#	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.