#	Comment	Response
1.1	This manuscript examines coral reef production rates (Net Ecosystem Production) in Hong Kong across a gradient of coral cover under described "marginal conditions". The methods are sound and the introduction illustrates the need for these types of studies.	We thank the reviewer for this positive assessment of the study's methodological approach and broader motivation. We are pleased that the reviewer found the methods sound and recognised the need for high-resolution, in-situ assessments of coral community production in environments such as Hong Kong. In revising the manuscript, we have retained this core focus while also addressing the reviewer's constructive comments, which have helped us to sharpen the framing, define "marginal conditions" more mechanistically, and clarify the scope of inference from our measurements. We have revised the manuscript to make clear that our measurements specifically quantify net ecosystem production and organic carbon cycling, rather than the full suite of processes contributing to reef metabolism, calcification, accretion, or overall reef health.
1.2	However, the conclusions are not supported by the results. Firstly, the description of marginal conditions is too broad. Of the proposed factors that contribute to marginality, such as nutrients or rainfall, many were either not measured or did not significantly differ between sites or previous studies. The paper needs to be reframed as a case study on reef NEP specifically in low light conditions (due to turbidity and latitude).	We thank the reviewer for highlighting the need for a clearer and more mechanistic treatment of "marginal conditions". We agree that the original framing was too broad in places and did not always distinguish clearly between drivers measured directly in this study, drivers characterised using regional environmental datasets, and broader stressors documented previously for Hong Kong. We have therefore revised the manuscript to define marginality mechanistically, as environmental conditions that constrain coral community performance and reef functional processes, including benthic production, calcification potential, and community development. In this framework, Hong Kong represents a marginal coral system because coral communities persist under low-light and turbid subtropical conditions, strong seasonal temperature variability, periodic freshwater influence, and documented anthropogenic pressures, all of which can constrain coral growth and community-scale metabolism. We have also revised the manuscript to make clear that our study specifically tests NEP responses within this marginal environmental context, rather than testing every potential driver of marginality directly. Accordingly, we have narrowed the conclusions to focus on low and variable NEP under these conditions, while treating nutrients, rainfall, and other regional stressors as contextual factors supported by cited datasets. Regarding reframing around light, we respectfully do not fully agree that the manuscript should be reframed solely as a low-light case study, because low light is just one of the important mechanisms of marginality defining Hong Kong's coral ecosystems. However, we agree that our conclusions should be tied to the mechanisms and variables actually addressed, and we have revised the manuscript accordingly.
1.3	Secondly, the paper is missing estimates of reef accretion or calcification which are critical to making conclusions about overall community metabolism and reef health. We simply do not know if changes in NEP have anything to do with the corals without simultaneous measurements of calcification. Coral cover of 30 – 60% is above average to high. 0% algae cover is very rare. There is an interesting story here why these reefs are so healthy (high coral cover, no algae) yet are net respiring and persist under low light conditions. This is briefly alluded to on L496 and	We thank the reviewer for this important comment. We agree that estimates of calcification, dissolution, or net ecosystem calcification would be required to draw conclusions about reef accretion, carbonate balance, or overall reef health. In an earlier version of this manuscript, we did include NEC estimates, but these were removed following concerns about the reliability of the pH sensor data used to calculate NEC. Specifically, after identifying issues with the pH measurements, we chose to remove NEC results and all NEC-related interpretations so that the revised manuscript would focus only on the NEP data that could be interpreted robustly. We have now further revised the manuscript to ensure that NEP is treated specifically as a measure of organic carbon cycling and the balance between photosynthesis and respiration, rather than as a complete measure of community metabolism, reef accretion, or reef health. We also agree with the reviewer that one of the most interesting findings is the apparent contrast between relatively high coral cover, negligible visible macroalgal cover, low-light conditions, and consistently low or negative NEP. We have therefore revised the Discussion to focus more explicitly on this pattern and to explore possible mechanisms, including light limitation, heterotrophic subsidy, respiration by non-coral benthic and microbial components, community composition, and the possibility that coral persistence in Hong Kong may be decoupled from strongly positive community-scale NEP.

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	should be the focus of the paper.	However, we respectfully note that the absence of NEC does not preclude interpretation of NEP as a community-scale measure of organic carbon cycling. Rather, it limits the scope of inference. We have therefore revised the manuscript to make this distinction explicit throughout.
1.4	I encourage the authors to re-write this manuscript and avoid broad brushing terms like “marginal conditions” and “community metabolism”. The authors measured NEP on a low light reef. There needs to be a stronger tie-in to other literature values of NEP, light, and coral cover. There are multiple studies provided below that have these values as well as insight to how reef NEP may function under bleaching, a co-occuring stressor during the study. The values of NEP are extremely low relative to other reefs; this also needs to be addressed.	We thank the reviewer for this helpful comment. We agree that the original manuscript used “marginal conditions” and “community metabolism” too broadly in places. We have therefore revised the manuscript to define marginality more mechanistically, as environmental conditions that constrain coral community performance and ecosystem processes relative to more favourable reef settings. In Hong Kong, these constraints include low light availability associated with turbidity and subtropical latitude, strong seasonal temperature variability, freshwater influence, sedimentation, and urbanisation-related pressures. However, we respectfully note that we do not view low light as the only defining feature of the system; rather, it is one important mechanism through which marginality is expressed in these coral communities. We have also revised “community metabolism” to more specifically refer to NEP and organic carbon cycling, unless broader metabolic processes are explicitly discussed as limitations. In response to the reviewer’s suggestion, we have expanded the Discussion to compare our NEP values with published values from other reef systems, including reefs experiencing bleaching or other stressors, and we now explicitly highlight that the NEP rates reported here are very low relative to many previously studied coral reef environments. This comparison strengthens the central interpretation that Hong Kong coral communities can maintain coral cover and persist under conditions where community-scale net production is low or negative.
1.5	Overall I do not support the publication of this manuscript until the authors are able to focus the conclusions specifically on reef production and low light. Statements cannot be made about reef health or function without calcification rates.	We thank the reviewer for clearly identifying the main issue that needed to be addressed. We agree that, without reliable calcification or net ecosystem calcification measurements, the manuscript should not make conclusions about reef accretion, carbonate balance, reef health, or overall ecosystem function. We have therefore revised the manuscript throughout to focus the conclusions specifically on net ecosystem production, organic carbon cycling, and the role of low light availability as a key constraint on community-scale production in Hong Kong coral communities. Statements implying broader conclusions about reef health, accretion, or total ecosystem function have been removed or qualified. We have also added explicit discussion of this limitation, clarifying that NEP provides important information on the balance between photosynthesis and respiration, but cannot by itself resolve calcification, carbonate accretion, or reef health.
1.6	L7 – Are these anthropogenic stressors or local climactic variability?	We thank the reviewer for highlighting this ambiguity. We agree that the initial wording could be confusing, as the high bioerosion and seasonality would be natural stressors, while the other stressors listed are anthropogenically driven (sedimentation, elevated nutrients). We have therefore revised the text for clarity to read “a range of local stressors” instead of “natural and anthropogenic stressors” (Line 7)
1.7	L14 – mmol missing “l”	Thank you for pointing this out. This has now been added (Line 15).
1.8	L15 – There are multiple studies that look at metabolic variability during bleaching periods, on degraded reefs, and in subtropical conditions, all arguably “marginal” conditions. You go on to cite several studies that work in these locations.	Thank you for the comment. This has been rephrased to put our study in better context with the other studies looking at metabolism in marginal conditions, while still trying to highlight that the number of these studies is small. (Line 15-16).

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1.9	L21- too many "ands" in this sentence	This sentence was misstructured. We have removed the specific stressors as these were not necessary to get the point of the sentence across.
1.10	L23 – remove changeable	This has been removed.
1.11	L39 – The linkage obstacles between benthic metabolism and overlying water chemistry referred to in these citations are due to water volume and residence time. In-situ measurements of DO or pH to calculate reef metabolism are restricted to reefs where there is a measurable change in these parameters. Your study does not address this, as it relies on chemical anomaly methods. Remove this line	We thank the reviewer for this clarification. We agree that the original sentence was not necessary for the methodological framing of this study and could distract from the specific gradient flux approach used here. We have therefore removed the sentence and revised the surrounding text to focus more directly on the use of dissolved oxygen gradients to estimate NEP.
1.12	L41- unclear. You are measuring NEP with two different approaches? Methods indicate you are using one approach, chemical changes in the overlying water column.	Thank you for pointing this out. The wording was indeed confusing and misleading. The first sentence of this paragraph has been removed and the remaining wording clarified to make it clear that we are measuring DO changes in the overlying water column (Line 57-58)
1.13	Figure 1 – Higher resolution image please	We thank the reviewer for highlighting this issue. We have revised this figure and increased the resolution for clearer readability.
1.14	L122 -125 – This belongs in the results	We agree that this sentence read somewhat like a result in the original version. We have revised the text to clarify that the statement is included as methodological justification for classifying the October 2022 deployments as late-wet-season deployments. Specifically, we now explain that although October can represent a seasonal transition period in Hong Kong, the 2022 autumn was anomalously warm, supporting our treatment of these deployments as representative of prolonged wet-season conditions. (Lines 122-130)
1.15	L189 – This does not need to be its own section. Consider adding to the statistical analyses section.	This has been moved to the end of the 2.6 data pre-treatment section following the related metabolism data treatment conditions (Line 230).
1.16	L308 – Your PAR units are missing a spatial value, usually m-2	Thank you for catching this. The correct units have been added for PAR throughout the manuscript.
1.17	L309 – The SD of these values overlap, was DO and salinity significantly different between seasons?	Thank you for pointing this out. The DO and salinity were indeed lower during the wet-season deployments than during the dry-season deployments. Using all time-point measurements, both variables differed significantly between seasons (Mann-Whitney U tests, $p < 0.001$). However, because these high-frequency measurements are temporally autocorrelated, these results are interpreted as evidence of deployment-period only differences in measured conditions rather than as full season differences. The statistical test has been added in the text here to clarify these results were statistically significant. (Line 330).

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1.18	L313 – Remain consistent with your DO units, either mg L-1 or umol kg-1. Choose one.	We have inserted consistent use of the units μmol kg^{-1} throughout the manuscript for DO values for easier comparison.
1.19	Figure 3 – Text and frame size is too small. Consider placing the wet season plot below to increase size to see trends and values	We agree with the reviewer that the text size was too small for easy readability on this figure. We have adjusted it accordingly. We however maintain that side-by-side orientation of this Figure is most appropriate for easier comparison between seasons.
1.20	L322 -327 – 36 and 65% coral cover with no algae is a very healthy reef and compares more to usual studies in optimal conditions, not marginal conditions.	We appreciate the reviewer's concern in classifying these sites as marginal, given their benthic community composition. Regarding the absence of algae: their abundance typically peak in Hong Kong in early Spring (Yeung et al., 2021, Cheung Wong et al., 2022) and is known to decline significantly by late summer and early autumn, when our surveys were conducted. Regarding the marginal classification. While coral cover % is high, the coral communities are still subject to functional and structural marginality (Schoepf et al., 2023) in that they show altered community composition and reef functioning (Duprey et al., 2016, 2020; Cybulski et al., 2020). Coral cover is just one of several metrics that may be applied to classify a community as marginal. We expand our definition of "marginal" communities in the introduction to better illustrate how each community fits the definition of marginal in different ways (whether it be structural marginality or functional marginality; Lines 52-53, Lines 422-423). Specifically, we use NEP as the metric of functional marginality quantified in this study, and coral cover as the metric for structural marginality. We have added text in the discussion to clearly state how each of the 3 sites are classified as marginal (Lines 419-423).
1.21	L329 – Unclear why the lowest coral cover makes it a "control" site. It seems instead to be your "treatment" site since you are trying to examine how marginal conditioned reefs compare to your standard reef, which, in the literature, would be closer to Tung Ping Chau at 36% coral. In any case you aren't performing experimental manipulations, so you don't have a control. You have an environmental gradient with Tung Ping Chau being most comparable to average coral reefs.	We thank the reviewer for this clarification. We agree that "control" was not appropriate terminology because this study does not involve an experimental manipulation or a true control site. The wording has been changed to highlight this site as being near the westernmost extent where corals can be found in Hong Kong and serves as a contrast to the 2 other higher coral cover sites (Lines 109-110).
1.22	L341- Here and elsewhere you need a space between mmol and O2	Thank you for spotting this spacing error. This has been corrected throughout the manuscript.
1.23	L342 -343 – These are extremely low rates of production and respiration, especially for a reef with 30	We thank the reviewer for highlighting this important point. We agree that the mean NEP rates reported here are low, or close to zero, compared with many shallow reef environments, however the variability of NEP throughout the day does show diel variation consistent with higher net production during the day

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	60% coral cover. Generally shallow benthic environments are closer to the order of 10 – 30 mmol O2 m-2 h-1. What were your changes in DO for these calculations?	and stronger net respiration at night at our higher coral cover sites. We have revised the Discussion to more clearly report our NEP rates in the context of other studies. We have provided the mean DO gradients for each deployment in response to the reviewer's question below. These gradients were generally small, consistent with the low-magnitude NEP values reported here. However, because NEP is calculated from both the DO gradient and time-varying turbulent exchange, the mean DO gradients should not be interpreted as directly equivalent to mean NEP. Mean ΔDO (umol kg-1) ± SD: Sham Wan Dry: 1.78± 1.35 TPC Dry: 1.59 ± 3.56 Sharp Dry: 3.21 ± 7.18 Sham Wan Wet: 2.92 ± 3.30 TPC Wet:1.76 ± 3.75 Sharp Wet (May): 11.13 ± 23.33 Sharp Wet (Oct):8.93 ± 30.09 The low NEP rates are consistent with the low benthic light environment measured during the deployments and, together with the absence of a saturating photosynthesis–irradiance relationship, suggests that community-scale production was strongly light-limited. We have therefore expanded the interpretation of these low rates, while also making clear that they reflect net organic carbon cycling and should not be interpreted as direct evidence of calcification, accretion, or overall reef health.
1.24	L406 – Please use PAR units here that align with your measurements of umol m-2 s-1	We thank the reviewer for highlighting this potential point of confusion. The light data reported is in units of kWh m^{-2} day^{-1} from the source material. While there is no direct conversion available between kWh m^{-2} day^{-1} and umol m^{-2} s^{-1}, we have added the approximate conversion into PAR units to this section for easier comparison (Lines 472-475).
1.25	L414 – These citations suggest that you are not the first study to look at reef production in marginal environments	We thank the reviewer for noting this potential ambiguity. We have revised the wording to make the novelty claim more precise. McIlroy et al. (2019) examined coral productivity using a controlled mesocosm / experimental approach, which provides important physiological context, but it did not measure in situ community-scale reef production under natural marginal environmental conditions. We have therefore clarified that, to our knowledge, this is the first study to quantify in situ, community-scale NEP for Hong Kong coral communities under their natural environmental conditions (Lines 479-481).
1.26	L430 – You say above that rainfall did not significantly differ between seasons but here you are saying it does?	See comment 3.32: We have added the important caveat that, while rainfall did not significantly differ between specific deployment periods in the wet and dry season, the rainfall was significantly different between the wet and dry season when averaged over the entire season. This needs to be highlighted as our deployment periods only lasted at most a few weeks, and the weather conditions observed during them may not be representative of the seasonal differences in full. (Lines 502-504). Season / Period / n / Mean ± SD rainfall Dry season / Nov 2021–Mar 2022 / 10,797 / 0.068 ± 0.482 mm h^{-1} Wet season / Apr–Oct 2022 / 15,304 / 0.329 ± 1.999 mm h^{-1} • U = 79,225,527.5, p = 2.39 × 10^{-37}
1.27	L437-L454 – Need a paragraph break in here.	A break has been added to this section to separate the two ideas discussed. (Lines 517-518).
1.28	L471 – Bleaching events have been shown to have no effect on NEP. Please review the following papers and	See comment 3.42: We agree with the reviewer that our initial assumption of bleaching's impact on NEP was overly simplistic. We have added to this discussion section to better illustrate that bleaching effects on NEP are variable and context dependent. Furthermore, we have added text to make clear that

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	include with your discussion of how anthropogenic stressors have been measured to affect reef metabolism:	in our study, bleaching impacts on NEP are likely confounded by several factors, including low-light or external nutrient input, and increased respiration demand. (Lines 543-551).
1.29	L530 – You did not measure calcification, remove.	We thank the reviewer for noting this. We agree that this statement was not supported because reliable calcification / NEC measurements are not included in the revised manuscript. The statement has therefore been removed.
1.30	L536 – We don't know this because you did not measure calcification or net accretion. As you state in L496, its possible that coral accretion remained unaffected due to enhanced heterotrophy. It is very difficult to make assertions about reef health, function, or "community metabolism" with only estimates of NEP.	We thank the reviewer for this important clarification. We agree that, without reliable calcification or net accretion measurements, we cannot determine whether carbonate accretion was reduced, unaffected, or maintained through enhanced heterotrophy or other mechanisms. We have therefore removed or qualified statements that inferred reef health, calcification, accretion, or overall ecosystem function from NEP alone. The revised text now interprets our results specifically in terms of NEP, organic carbon cycling, and the balance between photosynthesis and respiration. We also explicitly acknowledge that NEP alone cannot resolve carbonate balance or overall reef functional status, and that future paired measurements of NEP and NEC will be needed to determine how organic carbon metabolism and calcification are coupled in these communities (Lines 579-581).
1.31	L543 – You cannot make assertions about the opportunity for coral growth because you did not measure reef accretion rates. With coral coverage in 30 – 60% range, these are healthy reefs obviously capable of growing. There is a missing piece here why a reef would have such high coral cover yet be net heterotrophic.	We thank the reviewer for this important comment. We agree that, without calcification or net accretion measurements, we cannot make direct assertions about reef accretion, carbonate balance, or the opportunity for coral growth. We have therefore removed or qualified statements that extended beyond NEP and organic carbon cycling. However, we respectfully disagree that moderate to high coral cover alone demonstrates that these are "healthy reefs" in the sense of actively accreting reef systems or optimal ecosystem function. The sites studied here are more accurately described as coral communities rather than accreting coral reefs, because corals in Hong Kong often develop as patchy communities on mixed rock, rubble, and sandy substrates rather than forming substantial reef framework. We have revised the manuscript to make this terminology more consistent. We also now clarify that coral cover reflects cumulative persistence and colony survival, whereas NEP reflects short-term community-scale organic carbon balance during the deployment periods. The contrast between moderate to high coral cover and low or negative NEP is therefore not necessarily contradictory, but it does highlight an important feature of these communities: coral cover can persist under conditions where contemporary net organic carbon production is low. We have expanded the Discussion to address this apparent decoupling and possible mechanisms, including chronic light limitation, heterotrophic subsidy, increased respiration demand, and seasonality in production.

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2.1	This work presents in situ measurements of NEP across a gradient of environmental conditions in coral reefs of Hong Kong. The study is valuable in that it captures high-resolution, community-scale metabolism in a system characterized by strong anthropogenic stressors and contributes to a relatively limited body of work examining metabolic variability in marginal coral reef environments. The manuscript is generally well written, but there are several areas where the interpretation and framing needs to be strengthened.	We thank the reviewer for their encouraging remarks about the goal of this study. We welcome their suggestions and comments on how to further strengthen the manuscript for publication.
2.2	The manuscript consistently reports negative NEP across sites and seasons, but the ecological implications of persistent net heterotrophy are not fully developed. The relationship between NEP and NEC is not discussed and should be introduced earlier, then revisited in the discussion. Also, please contextualize these results against studies outside of Hong Kong.	Because NEC was not measured in this study, we have revised the manuscript to avoid any implication that NEP can be used to infer calcification, coral growth, reef accretion, carbonate balance, or other aspects of inorganic carbon cycling. We have therefore removed detailed discussion of NEC from the main interpretation and now mention it only briefly in the Future Directions section, where we identify paired NEP–NEC measurements as an important priority for future studies. We have added further discussion comparing our observed low NEP against other studies that have measured community-scale NEP as well for contextualization and added mention of the implications of persistent net heterotrophy of coral communities.
2.3	The concepts of "marginal" vs "extreme" environments are introduced but not consistently applied. Clarification of how each site fits within this framework would strengthen the manuscript.	We thank the reviewer for this constructive feedback. We have revised the section of the Introduction to remove use of the "extreme" definition of Hong Kong and instead describe Hong Kong as an environment with marginal environmental conditions in a regional scale, driven through seasonality and anthropogenic stressors, to better fit in with the literature of marginal reefs. We clarify that these larger scale marginal environmental conditions are distinct from the 'community-scale' marginality that we use to define each of our specific study sites. Furthermore, we have given further details on how each site may be classified as marginal specifically, in terms of either structural or functional marginality. Specifically, we distinguish Sham Wan as structurally marginal due to reduced coral cover and diversity, and TPC and Sharp as functionally marginal due to low production. This was an important distinction to add to the manuscript so the reader may not be confused by different sites with vastly different coral cover being described as "marginal" arbitrarily.
2.4	A substantial proportion of the dataset was removed during preprocessing. Can you provide additional justification and discussion of how this may affect results?	We have added additional justification and made clearer the necessity of the data filtering steps for NEP measurements. We importantly note that these data filtering steps, while necessary for accurate NEP quantification, may bias estimates towards conditions with stable boundary-layer structure and adequate flow, and are not necessarily representative of the whole suite of hydrodynamic conditions

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		experienced by the community over the deployment times (Lines 209-223). We also note that a previous study (Platz et al., 2022) also removed comparable amount from their dataset when using the gradient flux method (65% data removed, August 2018 dataset) in order to contextualize our filtering steps with other studies.
2.5	The designation of Sham Wan as a control site is not fully convincing given its low coral cover and different benthic composition from other sites.	See comment 1.21. We agree with this conclusion that Sham Wan should not be considered “control” site. This description has been adjusted.
2.6	The explanation for increased variability in the wet season is vague and should be expanded with clearer hypotheses and looped into the abstract. For example, lines 432-433 call out some potential conditions that would drive increased variability in NEP in the winter, but it seems you could access the data (or already have it) to test this hypothesis.	We thank the reviewer for this helpful suggestion. We agree that the explanation for greater wet-season NEP variability required clearer development. We have revised the Discussion to present more explicit hypotheses for this variability, including stronger fluctuations in benthic light availability, increased rainfall, higher temperatures, greater stratification, and possible increases in organic matter supply and respiration demand during the wet season. Where these variables were available from our in situ sensors, CTD casts, HKO weather data, or EPD monitoring data, we have distinguished between measured patterns from proposed mechanisms. We have also revised the Abstract to note that wet-season NEP was not only lower / more negative, but also substantially more variable (Lines 14-15), likely reflecting stronger short-term environmental variability under wet-season conditions.
2.7	Abstract: consider including potential explanations for increased NEP variability in the wet season.	A brief mention of the mechanisms behind the increased wet season NEP variability has been included in the abstract while still maintain the word limit. Mention of the variable light and hydrographic conditions in the wet season has been added to provide the reader information about the drivers of the increased variability (Lines 14-15).
2.8	Figure 1: figure is blurry	See comment 1.13: We thank the reviewer for pointing out the inadequate resolution of our current figure. This figure has been revised to make the resolution sharper and clearly visible for the reader.
2.9	Lines 100-101: But in the intro you say that Pearl River discharge reduces water clarity and thus corals don‘t live near the estuary.	These lines refer to corals in the eastern waters of Hong Kong, farthest away from the Pearl River discharge. We have rephrased the wording for clarity and to reinforce the environmental gradient, changing “unaffected” to “less affected” (Line 99).
2.10	Lines 134-135: Begging for a reference	Thank you for pointing out this gap in citation. Several references have been added here relating to the specific variables mentioned in the text and their relation to coral metabolic processes (Lines 146-147)
2.11	Lines 284-291: I may be misinterpreting this but it appears that salinity values for the dry season may be repeated. More generally, the presentation of environmental variables as a sequence of ranges and means is somewhat dense, and this section could be	There was indeed a typo, where it should have been “wet season” instead of “dry season” on line 285. This has now been corrected. Several of the reported means and range values have been removed from this section and only the most impactful values remain. We retain the mention in the

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	streamlined or synthesized to improve clarity for the reader	text that all means and ranges can still be found in the supplementary table.
2.12	Section 3.1.3: This section reads somewhat as a standalone description. While atm conditions are referenced elsewhere in the ms, their relevance and purpose in this section are not clearly articulated. Consider linking more explicitly in discussion.	We thank the reviewer for this helpful comment. We have revised this section to clarify why atmospheric conditions were included, specifically because solar radiation, wind, and rainfall can influence benthic light availability, water-column mixing, turbidity, freshwater input, salinity, and organic matter supply, all of which may affect NEP. We have also strengthened the links between this section and the Discussion, where these atmospheric drivers are now more explicitly considered in relation to seasonal differences and wet-season variability in NEP (Lines 501-509).
2.13	Figure 3: The colors on the graphs do not match the colors in the figure description.	Thank you for highlighting this error. This has now been corrected with the correct colours.
2.14	Line 397: Avoid using baseline in a heavily impacted system.	We have removed the use of “baseline”, as we agree with the reviewer that this phrasing would be misleading in such a heavily impacted system.
2.15	Lines 403-404: What about the role of allochthonous OM inputs in driving negative NEP?	We thank the reviewer for highlighting this potentially important factor in decreasing NEP. We have added text to the discussion that addresses external nutrient input’s impact on community NEP and put it in context with other potential local conditions that may act to decrease the NEP of these communities. (Lines 453-465).
2.16	Line 417: Here you imply that negative NEP is bad for coral reefs, which makes sense, but I think you need to set this up more clearly in the introduction.	We have added additional context in the Introduction explaining that NEP provides insight into community-scale organic carbon cycling and the balance between photosynthesis and respiration. This helps clarify why persistent net respiration may indicate constrained net organic carbon production, while avoiding broader conclusions about reef health or calcification (Lines 63-67).
2.17	Lines 426-427: Vague explanation. Please expand.	We have changed the structure of this paragraph so as to better highlight the factors (both environmental and weather related) that could explain this pattern presented on line 426. We agree with the reviewer that before, the wording was confusing and mislead the reader from the focus of the section. (Lines 490-512).
2.18	Lines 536-537: The implication here is that NEP results inform NEC, but since you did not measure NEC, I think you need to make that connection more clear.	We thank the reviewer for identifying this ambiguity. We agree that the original wording could imply a direct inference from NEP to NEC, which is not supported because reliable NEC measurements are not included in this manuscript. We have therefore revised this section to remove any implication that NEP results can be used to infer calcification, dissolution, or carbonate balance. The revised text now treats NEP specifically as a measure of organic carbon cycling and notes that paired NEP and NEC measurements would be required to evaluate coupling between organic and inorganic carbon metabolism in these communities. And that pairing NEP/NEC measurements could be useful for future studies.

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3.1	This study presents in situ measurements of net ecosystem production (NEP) at three coral reef sites in Hong Kong. The goal of the study was to characterize spatial and seasonal variability in NEP trends across gradients in environmental conditions and benthic community composition. Although the datasets provide valuable insight into spatiotemporal variability of NEP within a region that remains relatively understudied, I have several concerns regarding the interpretation and framing of the results.	We thank the reviewer for their constructive assessment of the manuscript and for recognising the value of the dataset in characterising spatiotemporal variability in NEP within an understudied region. We have revised the manuscript substantially in response to the reviewer's comments, particularly by refining the framing, clarifying the scope of inference from NEP, and strengthening the interpretation of low and variable NEP across environmental and benthic community gradients.
3.2	In particular, the manuscript tends to overextend the interpretation of NEP as a proxy for coral community metabolism and ecosystem functioning without adequately acknowledging the need for complementary metabolic measurements (net ecosystem calcification; NEC) to fully capture ecosystem-scale processes. Additionally, several aspects of the manuscript would benefit from further development, particularly the discussion section where many interpretations are presented without sufficient mechanistic explanation or supporting context. Finally, the reported NEP rates are considerably lower than those in prior studies, and are surprisingly similar between the 66% and 4% coral cover sites. This is a key result that warrants more interpretation and discussion than is currently provided in the manuscript.	We thank the reviewer for this constructive comment. We agree that the original manuscript overextended the interpretation of NEP in places and did not sufficiently distinguish organic carbon cycling from broader ecosystem-scale metabolic processes. We have therefore revised the manuscript throughout to clarify that NEP reflects the balance between photosynthesis and respiration, and that complementary NEC measurements would be required to assess calcification, dissolution, carbonate balance, or broader reef-scale ecosystem function. We have also expanded the Discussion to provide more mechanistic interpretation of the low and often negative NEP values observed across sites. In particular, we now discuss the apparent decoupling between coral cover and mean NEP, noting that coral cover reflects longer-term community persistence, whereas NEP reflects short-term net organic carbon balance during the deployment periods. Although mean NEP was similar between the high- and low-coral-cover sites, the high-coral-cover site showed substantially greater NEP variability, indicating stronger diel and short-term metabolic dynamics despite similarly low average production. We have revised the manuscript to highlight this as a key result and to discuss possible mechanisms, including chronic light limitation, allochthonous organic matter inputs, respiration by non-coral benthic and microbial components, hydrodynamic variability, and differences in benthic community composition.
3.3	Overall, I think this study has merit but I recommend major revisions to address these conceptual, methodological, and interpretive concerns. Please see all the specific comments below	We thank the reviewer for recognising the merit of the study and for providing constructive comments. We have revised the manuscript to address these conceptual, methodological, and interpretive concerns, as detailed in the responses above and below.
3.4	**Abstract** General: I appreciate the concise abstract but if word count allows for it, I suggest including some additional details about the three sites to support the characterization that the coral communities are "persisting under marginal environmental conditions". For example, briefly mentioning coral cover trends, community stability, and/or other long-term metrics would help contextualize this claim alongside the short-term metabolic measurements presented.	We have added text to the abstract's first sentence highlighting several of the local environmental conditions of focus in this study and brief description of the characteristics of Hong Kong communities versus more tropical reef systems. These ideas are expanded upon in the introduction and discussed in relation to measured NEP values in the Discussion section.

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3.5	(Lines 8-9 and Lines 15-17): The use of "adapt" may overstate the biological interpretation of the results. While I suspect the authors likely intended it to mean an adjustment to marginal environmental conditions, it can also imply improved fitness or performance. The NEP measurements identified net heterotrophy at the three sites and the absence of NEC data to accompany these findings makes it difficult to assess overall ecosystem functioning. I therefore recommend more neutral phrasing such as "how coral ecosystems may respond to changing ocean conditions" or "how coral ecosystems may cope with marginal ocean conditions in the future."	Thank you for pointing this out. We have adjusted the wording used as suggested to better fit the scope of the findings of this study. We also noticed several other uses of the word "adapt" throughout the original manuscript, which we have revised with more appropriate wording per the reviewers suggestions.
3.6	**Introduction** General: This section would benefit from additional citations and further development of the conceptual framework. Many statements are currently supported by a single reference despite a large body of literature addressing the topics. Expanding the in-text citations and more holistically synthesizing existing work would strengthen the introduction and overall framing of the study. This is particularly noticeable in the second and seventh paragraphs, both of which are central to the study's motivation and should be expanded upon.	We thank the reviewer for their useful commentary and recommendations for strengthening this section. We have reorganized and restructured the introduction section to better frame our study's objectives in the context of the larger body of literature in related NEP studies. Furthermore, we have removed any suggestion that the present study addresses NEC, or overall community functionality, to help keep focus on this study's specific objectives. We have expanded on the focal ideas that put the importance of this study into focus in the introduction.
3.7	(Lines 27-30): I suggest rewording this to retain the key concepts while avoiding a reliance on definitions from a single study that are not universally defined across the broader literature.	We agree with the reviewer that our previous definitional description of 'extremeness' and 'marginality' was not the best way to communicate these broad terms. While we maintain the "functional marginality" and "structural marginality" framework laid out by Schoepf et al., (2023) for the purpose of providing justification for defining our 3 study sites as "marginal", no further reliance on specific definitions of extreme or marginal from one study are used as these terms are still broadly defined in the literature.
3.8	(Lines 30-31): This sentence could be interpreted as directly equating reef metabolism to ecosystem functioning. While reef metabolism reflects underlying ecosystem processes, it is more of an indicator of functionality rather than functionality in itself.	We have revised this section, and the manuscript more broadly, to avoid equating NEP with overall ecosystem functioning. We agree that NEP is best treated as an indicator of one component of ecosystem function, specifically organic carbon cycling and the balance between photosynthesis and respiration. Because NEC was not measured, we have removed or qualified wording that could imply that NEP alone characterises overall community metabolism, calcification, carbonate balance, or broader ecosystem functioning.
3.9	(Lines 31-33): The statement "marginal conditions, by definition, limit functionality" is	We agree with the reviewer that our original statement defining marginality as limiting functionality was mistaken.

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	overly broad. While marginal environments are often associated with reduced diversity or altered ecosystem functioning, there are also examples of systems maintaining ecosystem functioning under certain conditions. Consider clarifying or rewording this sentence since "by definition" comes across too strong.	We revised this section so as to not mischaracterize the environmental conditions or community characteristics described in this manuscript with black and white terminology.
3.10	(Lines 38-40): This is an example where including additional citations would strengthen the statement. There are many studies that touch on this point, such as the Takeshita et al. publications that are currently in the reference section but not included here. I also recommend rephrasing "lack of understanding" to instead focus on the need for further characterization of NEP across various reef types and environmental conditions. There isn't a lack of understanding, rather a need for broader understanding.	See comment 1.11: We thank the reviewer for their insightful feedback. We agree this section as a whole needed further strengthening and revision. We have removed mention of a "lack of understanding" We agree with the reviewer and added discussion about the need for further characterization of NEP in different coral environments (Lines 578-584). We have removed the sentence in Lines 38-40, and revised the surrounding text to focus specifically on the use of DO gradients to estimate NEP.
3.11	(Lines 42-44): NEP is also an important indicator of biomass assimilation. Consider incorporating this aspect to provide a more complete description.	We thank the reviewer for this suggestion. We agree that NEP can be related to organic carbon availability for biomass production at the ecosystem scale. However, we have chosen to avoid emphasising biomass assimilation in the revised manuscript because NEP alone does not identify the fate of fixed carbon, the trophic compartment in which biomass may accumulate, or the extent to which organic carbon is retained, respired, or exported from the system. We have therefore kept the description focused on NEP as a measure of organic carbon cycling and the balance between photosynthesis and respiration, while noting more cautiously that positive NEP may indicate potential net organic carbon production within the community.
3.12	(Lines 45-50): This paragraph mentions that the physical environment impacts NEP but does not expand on that point. Suggest briefly including how key physical drivers, such as residence times, hydrodynamics and tidal variability, can influence metabolic processes.	We have added here specific physical variables that have been shown to influence NEP rates, however we refrain from expanding on their specific impacts on NEP as, firstly, the gradient flux method by its nature is sensitive to flow conditions, and may not capture periods where key physical drivers are controlling the NEP of the system. Secondly, the impact of residence times, which is the main physical feature of interest in our sites (low flow), has been shown to have mixed impacts on NEP, with evidence that increased flow can stimulate production (Finelli et al., 2006; Shakya & Allgeier 2023) but that increase flow also stimulates increased respiration (Patterson et al., 1991; Long et al., 2013). We thus refrain from further discussion about these physical drivers in this section (Lines 60-61).
3.13	(Lines 51-52): This comment relates to my earlier point about the definitions of "extremeness" and "marginality" (see Lines 27-30). In those earlier definitions, marginality appears to refer to community characteristics, whereas here, both terms describe environmental conditions. Clarifying whether these terms are distinct or synonymous would improve readability.	See comment 2.3: We thank the reviewer for highlighting this ambiguity. We agree that the previous wording did not apply these terms consistently. To improve clarity, we have revised the manuscript to remove the use of "extreme" as a separate framing term and to define "marginal conditions" more explicitly and mechanistically. We now distinguish between marginal environmental conditions, including low light availability, turbidity, strong seasonality, freshwater influence, sediment / nutrient exposure, and urbanisation-related pressures, and community or functional marginality, including

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		patchy coral development, simplified community structure, and low or negative community-scale NEP. These revisions clarify that "marginal" is not being used solely to describe coral cover, but rather the broader environmental and functional context in which Hong Kong coral communities persist.
3.14	(Lines 56-59): Consider providing additional details on the dominant bioeroders at the sites (e.g., parrotfish, urchins, sponges), as this information can provide important context for the measured NEP rates.	We have added mention in this statement of the dominant bioeroder, sea urchins, that impact Hong Kong coral communities (Line 43). We agree with the reviewer that specifically mentioning the type of bioeroder is wise here, in order to reduce speculation on what type of bioeroder we are referencing.
3.15	(Lines 61-64): This statement would benefit from additional references, if available.	Because this section was discussing impacts on 'coral growth' specifically, we have decided to remove this statement so as to not confuse the reader into thinking this paper's focus is on coral growth, calcification rates, NEC, or any related topic.
3.16	(Lines 65-70): Please expand this paragraph since it is highly important to your study. What type of adjacent anthropogenic activities and why would those impact susceptibility? Additionally, the current wording seems to imply that turbid environments are favorable reef habitat. While turbidity can reduce heat and light stress, those are typically acute benefits that occur during thermal stress events. This context is needed since oligotrophic conditions are generally considered favorable habitats, so clarifying this would strengthen the introduction.	We thank the reviewer for highlighting the need for additional context when discussing the pros and cons of turbid environments. Before we mention the positive benefits of added turbidity in reducing thermal stress during heating events, we have added important caveats on the downsides to the community of turbid waters, including a compressed depth range, and higher susceptibility to sea-level rise (Lines 30-31). This helps better balance our discussion of turbid reefs from sounding overly supportive, but maintaining the critical point that turbid conditions are expected to spread across more coral communities in the future, so they are still critical marginal environments to research in the present day.
3.17	(Lines 79-81): Please be careful when equating NEP with coral community metabolism. While NEP is an important indicator of organic carbon cycling, the absence of NEC limits the ability to fully characterize community metabolism and ecosystem functioning. The current wording is not incorrect, but should be clarified to reflect this distinction.	We thank the reviewer for pointing out this necessary distinction. Rephrasing has been completed throughout the manuscript to clarify that we measured community-scale organic metabolism, rather than oversimplifying the term used to 'community metabolism' without measured NEC.
3.18	**Methods** General: The methods section would benefit from additional details throughout, particularly regarding site-specific benthic community composition, instrument configuration, and data processing. Clarifying these components would improve reproducibility and confidence in the results. Additionally, the direct comparison between wet and dry season needs further justification, as the sampling periods are separated by ~10 months and three of the four wet season time points could represent seasonal transition periods.	We thank the reviewer for their beneficial synopsis of the Methods section. See comment 3.20 regarding site-specific benthic community composition. We have added additional information of each sensor used to make reproducibility of our deployment instrument configuration easier. Further justification behind our data processing steps has been included for added clarity behind our filtering methods and analyses. Further justification has also been added to ensure our late-wet season deployment may still be considered representative of wet season conditions and not yet transitional period through long-term analysis of SST data from both the wet and dry seasons versus our specific deployment times showing strong agreement between our

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		deployment's water temperature and that of the long-term recorded wet season's SST in Hong Kong. We highlight again that the wet season of 2022 was extraordinarily warm and that wet season water temperature conditions were extended later into the year than normal during this time (Lines 123-131).
3.19	(Lines 91-94): Consider adding mean values (+/- SEM) alongside the reported ranges to provide a clearer comparison across seasons. SEM is more meaningful for data interpretation, so alternatively that could replace the ranges entirely.	We thank the reviewer for their useful feedback on how to represent the data. We would like to clarify that the purpose of Figure 1 is to show the spatial west-east gradient in EPD variables, not to be interpreted as a seasonal comparison. The figure caption has been revised to highlight this distinction. However, we have added the means ((+/- SEM) of the EPD variables in the Results section **(**see section 3.1.2) to describe the seasonal summaries, while still reporting ranges to describe the spatial gradient.
3.20	(Lines 102-104): This statement would benefit from including quantitative coral cover values for each site.	We thank the reviewer for their suggestion. We included the quantitative coral cover values at each site in the results section (see section 3.1.5), as we think these quantitative values fit better in the Results, rather than in the methods section, where we provide a more qualitative assessment of each site as described by previous studies.
3.21	(Lines 109-110): The use of "most marginal" is too qualitative for the point being made. Suggest specifying the environmental and/or ecological characteristics that support this terminology.	We agree that the "most marginal" terminology was too qualitative. We have replaced this wording with wording that focuses on the point that this site lies as the westernmost extent of coral communities in Hong Kong (Line 110).
3.22	(Table 1 and Lines 120-125): The distinction between wet and dry season requires further support. Several wet season time points could be classified as transitional periods between wet and dry season. The "prolonged wet season in 2022" should be justified with observational data and quantitative values that support this claim, rather than inferred through "prolonged elevated temperatures." Providing this type of evidence would strengthen confidence in the seasonal comparison.	As also noted in response to Comment 1.14, we have revised the manuscript to provide clearer methodological justification for treating the October 2022 deployments as late-wet-season deployments. We now explicitly acknowledge that October can represent a seasonal transition period in Hong Kong, and we provide additional quantitative support showing that conditions during the 2022 deployments remained closer to wet-season than dry-season conditions. Specifically, we note that autumn 2022 was anomalously warm and add measured water temperature values from the deployment period to support the classification as prolonged late-wet-season conditions (Lines 122–130).
3.23	(Lines 126-135): The use of monthly averaged data may not capture conditions during short deployment periods so this limitation should be acknowledged and carefully considered throughout the manuscript.	We thank the reviewer for this important clarification. We agree that monthly averaged EPD monitoring data cannot fully resolve environmental conditions during short deployment periods or be used to explain high-frequency NEP variability directly. We have revised the manuscript to make clear that the EPD data are used to characterise the broader spatial and seasonal environmental context around Hong Kong, rather than to provide exact conditions during each deployment window. We have also added this limitation to the Methods section (Lines 142-144) and have taken care not to overinterpret monthly monitoring data in relation to short-term NEP dynamics.
3.24	(Lines 160-161): The position of the lower sensor height (Z_2 = 8cm) is closer to the benthos than prior gradient-flux studies.	We thank the reviewer for this helpful comment. We have added justification for the lower sensor height used in this study. The lower sensor was positioned at 8 cm because the

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	Since this can lead to different boundary layer assumptions, consider adding justification for this configuration and clarify how this might influence these assumptions and subsequent flux calculations.	Hong Kong coral communities sampled here have relatively low and patchy benthic relief compared with many higher-relief reef flat or spur-and-groove settings, so this height was selected to remain within the near-bed boundary layer while avoiding direct obstruction by the benthos. We have also added comparison with previous gradient-flux studies, noting that lower sensor heights vary among studies, including values close to our configuration, such as 10 cm in Pisapia et al. (2019), and higher placements such as 20 cm in McGillis et al. (2011) and Boles et al. (2026), and 30 cm in Takeshita et al. (2016). We now clarify that data were excluded whenever boundary-layer assumptions were not met, including periods when the velocity structure was inconsistent with the law-of-the-wall relationship or when flow / DO conditions were unsuitable for reliable gradient-flux calculations (Lines 174-177).
3.25	(Lines 178-188): Additional details on instrument specifications (e.g., accuracy, calibration procedures, and relevant settings) would improve reproducibility of the methods.	We thank the reviewer for this suggestion. We have added additional instrument specifications to improve reproducibility, including sensor accuracy, calibration or cross-calibration procedures, sampling intervals, and relevant deployment settings. Specifically, we now report the accuracy of the DO, PAR, and CTD sensors, note that DO sensors were cross-calibrated after deployment, clarify that Vectors sampled at 16 Hz using 60 s bursts, state that sensors recorded one data point every 60 s and were averaged over 10 min for analysis, and clarify that CTD profiles were processed in 0.1 m depth bins.
3.26	(Line 184): Clarify whether the sensors recorded continuous measurements, discrete pulse measurements, or a single measurement within each 60-second interval. This distinction is important for evaluating data quality and the filtering steps applied.	Additional wording to make clear that each sensor recorded one data point every 60 seconds and that vectors were set to burst sampling has been added for clarity.
3.27	(Lines 195-196): Please provide additional detail on the velocity threshold used. This would enable direct comparisons between the thresholds used in prior studies (e.g., <0.5 cm s^{-1} in Khrizman et al. 2025).	Additional detail on the reasoning behind the velocity threshold has been added to the beginning of section 2.6 (Lines 212-216). The gradient flux method requires flow high enough to induce turbulent mixing, but low flow conditions differed among deployments, so a fixed cutoff was not ideal. Thresholds for velocity vary across studies, as the reviewer had mentioned Khrizman et al., 2025 (Also see Platz et al., 2020; 3 cm s^{-1}). These cutoffs will be site-specific. A fixed cutoff at a higher value would have likely removed too much data from these low-flow Hong Kong deployments, while a very low universal cutoff may not adequately account for deployment specific flow conditions.
3.28	(Lines 203-206): A substantial proportion of the data was removed during the filtering step. Given this, additional detail on the filtering criteria and justification for data removal would improve confidence in the remaining dataset.	Additional justification for the importance of each data filtering step has been added to section 2.6, highlighting that each of these steps is necessary to remove NEP estimates when gradient flux assumptions were violated. It is also worth noting that our dataset had a higher proportion of data removed than most other GF studies (but see Platz et al., 2022; 65% data removed in Aug 2018 dataset). This is due to Hong Kong not being ideal for continuous gradient flux measurements because flow can be weak or variable, communities are patchy, and unstable boundary layer gradients can occur. The retained dataset ensures that reported NEP values were calculated only when gradient flux

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		assumptions were satisfied, but these values should not be interpreted as continuous averages across every condition experienced during the deployment.
3.29	**Results** General: The NEP rates are consistently low across all sites, and it is surprising that values are broadly similar between the two with markedly different coral cover (Sharp Island at ~66% and Sham Wan at ~4%). Additional context or discussion would help explain why NEP does not appear to drastically change with coral cover or overall benthic composition, as might be expected. Clarifying whether this reflects methodological constraints would strengthen interpretation of the results.	We thank the reviewer for their constructive feedback. We have revised the discussion to highlight possible mechanisms behind the observed NEP rates between sites and put our NEP rates in better context with other studies. We also acknowledge that the gradient flux approach may not be the ideal method to measure high-frequency benthic flux measurements in Hong Kong. This is seen in our data filtering steps necessary to remove any artefactual measured NEP values, and we have added mention in our discussion about the gradient flux limitations and assumptions that may be violated during portions of our deployment times. (Lines 512-515, 522-530, 595-599).
3.30	(Lines 254-255): Please clarify whether the reported values are mean +/- SD or mean +/- SEM. This distinction should be stated clearly the first time it is used in the results section.	We thank the reviewer for highlighting this oversight. Added mention that this is SD is now included in the opening sentence of this results section (Line 276).
3.31	(Lines 281-293): The EPD monitoring data provides useful environmental context, but the limitations should be made clearer. These values are based on periodic surface measurements and monthly averages, which may not fully capture the variability experienced during the shorter 7-14 day deployments.	See comment 3.23: We have made an additional clarifying statement in the methods section discussing the EPD data to make clear that this data is used only to characterize broad spatial and seasonal environmental context, rather than the exact environmental conditions experienced during each of the short-term deployments.
3.32	(Lines 298-299): The lack of significant rainfall differences between the wet and dry season deployment periods suggests that the seasonal distinction may be less clear-cut than presented. I recommend providing strong clarification for how these deployment periods properly represent wet versus dry season conditions.	See comment 1.25: We have added the important caveat that, while rainfall did not significantly differ between specific deployment periods in the wet and dry season, the rainfall was significantly different between the wet and dry season when averaged over the entire season. This needs to be highlighted as our deployment periods only lasted at most a few weeks, and the weather conditions observed during them may not be representative of the seasonal differences in full.
3.33	(Figure 3): For clarity, consider indicating directly in the figure the time periods of each deployment. This could allow for easier interpretation and replace the (# days) sub-text.	We thank the reviewer for this helpful suggestion. We agree that the deployment dates are more informative than the number of days for each deployment, and we have added these date ranges to the figure.
3.34	(Lines 327-329): This provides important support for why Sham Wan is treated as the marginal site. Suggest introducing some of this context earlier in the manuscript so that readers understand this distinction before reaching the results.	We have added a mention in the Study Sites section pointing out that Sham Wan is dominated by rocky substrate. (Lines 108-109). Additionally, we have rephrased Sham Wan as not a ‘control site’, but rather the westernmost site of coral extent in our study.
3.35	**Discussion** General: The interpretation of NEP as a proxy for coral community metabolism and	We have tightened the Discussion to focus on NEP-related organic carbon cycling, production, and respiration. We provide a brief mention in the future directions section for future studies integrating NEC measurements to fully

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	ecosystem functioning should be treated with more caution. While NEP provides important insight into organic carbon cycling, it does not capture inorganic carbon cycling (calcification and dissolution) and therefore does not fully characterize community metabolism. This distinction is important to address within the discussion section.	characterize the community metabolism. A significant portion of the discussion has been rewritten to better contextualize our NEP results without over-interpreting our findings beyond what organic carbon cycling can inform us about.
3.36	(Lines 403-406): Suggest broadening this interpretation since these patterns could also reflect differences in benthic biomass and community composition, rather than physiological performance alone.	We thank the reviewer for their thoughtful comment, and we agree that an expansion of the explanation for our observed respiration patterns is necessary. This specific excerpt and section 4.1.1 has been revised to more fully characterize what variables may have been responsible for the net respiration, beyond just low light.
3.37	(Lines 412-415): This is an incomplete characterization of the relationship between environmental drivers of NEP. While increased PAR can enhance photosynthesis, elevated temperatures during the wet season can also increase respiration. Given that all sites are largely net heterotrophic, this interaction introduces additional complexity that should be acknowledged.	We agree with the reviewer that the increased PAR in the wet season cannot be viewed in isolation when explaining seasonal differences in NEP. We have added discussion addressing the differences in environmental variables between seasons, including temperature, and how those may also alter observed NEP differences between seasons.
3.38	(Lines 427-429): This is an important point but is underdeveloped in its current state. Consider expanding on the underlying mechanisms or providing supporting context to strengthen this interpretation.	We thank the reviewer for highlighting the underdeveloped point discussed in this section. Additional description of each measured environmental variable that could have contributed to the seasonal difference in NEP has been added, beyond just the original one sentence brief mention that was present originally (Lines 490-501).
3.39	(Lines 437-439): This sentence is difficult to follow and the underlying concept would benefit from clarification.	We agree with the reviewer that sentence was unnecessarily wordy and overall not needed to illustrate the point we were trying to communicate. This sentence has been removed, and the proceeding sentence has been revised for clarity on why strong stratification may lead to a violation of GF assumptions.
3.40	(Lines 449-452): There appears to be a notation and unit inconsistency here. In the methods, z_1 and z_2 are defined as the sensor heights (z_1 = 124 cm and z_2 = 8 cm), whereas here they are presented with incorrect values, reversed notation, and are described in the context of bottom water velocity.	We agree with the reviewer the wording was confusing. We were attempting to show the height at which the top and bottom velocities were measured. Additional wording has been added to add distinction between height and velocity here (Lines 525-527).
3.41	(Lines 456-458): Please develop this point further, as benthic community composition is likely a stronger driver of metabolic activity than diversity. Several parts of this sub-section put too much emphasis on diversity being a main contributor.	We thank the reviewer for pointing out this overly diversity-focused angle of this section of text. This was misleading and has been rephrased so that rather the abundance of metabolically active organisms and dominance of rocky substrate are a better explanation for the low NEP variability observed at Sham Wan, rather than diversity.
3.42	(Lines 471-474): This statement is not well supported by prior studies and would therefore benefit from additional context and further development.	We agree with the reviewer that this section describing bleaching impacts on NEP was underdeveloped. We have revised the discussion to community-scale responses to bleaching events are context dependent and have shown a

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		range of responses in prior studies. We also make clear that bleaching is only one of several possible influences on NEP at our sites and we refrain from implying that bleaching alone explains the low NEP rates observed (Lines 546-554).
3.43	(Lines 487-489): Please avoid generalizing the results to all coral reef habitats, as this is not broadly supported by the literature. Consider rephrasing to focus specifically on the three study sites.	We have added additional context to highlight that the statement is in relation to the specific study sites, and not generally applicable to all coral habitats.
3.44	(Lines 528-530 and Lines 536-537): Please keep the discussion focused on production and respiration since NEC was not measured in this study. Statements referring to “calcification” or “growth” should be avoided.	We have reframed the manuscript to only focus on implications of the measured variable of this study, NEP. The only mention of NEC we have included is when discussing possible future directions of research, where we highlight that including NEC in future metabolism studies could be beneficial for a fuller characterization of the community’s function (Lines 585-593)