these are shallow, tidally energetic systems that have been shown in the past to be well-mixed, especially due to the elevated tidal height in the region (Archer 2013, Haag et al. 2023). Additionally, the consistency between the inside and outside moorings at a site suggest that the bulk water mass was well mixed so without a strong freshwater input, we believe that the water column would be homogenous. We have added this caveat to the methods on lines 106-108 to caution our results.

Good points about bicarbonate utilization decoupling the CO2:O2 relationship. I like the addition in 106-8 supporting your statements about water column mixing.

Minor points:
-In Fig 1, would help to make the land a different shade than water so the reader doesn't have to spend time figuring out which is which, especially in the site-specific panels.
I have shaded the land darker as per the suggestion.

Much easier to digest!

--Methods: Unclear what company makes the DIC analyzer used with just a model #
We have added Apollo SciTech next to the model #.

--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.

--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 (σ_θ)"? Possible to put theta instead of zero in the figure?

--Fig 4: the log scale used for Jakolof Bay pCO2 data is unusual—would not suggest using it because it obscures the values. If they keep the log scale, they definitely need to specify the base;

when converted to linear scale, the pCO2 values range from ~630,000 to 100,000,000 (log10(pCO2) of 5.8 to 8), which is clearly bogus. I'm assuming they meant log(e), which would translate to more like 300-3000. Plausible, but I'd still question the upper end of this for AK waters. The comment about the "astronomical rise" in FCO2 at JB definitely sounds like a strong sign of flawed data quality onset. (* FWIW, I was taught 10 is the default base for log, and ln is for base e, so this may vary across readers.)*
We have removed Jakolof Bay data from the manuscript.

--Line 221: do you really mean "uncertainties" or variability? If uncertainties, those numbers do not instill confidence in the results. As variability numbers, these seem more reasonable.
They were conservative uncertainties that include any error associated with differences between sensor readings and calibration bottles, uncertainty in the laboratory seawater analyses, etc.

--Fig 5 colors don't match legend? Or I think you just can't see the green? Put the green on top? And put legend with the panels it goes with?
The legend colors is only for the lefthand panel which has green in it but the overlap is so great that it can be difficult to see. We have remade the figure to reduce confusion.

Much better!

--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?

--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).

--Fig 6: several different font sizes, panels are not the same width
The edits have been made to Fig. 6.

To give you the good with the bad, I said I liked the original Fig 6 better, but I like the new Fig 7 way better! Well done.

--Fig 8: the inside and outside mooring plots appear to be identical for KB…mistake or real?
I have rerun the analysis and there might have been a mistake because they do look slightly different but the inside and outside plots still look really similar so that won't have an effect on the results.

Solid. The best kind of revision…fixing an issue without involving too much extra work. ;-)

L309-311: I mean, sure, there's MORE kelp around the inside mooring at JB than the outside mooring, but the outside mooring has higher pCO2, yes? So that could suggest more respiration there, in contradiction to this statement? Also, on the plots with both inside and outside mooring data, it's often very hard to confirm the authors' interpretation in the text because the data overlap so much. Maybe the authors could put a delta plot panel in the O2 and CO2 figures (figs 3-5, also fig 9) so that readers can see the patterns the authors describe more clearly? It could be a single plot at the bottom with both sites' (WB, KB) delta(inside-outside) values shown in it.
We have removed Jakolof Bay from the manuscript, but yes, there might have been some confusion around the language associated with the pCO_2 at the moorings.

--Citing figures 8 and 9 on lines 375-376--seems like the figure #s are wrong.
They were the wrong figure #s when there used to be an additional figure that was removed. I have changed the figure #s and checked throughout the manuscript for additional figure # issues.

--Section 4.3, first paragraph: Would the loss of the fixed nutrients from coastal/nearshore areas be a detriment to these ecosystems if kelp was grown "at scale" in this region and subsequently sunk to the seafloor?
Yes, that is a good point. I have added a sentence to lines 442 to 444 to that effect.

L412 and later on: normally expressed as "at saturation" (vs. under or supersaturated) rather than in equilibrium (which can refer to a less fixed value, such as atmospheric, vs. the "saturation" value, which takes changes in T and S into account intrinsically)
We have changed the wording from at equilibrium to at saturation with respect to aragonite saturation.

L415: may be better to word as "may decrease exposure to conditions favoring dissolution"
Wording changed.

L416-417: Important point!!!

L419: Unless you are discussing specific species, technically dissolution shouldn't be a major issue when aragsat is > 1 as it is everywhere/every time when not under significant biofouling conditions.
Following this comment, we have decided not to stray too far into discussing the effects of aragonite saturation state other than suggesting that under future OA conditions when the aragonite saturation dips to lower values, kelp farms may or may not actually increase the value. See lines 425-434 for changes.

Fig A1: The Evans et al. 2015 relationship does not seem appropriate for these sites, in that the line is substantially below all but two of the measurements from this study; thus, using salinity as a proxy will underestimate TA systematically, assuming their discrete samples were representative. You could look at the effect that the magnitude of difference you saw between *measured and estimated values would have on your results (Fassbender et al 2017 did something similar: https://link.springer.com/article/10.1007/s12237-016-0168-z).*

As suggested, we computed a sensitivity analysis similar to Fassbender et al. 2017. We used the Evans et al. 2015 salinity-temperature relationship to predict the TA for the discrete spatial survey samples that we had measured in the lab for TA already. The residuals for that can be seen in the table below. Then, we estimated pCO_2 using the predicted TA and measured TA with the package seacarb in R to quantify the sensitivity to that variable (see table). We can conclude from this that the Evans equation works well in Windy Bay for predicting TA in that the sensitivity in pCO_2 is low, but the same is not the case for Kalsin Bay. This is perhaps not surprising given Fig. A1.

Location	**TA residual (µmol kg^{-1})**	**pCO_2 residual (µatm)**
Windy Bay	-15.22 ± 9.85	3.76 ± 2.47
Kalsin Bay	-51.65 ± 24.32	-140.53 ± 58.49

Therefore, we created a new linear relationship using the discrete spatial survey samples for Kalsin Bay only. While we would have wished for a larger training set, we used 6 of the 9 samples to create the equation and the last three to perform the same exercise as above to confirm its accuracy.

Location	**TA residual (µmol kg^{-1})**	**pCO_2 residual (µatm)**
Kalsin Bay	-9.62 ± 4.74	-15.40 ± 6.79

In Kalsin Bay, CO_2 sensors were calibrated using discrete TA bottle samples, so the site-specific TA relationship was not required for estimating the CO_2 timeseries or the later air-sea flux estimate. In contrast, for Windy Bay, where only pH sensors were deployed, the Evans et al. (2015) relationship was necessary to estimate TA from salinity in order to estimate pCO_2, and it appears from the sensitivity analysis above that the Evans relationship is a good proxy. We do, however, need to use the Kalsin Bay-specific relationship for the decomposition analysis. We have explained this new method in lines 139-144 and in a new Appendix section.

Fig A3: Not sure why the inside/outside values were divided rather than subtracted for FCO2 values (was not explained on p. 11 either).

This figure is meant to represent the ratio between the inside and outside moorings to highlight the multiplicative changes in air-sea CO_2 exchange at the kelp farm. We wanted to demonstrate that the difference between the inside and outside farm increases during spring, especially as the system enters net autotrophy. We do see the value in subtracting the values, which is why that is how we calculated the net FCO2. We have added to the results on line 273 the reasoning for this to increase clarity.