nature portfolio

Peer Review File

Alkalinity-enhanced artificial substrates modulate local pH and increase survivorship of early-stage coral recruits

Corresponding Author: Professor Vivek Prakash

This file contains all editorial decision letters in order by version, followed by all author rebuttals in order by version.

Version 0:

Decision Letter:

** Please ensure you delete the link to your author home page in this e-mail if you wish to forward it to your coauthors **

Dear Professor Prakash,

Your manuscript titled "Local alkalinity enhancement using artificial substrates increases survivorship of early-stage coral recruits" has now been seen by 3 reviewers, whose comments are appended below. You will see that they find your work of some potential interest. However, they have raised quite substantial concerns that must be addressed. In light of these comments, we cannot accept the manuscript for publication, but would be interested in considering a revised version that fully addresses these serious concerns.

We hope you will find the reviewers' comments useful as you decide how to proceed. **In particular, we would like to ask you to address the concerns on the following editorial thresholds for further considerations in Communications Earth & Environment:**

(a) Present compelling new evidence into the role of artificial alkalinity enhancement in the survival of early stage coral recruits.
(b) Provide clear interpretation of the data, in view of the issues highlighted by the reviewers regarding experimental designs (e.g., flume design, design of the biological assay),
(c) Consider the possibility for additional measurements,
(d) Re-frame the text, so your discussion and conclusions are supported by your findings, and
(e) Consider the recommended statistical approaches.

Should additional work allow you to address these criticisms, we would be happy to look at a substantially revised manuscript. If you choose to take up this option, please either highlight all changes in the manuscript text file, or provide a list of the changes to the manuscript with your responses to the reviewers.

When resubmitting, please provide a point-by-point response to the reviewers' comments. Please submit your responses as a separate file, distinct from your cover letter where you can add responses to the Editors' comments that you do not want to be made available to the reviewers. Word files are preferred. We recommend that any figures, tables or graphs that are included in the response to reviewers are also included in the main article or Supplementary Information.

Please bear in mind that we will be reluctant to approach the reviewers again in the absence of substantial revisions.

If the revision process takes significantly longer than three months, we will be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

We are committed to providing a fair and constructive peer-review process. Please do not hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the reviewers' comments with a list of your changes to the manuscript text (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and any completed checklist:

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Please do not hesitate to contact us if you have any questions or would like to discuss the required revisions further. Thank you for the opportunity to review your work.

Best regards,

Nadine Schubert, PhD
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Somaparna Ghosh, PhD
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REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Ruszczyk et al., present a very interesting study where they investigate local changes in pH produced by carbonate and bicarbonate enhanced settlement tiles in a unidirectional flume design. The results could be impactful for coral reef restoration, however there are a few notable issues within their manuscript that impair the interpretation of the results. I would request more clarity on these points or significant changes in these areas before publication.

The first major issue is the authors frame the entire manuscript as local alkalinity enhancement, however total alkalinity is never measured. Since this work requires microelectrodes to measure the local changes in the boundary layer, it is not surprising they cannot measure TA as TA “sensors” only reached the market in the last 6-12 months and have not yet been proven reliable. While it is a reasonable hypothesis that the observed changes in pH are indicative of local alkalinity changes, there are separate drivers of these metrics and they can become decoupled from one another. Similarly, there does appear to be a strong treatment effect which would suggest that the “chemistries” are the result, however there was also no data presented to demonstrate that it was carbonate or bicarbonate dissolving into the water from these tiles. This latter critique is a much smaller issue than the former as these chemistry changes were the only additions made, however it is another case wherein the authors are using pH as a proxy for a separate measurement. As previously stated herein, it would be fair to hypothesize in the discussion that it is (bi)carbonate dissolving into the water and the alkalinity is changing but without direct measurement, it is not appropriate to frame all of the effects as responses to alkalinity.

Below I have highlighted multiple sections where this is a large issue:

Title: Local alkalinity enhancement using artificial substrates increases survivorship of early-stage coral recruits
I would suggest something similar to: “Alkalinity enhanced artificial substrates modulate local pH environments and increase survivorship of early-stage coral recruits”
The first two sentences of the final paragraph in the introduction are framed in a way to suggest that the alkalinity of the water is changing.
Figure 1a is stating its a change in alkalinity.
The second major issue with this manuscript is because of their flume design with unidirectional flow. Flow in natural coral reef ecosystems tends to be oscillatory, following surge and wave patterns, not unidirectional as the flume in the current study uses. In an oscillatory environment, laminar flow is interrupted and changes how flow around non-flat surfaces interact which is exceptionally important when interpreting how substrates with divots differ from flat surfaces.

“There were minimal differences in the flow above the tiles between these two surface topographies with regards to velocity and vorticity, indicating that there is no breakdown in the laminar boundary layer…”
This is likely only true under unidirectional laminar flow circumstances as Levenstein et al., (2022) showed.
The survivorship data from the divotted substrates could be negatively influenced by this unidirectional flow. Within the current study, the authors suggest that the divots create micro-environments where there is little water movement and interaction with the main body of water. This might only be true for unidirectional flow as stated above, in a wave-driven flow regime, more eddies are formed in divotted or textured surfaces. These eddies would allow for greater mixing of these areas. I imagine a lot of the mortality in the divots is the result of this issue and likely the artificial creation of anoxic zones which might not be present in different flow regimes and on coral reefs.

Results:
Throughout the manuscript the authors present p-values but it would be helpful to include what tests procured the p-values to more easily understand the results.

Results – pH
The authors state: “The laminar flows in flumes were comparable to that encountered on a coral reef ($P < 0.0001$, Figure 2a-c)…” There are two issues with this statement:
Figure 2 never presents any comparison of flow rates to natural coral reef flow regimes so it is unclear what they are referring to.
See above critiques on unidirectional flow.

“ Within the laminar boundary layer, all AE tiles significantly increase pH…” No p-value is reported.

“Linear regressions were fitted to the change in pH for the different chemistries versus tile age…”, Please add “in flow conditions” as the previous paragraphs are comparing flow and non-flow environments so its important context for the reader to know that now you are only talking about flow systems.

The sentences “Three out of the eight total regressions… did not change with the respect to time.” and the following sentences at the beginning of the next paragraph “Data points were grouped by chemistry…” seem to be out of order as these are talking about the same figure and conditions. It should probably be restructured to have the introduction sentence “Data points were grouped…” Before you talk about the regressions.

“Three out of the eight total regressions were significant indicating the declining effectiveness of tiles with no additions…” In the supplemental table supplied, all of the slopes are positive which would be a positive relationship. Looking at the figures, the slopes do indeed appear to be negative which suggests the supplemental table is not accurately reporting the slopes.

“All tiles, except for those with no additions, had an average pH higher than baseline at the surface of the tile and within the laminar boundary layer”. The previous sentence states baseline pH is the pH without any tiles, however this data is never presented in a main or supplemental figure. I understand that the main figures are ΔpH values from the baseline but it would be extremely helpful to see these comparisons in the supplement.

“Overall, these aging tests showed that the AE tiles could elevate pH within the fluid boundary later by…”
This statement is only true for 1% additions, please be specific here.
Correct “later” to “layer”

“There were minimal differences in the flow above the tiles between these two surface topographies with regards to velocity and vorticity, indicating that there is no breakdown in the laminar boundary layer…”
More information in figure S5 is needed. From the heatmap it does appear there is a huge difference in the color above the flat vs. divot substrates. Without a more thorough description in the supplement, this finding does not seem to be supported.

Figure 2g is missing error bars.

Results – Settlement, survivorship, and growth
“per tile was analyzed on day 0 using an ANOVA with chemistry and topography as factors”. Was this two, one-way ANOVAS (% settlement ~ chemistry + topography) or a two-way ANOVA (% settlement ~ chemistry*topography).

What is the density of settlers in the divots? There is a good chance the density is way too high. It might be that there was

only low survival in divots that had high density.

The whole paragraph does not include any statistics. The authors state chemistry and topography were significant predictors of coral growth in a GLM. However, in the text and supplemental table, there was never any R2 or p-value for the whole model. It might also be helpful to compare Δchanges using an ANOVA structure. I assume the reason why this wasn't done was because there were so few survivors by the final time point (e.g. too low n). However, any more statistical information you could supply on this would be extremely helpful for the field and interpretation. Additionally a general additive model (GAM) might be more helpful in this interpretation as it can deal with low n per time point more effectively than an ANOVA or linear model.

Methods:
The authors state that there were tests for data normality, however they never state what those tests were. This is very concerning as a lot of these tests are not always appropriate for biologic data as they have underlying assumptions which can make them inappropriate for data. e.g. many cannot handle too high n values and will not give reliable results. A better metric might be to present qqplots in the supplement. However, this might actually help the authors in some cases such as the results suggested in Figure 3d–e. The authors state that these are not significant but based on the error bars, this seems unlikely to be true. The reason could be that the data isn't in-fact gaussian. Probably worth trying to transform the %survivorship data with an angular transformation (asin(sqrt(%survivorship)) or something similar.

Reviewer #2 (Remarks to the Author):

This is a well-written manuscript that presents a novel and relevant approach to enhancing coral recruitment success. The topic is timely, and the work clearly reflects a significant amount of effort. The integration of microsensor-based pH characterization, recruitment substrate fabrication, hydrodynamic modeling, and coral larval assays is impressive, and the technical execution—including the flow setup, modeling, and microsensing—appears to be very strong.

My main concern lies with the experimental design of the biological assay involving coral settlement and survivorship. This experiment, which is central to the study's ecological relevance, used tiles that were conditioned for 30 days with live rock, CCA, and biofilms. While this conditioning step is ecologically realistic, it introduces an active benthic community that likely alters the local microenvironment, particularly pH and dissolved oxygen. However, no pH measurements were performed during the biological experiment itself, which limits the ability to link the observed effects on coral survivorship to substrate-driven alkalinity enhancement.

It is well established that biofilms, algae, and CCA can significantly influence pH at the substrate interface, and in this case, they may have masked, modified, or even amplified the chemical signal of the tile itself. Thus, without pH measurements during the biological assay, the interpretation that increased survivorship results directly from carbonate additions remains speculative.

Additionally, it would be important to consider whether the biofilm or benthic community composition differed across tile chemistries. Subtle shifts in microbial or algal assemblages due to tile-induced pH changes could be a key mechanism underlying coral performance. While full microbial characterization may be beyond the scope of this study, even basic data (e.g., biofilm biomass, microscopy, or visual differences) would help disentangle chemical vs. biological effects.

At a minimum, I recommend the authors include pH measurements on the biologically conditioned tiles over time. If this is not possible, then a detailed discussion why this data was not collected is needed.

Reviewer #3 (Remarks to the Author):

Summary: Ruszczyk et al. investigated the use of cementitious substrates incorporating sodium carbonate or sodium bicarbonate for providing local "alkalinity enhancement" (AE) of coral larval settlement and post-settlement growth & survival. These substrates were prepared with two different topographies and tested in a flume tank to assess their ability to modify the pH of the laminar boundary layer. Finally, settlement, growth, and survival experiments with O. faveolata larvae were conducted on the substrates and trends based on substrate chemistry and topography were found. The scope of the work is impressive and the results are interesting. I think this paper will contribute to the growing effort to investigate inorganic aspects of larval settlement and optimize the physicochemical properties of settlement substrates. However, I thought there were quite a few unclear points in the manuscript. I include some specific questions and suggestions below for its improvement:

Introduction
-I enjoyed the thorough background on coastal resilience in the introduction, but felt that there was not enough space dedicated to previous work on artificial restoration substrates. Recent work on local (bio)chemical enhancement of substrates, in particular, should be included, for example:
Tebben et al., Scientific Reports 5, 10803 (2015).
Levenstein et al., ACS Sustain. Chem. Eng. 10, 3960–3971 (2022).
Yus et al., Ecological Engineering 202, 107236 (2024).

Results
-Consider using “stagnant conditions” or something similar instead of “Stagnant flow conditions” throughout the paper. Use of the word “flow” could be confusing.
-In your boundary layer measurements (Fig. 1d), how do you know there is no overlap between the boundary layer created by your substrate and boundary layers further upstream? In other words, is your flow fully developed upon reaching the start of your substrate?
- In “Tile Verification: Diffusion and Advection”: I do not understand where you got your stated delta pH ranges based on the data presented in Figure 2a-c. In stagnant conditions, the 2% carbonate tile goes to >0.5 pH units in 6 hr (>0.4 units) and likewise, in flowing conditions, after around 30 min, there were spikes >0.5 pH units (>0.1 units). Maybe you are referring to the results of the 12-week aging presented in Figure 2e, but I think you should keep these results separate and make it explicit in the text to which time (<=6h or 12 wk) you are referring.
- In “Tile Verification: Diffusion and Advection”: I do not see in Figure 2a-c what would support the statement “the laminar flows in the flumes were comparable to that encountered on a coral reef” and the p value provided.
-Also in the aging experiments, I do not understand your statements referring to the effectiveness of tiles in changing pH over time. For example, you say the 1% bicarbonate and carbonate additions did not “change over time”, yet in Fig. 2d, these are the treatments with linear fits showing positive slopes and an increase in pH over time.
- In “Chemistry and Topography”, please include here the divot size and geometry for the reader (in addition to in the Materials & Methods) Also, I disagree with your characterization that there is no recirculation over the divots due to the flow in the flume being laminar. There is certainly recirculation within your divots; most likely you have a skimming-type flow. If your flow was near the transition between laminar and turbulent flow and the roughness features were large enough, it could be possible for surface roughness to “trigger” turbulence above the substrate. However, it is not the fact that your flow starts laminar that prevents this from happening. It is also not surprising that the pH remains higher in the divots as there is low net flow in or out. Yet, while there is protection from the bulk flow, there is still an isolated recirculatory flow within the divots. This flow is much lower in speed than the free-stream, but not zero.
-Please specify in the Results section that settlement experiments were not conducted in the flumes, but that the growth and survivorship experiments were. It is important to the interpretation of the results to know which experiments were conducted under flowing or stagnant conditions.

Discussion
-How is delta pH calculated? Is it the change in pH compared to the initial/ambient condition of your seawater before it arrives in the flume or is it the difference between the pH at the surface and above the surface of each substrate? If it is the former, please skip to my next point, if it is the latter, I do not think this is a good way to present the pH. It makes the AE ability of the 2% carbonate substrate appear artificially worse than the 1%, when it is in fact the opposite. The delta pH should represent the real AE experienced by the settlers regardless of the change in pH above the boundary layer.
-How many times did you repeat the pH measurement for the 2% carbonate tiles? To me, it has to be an error or outlier if delta pH was calculated in the first way (see above comment). I do not agree with your reasoning in the discussion for why it increases the pH less than the 1% tiles (but I am confused by what you say: see my point above). In a surface area-limited situation, it is possible for the 1% and 2% treatments to have the same leaching rate into the water column, but it is not possible for the 2% to leach less (unless the 2% composition results in a much different porosity/surface structure of the cement, which is unlikely). Then assuming equal or greater leaching, the added advection would not affect the 2% tiles any differently than the 1%. It is also troubling as your survival results show that the 2% treatment had the largest effect. This means that if your pH results are indeed accurate, the reason for the increased survivorship is not related to boundary layer pH, undercutting the main story of your paper (unless you calculate delta pH in the second way).
-In the calculation of the Pe number, I think the height of the flume is more relevant than its length in your case, because you are interested in the transport of ions perpendicular to the substrate. This is different than the classical conception of the Pe number for species diffusing and reacting along a pipe flow.
-When you say the “initial settlement of O. faveolata was not strongly affected by chemistry,” I think you should make it clear that it is in regards to the two chemicals investigated here, and not a general statement. Yus et al. (2024) showed that O. faveolata larvae settlement preference was affected by alkaline earth ions leached from substrates.
-You seem to imply that the larvae preferred your divots due to attachment point theory (APT), but 3 mm is much larger than optimal topographical features predicted by ATP, which is focused on almost perfect matching between larval diameter and the feature width. When the larvae are not able to touch the divot on all sides, the contact area is lower than APT. Your result is in line with what is reported in ref. 17.
-Similar to a point above, while there is little flow in/out of the divots, there is likely still recirculatory flow within the divot as long as the settlers are not occupying too much of the internal volume. But you are right that this decreased mass transport in/out of the divot could have a negative effect on larvae (e.g., lack of nutrients and waste removal).

Materials and Methods
-Please provide the supplier of the Portland limestone cement and the chemical additives.
-For the pH measurements, can you give some information on the sensitivity and uncertainty you expect? A result of around 0.1 pH seems quite precise to expect from an electrochemical probe under flowing conditions. Normally, these types of probes have some equilibration time (for stabilizing the ion flow through the membrane or capillary), so in a dynamic system within a thin boundary layer (as opposed to a well-mixed beaker), do you think the measurement is reliable enough to be significant at 0.1 units?
-Your Reynolds number in the recirculating configuration is quite high, near the critical Re. So you may be in more of a transitional flow regime than a purely laminar flow.
-Why did you sometimes place the pH probe 3.26 mm above the surface and other times 1 cm? This was not clear in the Results, only the 3.26 mm distance was mentioned there.
-I think the Methods could be organized a bit better, for instance, there are several places that mention different aspects of

the pH microelectrodes, which could be grouped together. More sub-headings would also help.
-Please provide a brief description of or reference for your gamete collection and fertilization protocol.
-Please mention in the Results section that a CCA cue was added and that settlement preferences were not in response to AE chemistry-based cues alone.
-How were outliers identified in the PIV datasets?

References
-There is a typo in ref. 17: “larvael”

Mark A. Levenstein, CEA, Université Paris-Saclay

Version 1:

Decision Letter:

** Please ensure you delete the link to your author home page in this e-mail if you wish to forward it to your coauthors **

Dear Professor Prakash,

Your manuscript titled "Alkalinity-enhanced artificial substrates modulate local pH and increase survivorship of early-stage coral recruits" has now been seen by our reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment.

** We have noticed that you have added two new additional authors Veronica Paul and Peter K. Swart. Please write the justification for these two additions at the top of the rebuttal letter (response to the reviewers' comments-document)**

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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Please outline your response to each request in the right hand column. Please upload the completed table with your manuscript files as a Related Manuscript file.

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Best regards,

Somaparna Ghosh, PhD
Associate Editor,
Communications Earth & Environment
Consulting Editor,
Communications Sustainability

Nadine Schubert, PhD
Editorial Board Member- Communications Earth & Environment

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

The authors have been very thorough in responded to my concerns and believe their revised manuscript should be accepted.

Reviewer #2 (Remarks to the Author):

The revised manuscript adequately addresses all previous comments and the manuscript appears now ready for publication.
-Daniel Wangpraseurt

Reviewer #3 (Remarks to the Author):

I am generally satisfied by the author’s responses to my questions and their extensive changes to the manuscript and can recommend publication. Although, I still find the 2% carbonate result difficult to interpret and the current explanation provided in the manuscript insufficient. I would prefer that a version of the answer given in the author response, regarding possible unknown chemistry/mineralization effects at the substrate surface, be used in the text instead of the current “advection hypothesis”. This is presented as self-evident on line 325, but in my opinion, there is no clear physical reason that advection would affect the 1% and 2% tiles differently (even considering the referenced aging experiment data), unless the increased mixing induces chemical/mineralogical changes that then have a cascading effect on the pH modification or even the pH measurement itself. The Ca, DIC, and TA data would also be interesting to note here. Despite this one lingering issue, I am overall impressed by the paper and the significant work on the manuscript revision.

One other small thing I noted on line 313 is that I believe the water depth should be 0.1 m and not 0.01 m.

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Version 2:

Decision Letter:

Dear Professor Prakash,

We are delighted to accept your manuscript titled "Alkalinity-enhanced artificial substrates modulate local pH and increase survivorship of early-stage coral recruits" for publication in Communications Earth & Environment. Thank you for choosing to publish your interesting work with us.

We would like to encourage you to write a "Behind the paper" blog post for our Earth & Environment Community website. You will receive an invitation letter.

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Providing great service is very important to us. We would greatly appreciate any comments you have about your experience at Communications Earth & Environment. We look forward to publishing your paper, and we hope to work with you again in the future.

Best regards,
Nadine Schubert, PhD
Editorial Board Member
Communications Earth & Environment

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Associate Editor,
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Response to Reviewers

We thank the editors and the reviewers who have evaluated at our manuscript, and we are delighted to see that they find the work very interesting, impactful, novel, relevant, timely, impressive, and strong. We also thank the reviewers for the critical feedback they have provided. We have addressed the concerns they highlighted and made necessary changes to the manuscript accordingly. Our detailed line by line answers follow.

The Reviewers' comments are repeated here below, followed by the authors' response in blue color. Any line references made in the response refer to the new, continuous line numbering in the revised manuscript.

Reviewer 1:

1. Ruszczyk et al., present a very interesting study where they investigate local changes in pH produced by carbonate and bicarbonate enhanced settlement tiles in a unidirectional flume design. The results could be impactful for coral reef restoration, however there are a few notable issues within their manuscript that impair the interpretation of the results. I would request more clarity on these points or significant changes in these areas before publication.

We thank the reviewer for their positive and encouraging comments, constructive feedback, and for supporting our work.

2. The first major issue is the authors frame the entire manuscript as local alkalinity enhancement; however total alkalinity is never measured. Since this work requires microelectrodes to measure the local changes in the boundary layer, it is not surprising they cannot measure TA as TA "sensors" only reached the market in the last 6-12 months and have not yet been proven reliable. While it is a reasonable hypothesis that the observed changes in pH are indicative of local alkalinity changes, there are separate drivers of these metrics and they can become decoupled from one another. Similarly, there does appear to be a strong treatment effect which would suggest that the "chemistries" are the result, however there was also no data presented to demonstrate that it was carbonate or bicarbonate dissolving into the water from these tiles. This latter critique is a much smaller issue than the former as these chemistry changes were the only additions made, however it is another case wherein the authors are using pH as a proxy for a separate measurement. As previously stated herein, it would be fair to hypothesize in the discussion that it is (bi)carbonate dissolving into the water and the alkalinity is changing but without direct measurement, it is not appropriate to frame all of the effects as responses to alkalinity.

We appreciate the reviewer's insightful observation and concur with their assessment. In response, we now performed additional experiments to measure the Total Alkalinity (TA), Dissolved Inorganic Carbon (DIC) and Calcium (Ca) from the tiles under stagnant conditions in fixed volumes. All the AE tiles showed decreased values of TA, DIC, and Ca, compared to the control tiles with no enhancement (new Figure S4 and Table S6). In all cases, visible precipitates were observed on the tile surfaces. The reduction in TA, DIC and Ca likely reflects the leaching of HCO_3^- and CO_3^{2-} ions from the tiles and their subsequent reaction with Ca^{2+} to form solid $CaCO_3$. In contrast, under advective flow conditions in the flume experiments, continuous flows would have minimized precipitation on the tile surfaces. It is therefore plausible that some of the $CaCO_3$ formed in the flumes was incorporated into the coral skeletons, contributing to the enhanced lateral and vertical skeletal growth observed.

There are several important questions related to this thread on investigating the exact relationship TA and pH that are complicated, and we will plan to address them in future work. But for now, we have now reframed the manuscript emphasizing that the AE tiles modulate the local pH, and lead to increased coral recruit survivorship. We thank the reviewer for raising this important point.

3. Below I have highlighted multiple sections where this is a large issue:

Title: Local alkalinity enhancement using artificial substrates increases survivorship of early-stage coral recruits
I would suggest something similar to: “Alkalinity enhanced artificial substrates modulate local pH environments and increase survivorship of early-stage coral recruits”

We agree with the reviewer and have modified the title as they suggested.

4. The first two sentences of the final paragraph in the introduction are framed in a way to suggest that the alkalinity of the water is changing. Figure 1a is stating it’s a change in alkalinity.

We agree with the reviewer and have modified these sentences and Figure 1a as they suggested. (Line 145)

5. The second major issue with this manuscript is because of their flume design with unidirectional flow. Flow in natural coral reef ecosystems tends to be oscillatory, following surge and wave patterns, not unidirectional as the flume in the current study uses. In an oscillatory environment, laminar flow is interrupted and changes how flow around non-flat surfaces interact which is exceptionally important when interpreting how substrates with divots differ from flat surfaces.

The reviewer makes a strong point – we agree that the unidirectional flow is not the most representative flow the corals experience. We opted for unidirectional laminar flow as it would remain steady throughout the duration of the experiment (~135 days), removing the confounding variable of unsteady turbulence affecting the chemical landscape, and allowing us to focus solely on the effect of the AE tiles with minimal change in flow. We plan to expand and continue these experiments in an oscillatory flow condition going forward as this would be an important future direction. In the discussion section of the manuscript, we now acknowledge unidirectional flow as a potential limitation of our current study, and that an important next step in this research would be to investigate oscillating flow conditions. (Lines 334-340)

6. “There were minimal differences in the flow above the tiles between these two surface topographies with regards to velocity and vorticity, indicating that there is no breakdown in the laminar boundary layer…”
This is likely only true under unidirectional laminar flow circumstances as Levenstein et al., (2022) showed.

Yes, the reviewer is right. These are indeed the conditions we were hoping to achieve in our flumes for the sake of a steady flow environment. In our revision, we have acknowledged that we likely do not have pure laminar flow in our flumes and that there is likely weak recirculation arising from a breakdown of the boundary layer (Lines 224-230), although this assumption is driven by theory and not our experimental observations. We have changed our verbiage here to

indicate that the flow is “fairly” laminar, although small-scale disturbances within the divots are likely, but were not detected in the current experimental settings.

7. The survivorship data from the divotted substrates could be negatively influenced by this unidirectional flow. Within the current study, the authors suggest that the divots create micro-environments where there is little water movement and interaction with the main body of water. This might only be true for unidirectional flow as stated above, in a wave-driven flow regime, more eddies are formed in divotted or textured surfaces. These eddies would allow for greater mixing of these areas. I imagine a lot of the mortality in the divots is the result of this issue and likely the artificial creation of anoxic zones which might not be present in different flow regimes and on coral reefs.

We agree that the unidirectional flow likely had a negative impact on the corals that settled in the divots. We explicitly hypothesize that the lack of flow within the divots resulting from the unidirectional flow may have created an anoxic environment for the corals. (Lines 356-365)

8. Results:
Throughout the manuscript the authors present p-values but it would be helpful to include what tests procured the p-values to more easily understand the results.

We agree with the reviewer and have restructured the way we report our results throughout this section to include the test(s) used.

9. Results – pH
The authors state: “The laminar flows in flumes were comparable to that encountered on a coral reef ($P < 0.0001$, Figure 2a-c)…” There are two issues with this statement:
Figure 2 never presents any comparison of flow rates to natural coral reef flow regimes so it is unclear what they are referring to.
See above critiques on unidirectional flow.
“ Within the laminar boundary layer, all AE tiles significantly increase pH…” No p-value is reported.

We understand the reviewer’s concerns and agree. Here, we intend to model near-substrate laminar boundary layer flows (cm scale) within a coral reef framework by matching flow speeds to those observed within a coral reef canopy (1-2 cm s^{-1}) (References 1-4 below). We have now included this clarification in the revised manuscript. (Lines 174, 505-507, 546-548).

References:
[1] S. Chang, C. Elking, M. Alley, J. Eaton, S. Monismitha, Flow inside a coral colony measured using magnetic resonance velocimetry, Limnol. Oceanogr. 54 (2009) 1819–1827.
[2] Pacherres, C.O., Ahmerkamp, S., Schmidt-Grieb, G.M., Holtappels, M. and Richter, C., 2020. Ciliary vortex flows and oxygen dynamics in the coral boundary layer. *Scientific reports*, *10*(1), p.7541.
[3] M.A. Reidenbach, J.R. Koseff, S.G. Monismith, Laboratory experiments of fine-scale mixing and mass transport within a coral canopy, Phys. Fluids 19 (2007) 075107.
[4] Enochs, I.C., Studivan, M.S., Kolodziej, G., Foord, C., Basden, I., Boyd, A., Formel, N., Kirkland, A., Rubin, E., Jankulak, M. and Smith, I., 2023. Coral persistence despite marginal conditions in the Port of Miami. *Scientific reports*, *13*(1), p.6759.

10. “Linear regressions were fitted to the change in pH for the different chemistries versus tile age…”, Please add “in flow conditions” as the previous paragraphs are comparing flow and non-flow environments so its important context for the reader to know that now you are only talking about flow systems.

We have made the suggested change. (Line 186)

11. The sentences “Three out of the eight total regressions… did not change with the respect to time.” and the following sentences at the beginning of the next paragraph “Data points were grouped by chemistry…” seem to be out of order as these are talking about the same figure and conditions. It should probably be restructured to have the introduction sentence “Data points were grouped…” Before you talk about the regressions.

We agree with the reviewer. Now we begin the paragraph discussing linear regressions with a statement that the data was grouped with respect to the four different chemistries and the two different sample heights. (Lines 183-194)

12. “Three out of the eight total regressions were significant indicating the declining effectiveness of tiles with no additions…” In the supplemental table supplied, all of the slopes are positive which would be a positive relationship. Looking at the figures, the slopes do indeed appear to be negative which suggests the supplemental table is not accurately reporting the slopes.

We see the reviewer’s concern and provide clarification. All our statistics were run on H^+ concentration (a linear scale), rather than change in pH (a log scale) so that we could use linear statistics. We have clarified this analysis in the Results section (Lines 183-194).

The positive slopes in Supplementary Table S2 are for the analysis using H^+, which increases as pH decreases. We have also compiled New Supplemental Figure S1 which mimics Figure 2 but reports tile chemistry in ΔH^+ concentrations.

13. “All tiles, except for those with no additions, had an average pH higher than baseline at the surface of the tile and within the laminar boundary layer”. The previous sentence states baseline pH is the pH without any tiles, however this data is never presented in a main or supplemental figure. I understand that the main figures are ΔpH values from the baseline but it would be extremely helpful to see these comparisons in the supplement.

We clarify our statistical analysis here to state we are testing to see if ΔH^+ (ΔpH) is equal to zero. If there were no change in the environmental chemistry, then the delta values would be equal to zero. These statistical results are presented in Supplementary Table S3. (Lines 204-211)

14. “Overall, these aging tests showed that the AE tiles could elevate pH within the fluid boundary later by…”
This statement is only true for 1% additions, please be specific here.
Correct “later” to “layer”

We have corrected this typo and included the specific statement for 1% additions. (Lines 211-213)

15. “There were minimal differences in the flow above the tiles between these two surface topographies with regards to velocity and vorticity, indicating that there is no breakdown in the laminar boundary layer…”
More information in figure S5 is needed. From the heatmap it does appear there is a huge difference in the color above the flat vs. divot substrates. Without a more thorough description in the supplement, this finding does not seem to be supported.

We note the reviewer’s concern. In the heatmap of Figure S3 (formerly S5), we are highlighting that there are almost no differences (close to zero) in flow fields immediately above the divots. There are indeed noticeable changes in color in the bulk fluid in the center of the measurement volume, however this magnitude (~0.2 cm s^{-1}) is one order of magnitude smaller than the actual mean flows (1-2 cm s^{-1}) in the bulk. We now clarify this point in the revised caption for Figure S3.

16. Figure 2g is missing error bars.

Interestingly, it is not. Our standard error in this set of experiments was below 0.002 for all data in pH units – a much smaller range than can be displayed on the *ΔpH* graph at the given scale. For alternate versions of this figure, we have included a ΔH^+ version (New Supplementary Figure S1) where the error bars are more visible.

17. Results – Settlement, survivorship, and growth
“per tile was analyzed on day 0 using an ANOVA with chemistry and topography as factors”. Was this two, one-way ANOVAS (% settlement ~ chemistry + topography) or a two-way ANOVA (% settlement ~ chemistry*topography).

We have since updated our statistical analysis considering suggestions from all the reviewers. Here, and throughout the Results section, we have included the specific test in parentheses before reporting the results of the test.

18. What is the density of settlers in the divots? There is a good chance the density is way too high. It might be that there was only low survival in divots that had high density.

We agree with the reviewer that there is a good chance that the settler density is way too high in the divots. In our current measurement methods, we do not have a good way to quantify density of settlers in divots since we can only reliably identify and measure one settler at a time. In future work, we will develop methods to quantify settler density.

19. The whole paragraph does not include any statistics. The authors state chemistry and topography were significant predictors of coral growth in a GLM. However, in the text and supplemental table, there was never any R2 or p-value for the whole model. It might also be helpful to compare ∆changes using an ANOVA structure. I assume the reason why this wasn’t done was because there were so few survivors by the final time point (e.g. too low n). However, any more statistical information you could supply on this would be extremely helpful for the field and interpretation. Additionally a general additive model (GAM) might be more helpful in this interpretation as it can deal with low n per time point more effectively than an ANOVA or linear model.

The reviewer suggests two approaches to refine the analysis of growth data: continuing the established ANOVA structure used to analyze settlement and survivorship, and/or using a

generalized additive model (GAM) rather than the original generalized linear model (GLM) to analyze trends. During our revisions, we performed both analyses. Our manuscript has been edited and updated to only include the ANOVA structure analysis on growth data.

While the GAM approach did perform better than the original GLMs, the best fitted GAMs relied heavily on a random effect term and did not reveal any factor (chemistry, topography, chemistry:topography, chemistry:time, topography:time, chemistry:topography:time) other than time spent in the flumes to be a consistent model term when predicting growth in any dimension (vertical, areal, or volumetric). These findings lead us to believe that while juvenile coral growth may be affected by the substrate, statistically, our findings are inconclusive as we are likely missing variables which have a greater contribution to coral growth. We hope to readdress this enhanced growth hypothesis in future work, which would need to refine the experiments to better measure growth in response to substrate factors (higher sample size, better control, and quantification of alkaline environment).

Because the GAMs and the ANOVA structure approach both result in a similar statistical story (alkalinity enhanced substrates have a minimal to inconclusive effect on growth), we opt to feature the ANOVA analysis in the manuscript for sake of conciseness and ease of understanding, as the growth analysis now has a parallel structure to all analyses in the paper. (Lines 261-277)

20. Methods:
The authors state that there were tests for data normality, however they never state what those tests were. This is very concerning as a lot of these tests are not always appropriate for biologic data as they have underlying assumptions which can make them inappropriate for data. E.g. many cannot handle too high n values and will not give reliable results. A better metric might be to present qqplots in the supplement. However, this might actually help the authors in some cases such as the results suggested in Figure 3d–e. The authors state that these are not significant but based on the error bars, this seems unlikely to be true. The reason could be that the data Isn't In fact gaussian. Probably worth trying to transform the %survivorship data with an angular transformation (asin(sqrt(%survivorship)) or something similar.

We thank the reviewer for their detailed feedback on the statistics and have implemented their suggestions. We have prepared new Supplementary Tables S7 and S8 that report the testing of assumptions. As a result of this – and other important suggestions to our statistical methods – we have consequently updated our statistical analysis to report specific instances when parametric versus nonparametric analyses were conducted on the data.

Concerning the comment regarding Figure 3d-e: we clarify in the caption that we are aggregating by location on the tile, rather than topography type (Lines 990-992). This aggregation over settlement location on a tile, rather than on tile topography type may have not been clearly presented, leading to the mistranslation of what is insignificant and hence, grouped, versus what is displayed in Figure 3d-e.

We have added schematic panels to Figure 2 and have updated our legend and caption in Figure 3 to clarify topography type versus settlement location.

We have applied the suggested angular transformation for statistics on all percentage data in the manuscript. (Lines 770-772)

Cumulatively, this has resulted in changes to the following figures and tables:

- Main Manuscript
    - Figure 2
        - Inserted panels of sensor position to clarify “above”, “surface”, and “divot” with respect to tile topography.
    - Figure 3
        - Updated legend to better present analysis by chemistry, topography, and location
        - Updated panel to correspond to revised analysis for
            - Initial number of settlers ~ Chemistry, Topography
            - Total survivorship ~ Chemistry, Topography, Chemistry X Topography
- Supplementary Tables
    - Updated Supplementary Table S5 to correspond to revised analysis
    - Addition of Supplementary Tables S7 and S8 to present testing of normality and equal variance

In summary, we view this study as the first proof-of-concept demonstration of AE tile–based local pH modulation for improving coral recruit survivorship. We recognize the reviewer’s important points regarding the TA–pH relationship and have incorporated new measurements and clarifications accordingly. In future work, we plan to expand on this foundation by directly quantifying TA–pH coupling and testing AE tile-based performance under oscillatory flow conditions. We believe that the new data, revised analyses, and clearer framing presented here have substantially strengthened the manuscript.

Reviewer 2

1. This is a well-written manuscript that presents a novel and relevant approach to enhancing coral recruitment success. The topic is timely, and the work clearly reflects a significant amount of effort. The integration of microsensor-based pH characterization, recruitment substrate fabrication, hydrodynamic modeling, and coral larval assays is impressive, and the technical execution—including the flow setup, modeling, and microsensing—appears to be very strong.

We thank the reviewer for their positive and encouraging comments, constructive feedback, and for supporting our work.

2. My main concern lies with the experimental design of the biological assay involving coral settlement and survivorship. This experiment, which is central to the study's ecological relevance, used tiles that were conditioned for 30 days with live rock, CCA, and biofilms. While this conditioning step is ecologically realistic, it introduces an active benthic community that likely alters the local microenvironment, particularly pH and dissolved oxygen. However, no pH measurements were performed during the biological experiment itself, which limits the ability to link the observed effects on coral survivorship to substrate-driven alkalinity enhancement.

It is well established that biofilms, algae, and CCA can significantly influence pH at the substrate interface, and in this case, they may have masked, modified, or even amplified the chemical signal of the tile itself. Thus, without pH measurements during the biological assay, the interpretation that increased survivorship results directly from carbonate additions remains speculative.

Additionally, it would be important to consider whether the biofilm or benthic community composition differed across tile chemistries. Subtle shifts in microbial or algal assemblages due to tile-induced pH changes could be a key mechanism underlying coral performance. While full microbial characterization may be beyond the scope of this study, even basic data (e.g., biofilm biomass, microscopy, or visual differences) would help disentangle chemical vs. biological effects.

At a minimum, I recommend the authors include pH measurements on the biologically conditioned tiles over time. If this is not possible, then a detailed discussion why this data was not collected is needed.

The reviewer brings up an important limitation of our current study. We rely on the assumption that the chemistry changes brought about by the AE tiles during the non-biological quantification experiments are similar to the chemistry changes with the biology present on the tiles.

We fully agree with the reviewer, and regret that it is not possible for us to carry out new experiments to include pH measurements on the biologically conditioned tiles over time, due to severe constraints with personnel, resources and funding. Hence, a biological characterization that would help to better disentangle chemical vs. biological effects is unfortunately beyond the scope of the current paper. We discuss this as a limitation in our understanding of substrate modifications and coral response, and suggest a more detailed investigation into the microenvironment of these tiles as an important next step and future direction in the understanding of AE tiles in restoration. (Lines 409-420).

Reviewer 3

1. Summary: Ruszczyk et al. investigated the use of cementitious substrates incorporating sodium carbonate or sodium bicarbonate for providing local “alkalinity enhancement” (AE) of coral larval settlement and post-settlement growth & survival. These substrates were prepared with two different topographies and tested in a flume tank to assess their ability to modify the pH of the laminar boundary layer. Finally, settlement, growth, and survival experiments with O. faveolata larvae were conducted on the substrates and trends based on substrate chemistry and topography were found. The scope of the work is impressive and the results are interesting. I think this paper will contribute to the growing effort to investigate inorganic aspects of larval settlement and optimize the physicochemical properties of settlement substrates. However, I thought there were quite a few unclear points in the manuscript. I include some specific questions and suggestions below for its improvement:

We thank the reviewer for their positive and encouraging comments, constructive feedback, and for supporting our work.

2. Introduction
-I enjoyed the thorough background on coastal resilience in the introduction but felt that there was not enough space dedicated to previous work on artificial restoration substrates. Recent work on local (bio)chemical enhancement of substrates, in particular, should be included, for example:
Tebben et al., Scientific Reports 5, 10803 (2015).
Levenstein et al., ACS Sustain. Chem. Eng. 10, 3960–3971 (2022).
Yus et al., Ecological Engineering 202, 107236 (2024).

We thank the reviewer for these great suggestions. We have expanded our background to include mention of substrate (bio)enhancements and have included these references. We have also included another recent study on artificial restoration substrates (Kundu et al., Trends in Biotechnology, 2025.) (Lines 128-139)

3. Results
-Consider using “stagnant conditions” or something similar instead of “Stagnant flow conditions” throughout the paper. Use of the word “flow” could be confusing.

We agree and have made this suggested change throughout the paper.

4. -In your boundary layer measurements (Fig. 1d), how do you know there is no overlap between the boundary layer created by your substrate and boundary layers further upstream? In other words, is your flow fully developed upon reaching the start of your substrate?

The flow in the experimental chamber of our flume is not fully developed. The boundary layer measurements shown in Fig. 1d retain a steady profile since the flow is unidirectional. Fig. 1d shows the entire experimental chamber, with the horizontal dimension being 30 cm.

5. -In “Tile Verification: Diffusion and Advection”: I do not understand where you got your stated delta pH ranges based on the data presented in Figure 2a-c. In stagnant conditions, the 2% carbonate tile goes to >0.5 pH units in 6 hr (>0.4 units) and likewise, in flowing conditions, after around 30 min, there were spikes >0.5 pH units (>0.1 units). Maybe you are referring to the results of the 12-week aging presented in Figure 2e, but I think you should keep these results separate and make it explicit in the text to which time (<=6h or 12 wk) you are referring.

We clarify the comparisons we are making to obtain these values and have reported them differently based on the reviewer's comments. In the diffusive setting, the tiles increased pH consistently over the 6 hr window, with 2% carbonate tiles increasing pH by up to 0.5 pH units. In advective settings, the change in pH over time is not as consistent. Rather than report a max value in advective conditions, we report the average ΔpH over time to see that while 1% bicarbonate and 1% carbonate tiles do not consistently increase pH, the "bursts" of pH over time are enough to result in an average increase in pH at the surface. (Lines 157-180)

6. -In "Tile Verification: Diffusion and Advection": I do not see in Figure 2a-c what would support the statement "the laminar flows in the flumes were comparable to that encountered on a coral reef" and the p value provided.

We understand the reviewer's concerns and agree. Here, we intend to model near-substrate laminar boundary layer flows (cm scale) within a coral reef framework by matching flow speeds to those observed within a coral reef canopy (1-2 cm s^{-1}) (References 1-4 below). We have now included this clarification in the revised manuscript. (Lines 174, 505-507, 546-548).

References:
[1] S. Chang, C. Elking, M. Alley, J. Eaton, S. Monismitha, Flow inside a coral colony measured using magnetic resonance velocimetry, Limnol. Oceanogr. 54 (2009) 1819–1827.
[2] Pacherres, C.O., Ahmerkamp, S., Schmidt-Grieb, G.M., Holtappels, M. and Richter, C., 2020. Ciliary vortex flows and oxygen dynamics in the coral boundary layer. *Scientific reports*, *10*(1), p.7541.
[3] M.A. Reidenbach, J.R. Koseff, S.G. Monismith, Laboratory experiments of fine-scale mixing and mass transport within a coral canopy, Phys. Fluids 19 (2007) 075107.
[4] Enochs, I.C., Studivan, M.S., Kolodziej, G., Foord, C., Basden, I., Boyd, A., Formel, N., Kirkland, A., Rubin, E., Jankulak, M. and Smith, I., 2023. Coral persistence despite marginal conditions in the Port of Miami. *Scientific reports*, *13*(1), p.6759.

7. -Also in the aging experiments, I do not understand your statements referring to the effectiveness of tiles in changing pH over time. For example, you say the 1% bicarbonate and carbonate additions did not "change over time", yet in Fig. 2d, these are the treatments with linear fits showing positive slopes and an increase in pH over time.

While it appears that the 1% bicarbonate and 1% carbonate tiles may increase pH over time, statistically speaking, they do not ($p = 0.09$). These p-values being close to, but not < 0.05 is likely representative of the high variance in the data for these tile chemistries. We include these non-significant p-values in the manuscript to highlight the analysis. (Lines 192-194)

While the regressions are not significant, indicating no change in tile effectiveness over time, we do strongly see that 1% bicarbonate and 1% carbonate tiles affect the local pH the most over the aging experiment. By comparing the average ΔpH values to an estimated mean of 0 (indicating no change in pH), we see that all the tiles, except for those with no additions significantly increased local pH. (Lines 204-213)

8. -In "Chemistry and Topography", please include here the divot size and geometry for the reader (in addition to in the Materials & Methods) Also, I disagree with your characterization that there is no recirculation over the divots due to the flow in the flume being laminar. There is certainly recirculation within your divots; most likely you have a skimming-type flow. If your flow was near the transition between laminar and turbulent flow and the roughness features were

large enough, it could be possible for surface roughness to “trigger” turbulence above the substrate. However, it is not the fact that your flow starts laminar that prevents this from happening. It is also not surprising that the pH remains higher in the divots as there is low net flow in or out. Yet, while there is protection from the bulk flow, there is still an isolated recirculatory flow within the divots. This flow is much lower in speed than the free-stream, but not zero.

We add the divot size and geometry to the Results (Lines 216-218) and clarify it in the Methods (Lines 451-455). We agree with the reviewer that there may be “skimming-type flow”. We add that there may be minimal flow in the divots resulting from boundary layer separation, but it is so small that we are unable to detect its presence using our flow quantification methods (Lines 224-230).

9. -Please specify in the Results section that settlement experiments were not conducted in the flumes, but that the growth and survivorship experiments were. It is important to the interpretation of the results to know which experiments were conducted under flowing or stagnant conditions.

We clarify this in the Results section. (Lines 246-248)

10. Discussion
-How is delta pH calculated? Is it the change in pH compared to the initial/ambient condition of your seawater before it arrives in the flume or is it the difference between the pH at the surface and above the surface of each substrate? If it is the former, please skip to my next point, if it is the latter, I do not think this is a good way to present the pH. It makes the AE ability of the 2% carbonate substrate appear artificially worse than the 1%, when it is in fact the opposite. The delta pH should represent the real AE experienced by the settlers regardless of the change in pH above the boundary layer.

We clarify in the Results that delta values are calculated with respect to the initial conditions of the water before a tile was added. (Lines 160-161)

11. -How many times did you repeat the pH measurement for the 2% carbonate tiles? To me, it has to be an error or outlier if delta pH was calculated in the first way (see above comment). I do not agree with your reasoning in the discussion for why it increases the pH less than the 1% tiles (but I am confused by what you say: see my point above). In a surface area-limited situation, it is possible for the 1% and 2% treatments to have the same leaching rate into the water column, but it is not possible for the 2% to leach less (unless the 2% composition results in a much different porosity/surface structure of the cement, which is unlikely). Then assuming equal or greater leaching, the added advection would not affect the 2% tiles any differently than the 1%. It is also troubling as your survival results show that the 2% treatment had the largest effect. This means that if your pH results are indeed accurate, the reason for the increased survivorship is not related to boundary layer pH, undercutting the main story of your paper (unless you calculate delta pH in the second way).

We agree that the results of 2% carbonate are puzzling to explain. While we only present one time series for each chemistry in **Figure 2b**, this result of 2% carbonate not increasing pH within the boundary layer was consistently observed throughout the aging experiment, indicating that from a measurement standpoint, it likely was not an outlier.

It is likely that there are additional chemical reactions going on with the cement tiles that may be altering the pH measurements. While we attribute any changes in chemistry to our (bi)carbonate modifications, several other compounds in cement likely affect local alkalinity and the chemistry about the tiles (i.e., CaO, SiO_2, Al_2O_3, and Fe_2O_3) which may affect how the higher concentration of carbonate altered the local water chemistry.

It is interesting to note that on all tiles, a visible precipitate – likely calcium carbonate – appeared on the surface of the tiles in stagnant conditions, which may have affected the pH. Although 2% carbonate did not have a large net change in pH as anticipated, the tiles did demonstrate the greatest change in Ca, DIC, and TA compared to the other tile chemistries (Lines 397-405, Supplementary Figure S4, Supplementary Table S6).

Future experiments would benefit from a greater knowledge of the ion concentrations in the tile that contribute to both alkalinity and pH to better disentangle the chemistry within the local environment, although such a complicated model is beyond the current scope of the paper.

12. -In the calculation of the Pe number, I think the height of the flume is more relevant than its length in your case, because you are interested in the transport of ions perpendicular to the substrate. This is different than the classical conception of the Pe number for species diffusing and reacting along a pipe flow.

We have recalculated Pe with the suggested length scale. (Lines 312-313)

13. -When you say the “initial settlement of O. faveolata was not strongly affected by chemistry,” I think you should make it clear that it is in regard to the two chemicals investigated here, and not a general statement. Yus et al. (2024) showed that O. faveolata larvae settlement preference was affected by alkaline earth ions leached from substrates.

Yes, we make this clarification where initially stated (Lines 342-343). Additionally, we add a section where we acknowledge that the local chemistry may be affected by the microbiome, which was untested in the current experiment. (Lines 409-412)

14. -You seem to imply that the larvae preferred your divots due to attachment point theory (APT), but 3 mm is much larger than optimal topographical features predicted by ATP, which is focused on almost perfect matching between larval diameter and the feature width. When the larvae are not able to touch the divot on all sides, the contact area is lower than APT. Your result is in line with what is reported in ref. 17.

The agree with the reviewer and have now modified our sentences appropriately (Lines 347-352).

15. -Similar to a point above, while there is little flow in/out of the divots, there is likely still recirculatory flow within the divot as long as the settlers are not occupying too much of the internal volume. But you are right that this decreased mass transport in/out of the divot could have a negative effect on larvae (e.g., lack of nutrients and waste removal).

We acknowledge here that skimming flow may result in minimal recirculating flow within the divots. (Lines 356-365).

16. Materials and Methods

-Please provide the supplier of the Portland limestone cement and the chemical additives.

We have added the requested specs to the Materials and Methods section. (Lines 442-446)

17. -For the pH measurements, can you give some information on the sensitivity and uncertainty you expect? A result of around 0.1 pH seems quite precise to expect from an electrochemical probe under flowing conditions. Normally, these types of probes have some equilibration time (for stabilizing the ion flow through the membrane or capillary), so in a dynamic system within a thin boundary layer (as opposed to a well-mixed beaker), do you think the measurement is reliable enough to be significant at 0.1 units?

Before any experimentation occurred, we validated our sensors against standard methods of measuring pH by comparing sensor readings to spectrophotometry measurements of pH. Accounting for noise in the sensors, the spectrophotometry measurements of pH fall within one standard deviation of the sensors, indicating high reliability. We have clarified this analysis in Supplementary Figure S5 and have added a new Supplementary Table S9 that better reports the results from this verification analysis.

During our experiments with tiles, we took precautions to ensure our sensors were calibrated to the dynamic solution – primarily including a calibration period of up to 30 min to ensure the sensors were producing a steady and reliable reading (Lines 594-596).

18. -Your Reynolds number in the recirculating configuration is quite high, near the critical Re. So you may be in more of a transitional flow regime than a purely laminar flow.

We regret that the Reynolds number mentioned previously was a mistake. We clarify that our Reynolds number in the recirculating configuration is less than 1,000 (Line 547) and is well within the laminar regime, as described in greater detail in our FlumeX manuscript (Ruszczyk et al., HardwareX, 2025).

19. -Why did you sometimes place the pH probe 3.26 mm above the surface and other times 1 cm? This was not clear in the Results, only the 3.26 mm distance was mentioned there.

The sensor positioning of 1 cm above the surface of the tiles was used in the initial advection quantification of the experiment to collect data at multiple heights. This secondary height is not presented in the manuscript, and we have removed this sentence from the methods.

We initially wanted to compare the change in pH within the laminar boundary layer at different heights to estimate a diffusion gradient of the change in pH, hence the various sensor heights for “above”. This analysis ended up being beyond the scope of the paper as we incorporated biological data into the manuscript.

3.26 $\pm$ 0.09 mm was the average height for the “above” sensor throughout the aging experiment and best represents the sensor height. We have edited and clarified any mention of the “above” sensor height to be within this range.

20. -I think the Methods could be organized a bit better, for instance, there are several places that mention different aspects of the pH microelectrodes, which could be grouped together. More sub-headings would also help.

Following the reviewer's suggestion, we have now divided the Methods section into the different experiments by adding/modifying the following subheadings:

- Total Alkalinity (TA), Dissolved Inorganic Carbon (DIC) and Calcium (Ca) Measurements (Line 469)
- pH Measurements in Stagnant Conditions: Diffusive Quantification (Line 494)
- Simulating the Hydrodynamic Landscape (Line 504)
- Particle Image Velocimetry of Tiles (Line 550)
- pH Measurements in Laminar Conditions: Advective Quantification (Line 564)
- pH Measurements in Laminar Conditions: Tile Longevity (Line 573)
- pH Measurements in Laminar Conditions: Topography Quantification (Line 603)
- *Orbicella faveolata* Settlement on Tiles (Line 613)
- *Orbicella faveolata* Grow out in Flumes (Line 657)

Subdividing the Methods further clarifies some of the sensor settings that were changed between experiments, rather than aggregating all the sensor data into one section. We believe this strengthens the manuscript and clarifies the different experiments performed as there were many!

21. -Please provide a brief description of or reference for your gamete collection and fertilization protocol.

We have provided more detail into the collection and fertilization processes. (Lines 624-633)

22. -Please mention in the Results section that a CCA cue was added and that settlement preferences were not in response to AE chemistry-based cues alone.

We clarify in the Results section that CCA was also used as a cue in the settlement experiments. (Line 247)

23. -How were outliers identified in the PIV datasets?

Outliers were identified by removing velocity vectors greater than or less than 4 standard deviations from the average velocity or vorticity. (Lines 755-757)

24. References
-There is a typo in ref. 17: "larvael"
This typo has been corrected. L861 (Levenstein et al. 2022 reference)

In summary, we consider this manuscript a foundational proof-of-concept study establishing AE tile–based local pH modulation as a promising approach for coral restoration. We thank the reviewer for their extremely detailed comments on hydrodynamics and flow characterization. In ongoing and future work, we will examine the effects of oscillatory flow regimes, refine boundary-layer modeling, and further explore the mechanistic links between substrate chemistry and coral skeletal growth. We believe that the substantial edits, new data, and clarifications introduced here directly address the reviewer's concerns and have strengthened the overall manuscript.

Response to Reviewers

We thank the editors and reviewers who have re-evaluated our manuscript. We are pleased that the manuscript has been accepted in principle for publication pending minor revisions. We have addressed the editors' and reviewers' comments and have documented the changes below.

The Reviewers' comments are repeated here below, followed by the authors' response in blue color. Any line references made in the response refer to the new, continuous line numbering in the revised manuscript with edits marked.

Editor:
We have noticed that you have added two new additional authors Veronica Paul and Peter K. Swart. Please write the justification for these two additions at the top of the rebuttal letter (response to the reviewers' comments-document)

To best address some of the feedback from reviewers after our first submission pertaining to the chemistry of the tiles, we performed additional experiments to demonstrate that the cement tiles could indeed increase seawater alkalinity. We also performed a more detailed water-chemistry analysis of this process. These additional analyses, which strengthened the manuscript, were performed by the added authors, and their inclusion reflects substantial contributions to the new experimental data and interpretation.

Reviewer 1:
The authors have been very thorough in responded to my concerns and believe their revised manuscript should be accepted.

We would like to thank Reviewer 1 for their positive assessment.

Reviewer 2:
The revised manuscript adequately addresses all previous comments and the manuscript appears now ready for publication.
-Daniel Wangpraseurt

We would like to thank Reviewer 2 for their positive assessment.

Reviewer 3:
I am generally satisfied by the author's responses to my questions and their extensive changes to the manuscript and can recommend publication. Although, I still find the 2% carbonate result difficult to interpret and the current explanation provided in the manuscript insufficient. I would prefer that a version of the answer given in the author response, regarding possible unknown chemistry/mineralization effects at the substrate surface, be used in the text instead of the current "advection hypothesis". This is presented as self-evident on line 325, but in my opinion, there is no clear physical reason that advection would affect the 1% and 2% tiles differently (even considering the referenced aging experiment data), unless the increased mixing induces chemical/mineralogical changes that then have a cascading effect on the pH modification or even the pH measurement itself. The Ca, DIC, and TA data would also be interesting to note here. Despite this one lingering issue, I am overall impressed by the paper and the significant work on the manuscript revision.

We appreciate the reviewer's feedback and thank them for their support. As such, we have removed our advection hypothesis to explain the 2% carbonate tiles' pH data in favor of a brief

discussion on potential chemical and mineralogical processes beyond the scope of the present study that may be contributing to the distinct response of the 2% carbonate tiles under flow conditions. Although the 2% carbonate tiles did not exhibit the expected pH modification under flow, Supplementary Figure S4 and Supplementary Table S6 demonstrate measurable increases in Ca, DIC, and TA near the tile surface. Further work will be required to quantify these potential chemical and mineralogical processes more rigorously. (Lines 340-351).

One other small thing I noted on line 313 is that I believe the water depth should be 0.1 m and not 0.01 m.
We have corrected this typo. (Line 328)