Response to reviewers #1 comments on ms no: egusphere-2024-3273 "Marine carbon dynamics in a coral reef ecosystem of Southern Taiwan" (Meng, Chang, Chou, Fan, Hsieh, Mayfield, and Chen)

Anonymous Referee #1

Overall, this manuscript characterizes the seawater carbonate chemistry variability, CO2 flux dynamics, and exposure of a nearshore marine ecosystem in the southern tip of Taiwan. These observations provide a short and sweet narrative of the dynamics of the system. In general, the nearshore is not often adequately characterized in terms of seasonal ocean acidification, hypoxia, and climate change dynamics, so this article would be a contribution to the literature and the study would provide a useful dataset for validating numerical model estimates of ocean conditions in the geographic region. Ultimately, however, I was left underwhelmed by this paper and felt that claims were presented without any relevant data to support it.

Thank you for your detailed feedback and for highlighting the importance of our study in contributing to the understanding of nearshore carbonate chemistry dynamics, CO_2 fluxes, and ecosystem exposure in the southern tip of Taiwan. We appreciate your recognition of the value of our dataset for validating numerical models and addressing gaps in nearshore characterization related to seasonal ocean acidification, hypoxia, and climate change dynamics.

We understand your concern regarding the claims in the manuscript and the need for stronger support through relevant data. To address this, we have taken the following steps in the revised manuscript:

- **Clarification of Claims: We have reviewed the claims made in the paper and ensured that all statements are directly supported by data presented in the results or appropriately referenced in the literature. Ambiguous or unsupported claims have been revised or removed.**
- **Enhanced Data Presentation: Additional figures and tables have been included to better illustrate key observations and trends in the dataset, particularly those related to carbonate chemistry variability and CO_2 flux dynamics.**
- **Addressing Gaps: Where possible, we have included supplementary analyses or references to validate our findings and provide more comprehensive support for the claims made.**

We hope that these revisions will address your concerns and enhance the overall impact and clarity of the manuscript. Your constructive feedback has been invaluable in improving the quality of this work, and we are grateful for

the opportunity to refine our study further.

Main Comments:

The abstract and introduction allude to the discussion of the influence of vertical mixing, intermittent upwelling, and biological effects on carbonate chemistry parameters, however, the results do not show any mechanisms/observations for how these drivers. For example, Chen et al., 2005 demonstrated an enhanced eddy induced upwelling signal during a spring, flood tide in late-February. However, data presented in this study was not displayed in such a way to convince me that any of the observed variability was due to upwelling.

Thank you for your valuable feedback. We appreciate your comments and acknowledge the need to more clearly demonstrate the mechanisms driving the observed variability in carbonate chemistry parameters. Below, we address your concerns:

Influence of Upwelling

We acknowledge that the data presented in the original manuscript did not explicitly highlight evidence of upwelling events. To address this, we have reanalyzed our dataset and identified specific periods characterized by physical indicators, such as surface water temperature anomalies, coupled with increased nutrient concentrations, which suggest potential upwelling events. These periods are now clearly indicated in the revised results section (lines 335–346). For additional details, please refer to our response to your subsequent comment.

Vertical Mixing

In this revision, we have included the mixed layer depth to highlight the stronger vertical mixing observed in spring and winter. Additionally, we analyzed and demonstrated the impact of mixing on carbonate chemistry. We hope this provides evidence to support our argument regarding the effect of vertical mixing.

Biological Effects

To address your concern, we conducted additional analyses examining the interplay among Chl *a*, pCO_2, temperature, and nutrient dynamics. Our findings indicate a significant correlation between Chl *a* concentration and surface nitrate levels, suggesting a nutrient-driven biological response. Furthermore, we have recalculated and incorporated the normalized total alkalinity (nTA) and normalized dissolved inorganic carbon (nDIC) values in the revised manuscript. By normalizing these parameters to a consistent salinity (35.6), we aim to minimize the effects of evaporation and precipitation,

enabling a more precise investigation of mixing and biological processes. Notably, nDIC exhibited a significant negative correlation with Chl *a* concentration during summer when using pooled data (r^2 = 0.01; p < 0.01). For further details, please refer to our response to Reviewer #2 regarding "Insufficient quantitative analysis" and "The relationship between Chl *a* and pCO_2 is misleading. Biological Effects."

We hope these additions adequately address your concerns and provide a more compelling linkage between the observed variability and the proposed mechanisms. We are grateful for the opportunity to refine the manuscript and welcome any further suggestions you may have.

Additionally, I am doubtful that the upwelling (especially eddy induced upwelling) drives vertical mixing of the entire water column. The same study (Chen et al., 2005) showed shoaling of lower temperature, higher nitrate and Chl a seawater to ~30 m depth in the same region. I would expect that a clear upwelling signal would be represented by enhanced water column stratification – with ocean warming at the surface and upwelling at depth.

Thank you for your insightful observation regarding the mechanism of upwelling and its potential impact on vertical mixing. In our previous study (Chen et al., 2005), we specifically designed high spatial and temporal resolution experiments to comprehensively explore the upwelling phenomenon. However, such a detailed experimental setup for studying upwelling was not implemented in this study.

We understand your concern about whether upwelling, particularly eddy-induced upwelling, can drive vertical mixing of the entire water column as opposed to causing stratification. While the upwelling process described by Chen et al. (2005) resulted in the shoaling of colder, nutrient-rich waters to approximately 30 m, cold surface water temperatures were also observed during upwelling events. The conditions in this study appear to be influenced by sustained or periodic upwelling events during suitable surface water temperature combined with external forces, such as wind-driven mixing or tidal forcing. These additional factors likely contributed to the homogenization of the water column, as reflected in the temperature, salinity, and pH profiles, which exhibit well-mixed conditions rather than stratification.

The occurrence of well-mixed conditions caused by upwelling has also been observed in our previous study in the East China Sea (Chen et al., 2022) and in this study region (Chen's unpublished data). To further support this

interpretation, we have included additional evidence in the revised manuscript. For example, the significant surface water temperature drop in Nanwan during the sampling period compared to earlier conditions—decreasing from 27.1°C to 21.7°C—suggests sustained periodic upwelling events (refer to Fig. S1b in the revision). Furthermore, we have incorporated a detailed discussion of the vertical profiles of nutrients and Chl *a*, which provide valuable insights into the interplay between upwelling and water column mixing (see Fig. X below for reference). Please also refer to lines 335–346 in the revised manuscript for additional details.

We hope this explanation, along with the additional analyses and evidence included in the revised manuscript, addresses your concern. We welcome further feedback to improve the clarity and rigor of our work. Additionally, we are willing to de-emphasize the impact of upwelling if this remains a concern for you.

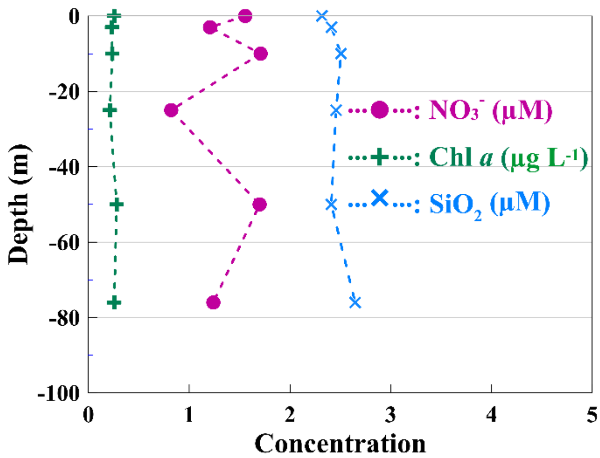

Fig. X The well-mixed vertical profiles of Chl *a*, nitrate, silicate concentrations at S10 during spring period.

Chen, C.-C., D. S. Ko, G.-C. Gong, C.-C. Lien, W.-C. Chou, H.-J. Lee, F.-K. Shiah, and Y.-S. W. Huang. 2022. Reoxygenation of the hypoxia in the East China Sea: A ventilation opening for marine life. Front. Mar. Sci. 8: 787808. doi:10.3389/fmars.2021.787808.

In general, the color scheme of the figures is not easy to see, I would recommend the authors to use a different color scheme for the figures or at the very least change the blue outline to black.

Thank you for your valuable feedback regarding the color scheme of the figures. Based on your suggestion, we have replaced the blue outline with black to create a more distinct and easily recognizable boundary. However,

we have retained the existing color scheme as it was specifically designed to be colorblind-friendly, ensuring better clarity for a diverse readership.
The revised figures are included in the updated manuscript. We believe these changes enhance the readability and accessibility of the figures. We greatly appreciate your insightful comments and the opportunity to improve our work.

The authors do a good job discussing the effect of wind speed on their calculations, but I do not believe that it is appropriate to use an average monthly wind speed for a single day of pCO_2 sampling. You can only say for a specific date and time that this was the pCO_2 and air sea flux.

Thank you for your insightful comment regarding the use of average monthly wind speed in our calculations and the interpretation of *p*CO_2 and air-sea CO_2 fluxes. We appreciate your concern and acknowledge the limitations of applying a monthly wind speed average to data collected on a single day.
To address this, we have taken the following steps in the revised manuscript:

- **Clarification of Methodology: We have revised the methods section to explicitly discuss the rationale behind using monthly wind speed averages and their associated limitations. We acknowledge that this approach provides a broader context for our estimates but may not accurately reflect short-term variability.**
- **Revised Interpretation: We have adjusted the discussion to emphasize that the reported *p*CO_2 and air-sea CO_2 fluxes represent conditions specific to the sampling date and time, rather than generalized monthly conditions.**
- **Sensitivity Analysis: Where feasible, we have conducted a sensitivity analysis to estimate how the variability in wind speed on shorter timescales (e.g., daily or monthly) might affect the air-sea flux calculations. These results have been incorporated into the discussion to provide a more nuanced interpretation.**
- **Recommendations for Future Work: We have included a note in the discussion about the importance of concurrent high-frequency wind speed and *p*CO_2 measurements for improving the accuracy of air-sea flux estimates in future studies.**

We believe these revisions provide greater transparency and address your concerns about the methodology and interpretation of our findings. We are grateful for your constructive feedback, which has helped us improve the rigor and clarity of our work.

Lastly, I am not convinced that the temporal sampling resolution (March, 2011; July, 2011; October, 2011; January 2013) is a good enough representation of the seasonal variability in such a dynamic environment. To improve the quality of the manuscript, I would recommend the authors to utilize any moorings, hindcast models, satellite products, or additional time series from the region to complement the dataset and provide a more robust correlation to the various mechanisms.

We appreciate your thoughtful feedback regarding the temporal sampling resolution and the suggestion to explore complementary datasets. As noted in our response to your previous comment, we acknowledge the limitations of our dataset. To address this, we have emphasized that the reported *p*CO_2 and air-sea CO_2 fluxes represent conditions specific to the sampling date and time, rather than generalized monthly conditions.

Additionally, where feasible, we have conducted a sensitivity analysis to estimate how short-term variability in wind speed (e.g., daily or monthly fluctuations) might influence air-sea flux calculations. While we recognize the constraints of using discrete sampling points (March 2011, July 2011, October 2011, January 2013) to fully capture seasonal variability in such a dynamic environment, these data represent the best temporal coverage available from our field campaigns.

To further address this limitation and enhance the robustness of our findings, we have incorporated additional data sources into our analysis. Specifically, we have integrated daily sea surface temperature data from nearby moorings over the study year, where available, to provide additional context and strengthen our interpretations.

These supplementary datasets and analyses have been incorporated into the revised manuscript, with corresponding details added to the methods and results sections. We believe this comprehensive approach effectively addresses your concerns and significantly improves the quality and robustness of the manuscript.

We sincerely thank your valuable suggestion again, which has allowed us to refine our work further.

Minor Comments:

Why did the authors decide to use Wanninkhof (1992) for wind speed when Ho et al., (2006) is more appropriate for the region?

We chose to use Wanninkhof (1992) in our CO_2 air-sea flux calculations to maintain consistency and comparability with prior studies, many of which rely on this widely established parameterization. This decision ensures that our

results can be contextualized within the broader body of literature. However, we acknowledge that Ho et al. (2006) may be more regionally appropriate due to its formulation, which is specifically tailored to certain conditions. To address this, we incorporated Ho et al. (2006) as a complementary approach, enabling us to evaluate potential uncertainties arising from different wind-speed parameterizations.

Our analysis revealed that flux values calculated using Wanninkhof (1992) were approximately 17% higher than those derived from Ho et al. (2006). This comparison highlights the sensitivity of flux estimates to the choice of parameterization and provides valuable insights into the implications of using regionally nuanced versus widely generalized models. By employing this dual approach, we aim to balance the need for methodological consistency with the incorporation of regionally relevant dynamics. (lines 484–492)

Table 1: Is all this information useful for the study or is there a better way to show this?

Thank you for your valuable suggestion. In this table, we intended to present all the analyses systematically, allowing readers to refer to it for their own purposes. However, as you pointed out, some of the information presented may be limited in relevance to the main manuscript. As a result, we have moved this table to the supporting information section. We hope this adjustment addresses your concerns and improves the clarity of the manuscript.

Figure 4: Where are the sites located and is it appropriate to interpolate across these sites given the course spatial resolution?

Thank you for your comment. The sampling site locations are shown in this figure and Figure 1 of the manuscript. These sites were carefully chosen to capture the spatial variability within the study area. Given the relatively fine spatial resolution, we consider interpolation across the sites to be appropriate, as the measured parameters exhibit consistent trends and the study region demonstrates spatial homogeneity.

Figure 5: Was only referenced once; is it appropriately discussed within the manuscript or does the figure have little value.

We appreciate the reviewer's observation regarding Figure 5 and its reference within the manuscript. Upon review, we acknowledge that the figure was referenced only once, which might give the impression that it is not fully

integrated into the discussion.

In this manuscript, our primary objective is to understand surface water pCO_2 dynamics in the coral reef ecosystem. Figure 5 presents the vertical profiles of pCO_2 and associated variables, offering readers a more comprehensive understanding of their distribution within this system. Although the figure is referenced only once, we believe it contributes valuable context, allowing readers to construct a more complete picture of pCO_2 variability in this ecosystem.

Therefore, we propose to retain Figure 5 in the manuscript, as we believe it enhances the overall narrative. However, if the reviewer feels it would be more appropriate, we are happy to move the figure to the appendix to ensure the main text remains concise.

We thank the reviewer for bringing this to our attention and for helping us ensure that all figures included in the manuscript are meaningful and well-integrated into the discussion

We thank the reviewer for bringing this to our attention and for helping us ensure that all figures in the manuscript are meaningful and appropriately discussed.

Figure 6: Where did the data from Tew et al., 2014 come from?

Thank you for your comment. We apologize for not providing sufficient details regarding the data source. The data presented in Figure 6 is redrawn from our previously published paper, and we have now clearly indicated the source in this revision: (redrawn based on the data presented in Fig. 8 of Tew et al., 2014).

Figure 7: A bit confusing, does this show that the nT effects increase pCO2 while the T effects decrease pCO2?

Thank you for your comment. This figure illustrates the mean values and the effects of 'T' (temperature) and 'nT' (non-temperature) on surface pCO_2 at each station across all sampling periods. In general, the results indicate that nT effects tend to increase pCO_2, while T effects generally decrease pCO_2 at most stations. However, a pronounced increase in pCO_2 due to T effects is observed at stations S31 and S33, located near the Nuclear Power Plant outlet, where water temperatures were consistently higher compared to other stations.

Lines 111-115: The two sentences in a row are redundant.

Thank you for your valuable suggestions. The two sentences have now been combined into the following revised statement: "*Primary productivity in marine ecosystems plays a crucial role in carbon cycling, as the fixation of CO_2 during periods of increased productivity enhances carbon uptake from seawater, potentially lowering its concentration.*"

Line 175: Was pH converted to T scale or kept in NBS?

Thank you for your inquiry. The measured pH values were kept on the NBS scale.

Line 243: "spring and winter [water masses] are intermediate between the two." I do not see this.

Thank you for highlighting the ambiguity in our statement. Based on the T-S diagram, the water in Nanwan Bay is a mixture of South China Sea (SCS) water and Kuroshio water, with a higher proportion of SCS water during spring and winter. We have revised the description as follows: "*An analysis of temperature and salinity data from Nanwan Bay, the SCS, and the Kuroshio Current indicates that Nanwan Bay predominantly consists of the SCS water mass during summer and autumn, while during spring and winter, the water masses are mixed between the two (Fig. 2).*" We hope this revision addresses your concern and provides better clarity.

Lines 275-277: I do not see the evidence.

Thank you for your comment. We apologize for the previously ambiguous statement. In this revision, the statement has been slightly modified to include more specific evidence, as follows: "*Additionally, seawater at station S10 during spring, characterized by relatively low temperature (23.3±0.2 ℃), low pH (8.16±0.01), and high salinity (34.32±0.03), suggests that this well-mixed pattern observed throughout the water column is likely with upwelling during this period (Figs. 3a, b and S2b; further details can be found in the next section).*" We hope this revision addresses your concern and provides greater clarity.

Line 320: Use of Takahashi et al. 2002 should be discussed earlier during the methods

Thank you for your valuable suggestion. We agree that discussing the use of Takahashi et al. (2002) earlier in the Methods section provides better context for its application. Accordingly, we have moved and expanded the discussion

within the Methods section as suggested. We appreciate your feedback, which has helped enhance the organization and clarity of our manuscript.

Line 365: Chl a does not drive photosynthesis; Chl a is a proxy for phytoplankton biomass.

We apologize for the confusion, and you are correct. The sentence has been slightly modified in the revision to: "*In general, Chl a serves as a proxy for phytoplankton biomass, which influences pCO_2 through processes such as photosynthesis, removing CO_2 from seawater (Chen et al., 2019).*"

Line 408: Is this greater than the error that was introduced by the calculation?

Thank you for your inquiry. The uncertainty in △*p*CO_2 is equivalent to the uncertainty in the calculated *p*CO_2. To ensure clarity for the reader, we have explicitly added this explanation to the revised text. We hope this addresses your concerns.

Lines 410-415 and 429-433: Text seems too much like a list. Can be presented in a better format.

Thank you for your insightful feedback regarding the text in lines 410–415 and 429–433. We have revised these sections to improve the narrative flow and reduce the "list-like" presentation. The updated text weaves the data into cohesive paragraphs, ensuring a smoother and more engaging format while maintaining the scientific clarity. (lines 452–458 and 471–478)

We appreciate your suggestion, as it has helped enhance the readability of the manuscript.