#	Comment	Response
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.