#	Comment	Response
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.

#	Comment	Response
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.

#	Comment	Response
	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

#	Comment	Response
		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

#	Comment	Response
		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

#	Comment	Response
	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

#	Comment	Response
		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

#	Comment	Response
	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

#	Comment	Response
		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)