

Peer Review File

Sustained coral reef growth in the critical wave dissipation zone of a Maldivian atoll



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22nd Sep 21

Dear Professor Kench,

Your manuscript titled "Coral reef growth in the critical wave breaking zone can retain the wave protection function of reefs under moderate sea-level rise" has now been seen by 2 reviewers, and I include their comments at the end of this message. They find your work of interest, but some important points are raised. We are interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. The reviewers see great potential in the observations presented, but are unconvinced that they provide robust support for the projections and wider conclusions given the temporal and spatial limitations of the study. We therefore ask that you tone down, caveat or remove unsupported conclusions as suggested by both reviewers. Please highlight all changes in the manuscript text file.

We are committed to providing a fair and constructive peer-review process. Please don't hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the referees' comments (which should be in a separate document to any cover letter) and the completed checklist:

[link redacted]

** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first **

We hope to receive your revised paper within six weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

We understand that due to the current global situation, the time required for revision may be longer than usual. We would appreciate it if you could keep us informed about an estimated timescale for resubmission, to facilitate our planning. Of course, if you are unable to estimate, we are happy to accommodate necessary extensions nevertheless.

Please do not hesitate to contact me if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Dr Clare Davis
Associate Editor
Communications Earth & Environment

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REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

General Comments

The topic of coral reef response to sea level rise is one of considerable scientific and informed public interest so this paper is highly appropriate for the journal. It is well written, generally (see below) well argued, and well structured. I do think, however, that it should be made clear that these results are derived from one location only, at a small scale (the survey areas are 0.25m²) and at only 4 sites around this particular atoll rim. I think it would be really helpful to the reader to have some more background information on the field site; there is none at all. What is the tidal range? What does the wave climate look like over a year? What is the local rate of sea level rise? What has been the history of recent ocean temperature variations and incidence of bleaching? Furthermore, the authors should not over-claim the wider application of their findings and the addition of some caveats, at several points, would seem sensible. This study is from an open ocean site in the Indo-Pacific reef biogeographical province where reef margins are characterised by algal rims and backed by cemented reef flats which may be several km in width and that often dry at low tide. This 'reef crest' setting is quite different from those seen on Atlantic province reefs where the reef crest is typically characterised by branched or bladed corals and is back by a shallow sand-field lagoon. Some of the general literature quoted on reef growth performance in this paper is from Caribbean reef corals and therefore there is a potential disconnect between the general arguments of the paper and the specific field setting. There is also a concern about the validity of an argument that is based on two years' field data. It has long been known that long-term geological rates of reef accretion are much lower than short-term measurements. In some locations, there are breaks in reef growth after the reef has been impacted by some form of disturbance – this might be the physical removal of reef by hurricanes and cyclones (presumably this Maldivian site is storm-free?..) or a hiatus in reef growth caused by bleaching (which does seem to be potentially relevant here), disease or change in nutrient status. The recognition of these longer term processes I find missing from this manuscript; the more geological perspective is lacking. These are not necessarily matters that the authors can resolve but they should certainly add these caveats to their manuscript.

Specific comments

Line 11: this is slightly oddly worded. It is not sea level rise per se that will limit capacity but rather the response of reefs to that sea level rise that is important. Needs re-phrasing (line 22 is much better...).

Line 24: is there any hard evidence from anywhere that a loss of reef elevation has actually led to quantified increases in beach erosion and shoreline retreat? (I discount modelling studies here). Is this still an untested assumption? If there is evidence it should be reported here.

Line 24-25: the control is not necessarily just elevational. It might depend also, for example, on the width and nature of the reef between the reef margin and a shoreline which might itself be of varying 'resistance'.

Line 27; 'cumulative vertical growth' suggests an uninterrupted, linear process. No timescale is given. But many reef surfaces have complex histories of changing level, not independent of timescale (a reef may have tracked sea level rise during the Holocene but at the sub-millennial scale have experienced periods of stasis or even elevation loss).

Line 30: it would be helpful to have an Indian Ocean inset map for location. I think it has to be made clear in the Figure legend that this is a typical cross-shore profile for an Indo-Pacific atoll. The fore-reef slope would be quite different on Caribbean reefs and where modification of incoming waves takes place well before the reef crest (see HH Roberts) and the term 'reef flat' would only apply in perhaps one or two locations (see Glynn(1973) and Milliman (1973)

Line 40: where did Holocene reef growth commence at 10K? Surely in most locations commencement would be more like 8 ka BP.

Line 46: or where changes in storminess (changes in frequency, magnitude or extension into currently storm-free areas with ocean warming) take out growth increments. Less than catastrophic mass coral bleaching may not kill corals but it will lead to a period of zero vertical growth. Meantime the bioeroders will keep bioeroding...

Line 62: repetition from lines 40-41. Not required twice.

Line 67-70: not entirely true. What about the application of the 'alkalinity depletion' method by Steve Smith and Don Kinsey, at a range of scales, up to and including the 'whole reef system'?

Line 70-72: there is a fundamental methodological issue here of comparing point-in-time census measurements with rates of sea level rise assessed on, at best, decadal timescales and where the sea level rise record may be at best regional and not site-specific to the site of the census.

Line 72-73: I thought that one of the findings of the Smith and Kinsey research was that there were not regional variations...

Line 85: 'lesser importance'. that may be true in the Indo-Pacific but it might not be the case elsewhere

Line 88: only partially true in the Caribbean. Here the reef crest typically does contain corals, in encrusting and bladed forms, as well as coralline algae.

Line 93: in the absence of a reef flat, wave re-formation can take place (although the energy distribution is quite different from a typical incident trade wind generated wave (see Roberts))

Line 104: it seems odd to only bring in Smith and Kinsey here...

Line 106-116: I think it is important to point out that these are very local measurements – in the context of the scale of the reef margin, point measurements only. And I think it should be said here that they are from a short time period also.

Line 122, 127: are these differences statistically significant?

Line 161: were environmental conditions over this two year period typical of the site in the longer-term? The manuscript gives no information on the environmental setting of this reef. Does it undergo strong seasonal shifts in hydrodynamic forcing as a result of the Asian monsoon? The measurements were made in February 2018, 2019 and 2020. What are conditions typically like in this month at the field site?

Line 349: Is the site affected by longer-period water level oscillations associated with the Indian Ocean Dipole?

Line 167: The reefs of St Croix are quite different in terms of their general ecology and topographic characteristics, the latter in part due to a fundamentally different sea level history over the last 6,000 years. This is not a meaningful comparison.

Line 172-173: it is odd that the reader is not provided with any general information on typical cover percentages in the quadrats for different groups of organisms but only at-a-point data when this coincides with a transect pin. Just how much 'bare space' is typical? The detailed data reported here only have real value if they are seen in a wider, entire reef margin context.

Line 182-183: this is interesting. But it is difficult to interpret further without information of those levels of rugosity and coral cover.

Line 185: was there a well documented storm event/s in the study period? Or is this pure speculation?

Line 188: but these echinoids were lagoonal?

Line 210-211: so are these comparisons really meaningful?

Line 217 – 229: these points are valid and well argued but in the longer-term the challenge is to come up with estimates of reef performance at the 'whole reef' scale because this is the scale that affects island lives and livelihoods. There is every likelihood that controls change as time and space scales change.

Line 242: in a) are these global sea level rise scenarios rather than regional ones? If so, that point should be made clear. In b) the green / gray / yellow shadings are not explained. Again, it should be made clear that this is an Indo-Pacific reef setting.

Line 262-263: so you are arguing from a two year study to a 'multi-decadal transition'? Ok – but I think that you need to add some caveats at this point.

Line 269: addition of boulders. Is this likely? Has this happened in the past? Or is this speculation?

Line 277-278: more detail of the bleaching event please. Did this event compromise the results from study years reported here? If not, why not?

Line 279-289: the degree to which site measurements can inform 'whole reef' future trajectories surely is still debateable. There must be scale effects.

Line 301: how were the 4 sites chosen on such a large diameter reef margin?

Line 306: does this mean 5 m behind the spur and groove? Does the presence of spur and groove affect the patterns of flow and wave breaking at the 4 sites?

Line 328: it is very good to see a proper estimation of error!

Line 349: I think the methodology here needs some support from referencing.

Reviewer #2 (Remarks to the Author):

Review of "Coral reef growth in the critical wave breaking zone can retain the wave protection function of reefs under moderate sea-level rise" manuscript by Kench et al.

Overall, the paper is well-written and the figures are of good quality. This study presents a new type of measurement of current reef crest accretion, which is greatly needed to understand the future of coral reefs in protecting shorelines and one of the only, if not the first, of its kind. The methodology is fantastic to make such measurements, and I believe these are the first of their kind and thus an extremely valuable contribution to the scientific literature.

Stating that, the leap from measurements over 2 years of unknown forcing to decades of net reef crest accretion to suggesting that reef crests can keep up with projected sea-level rise seems to a step too far based on our understanding of net coral reef accretion. Lucien Montaggioni's (2005) Earth-Science Reviews paper and the references included therein note a few main points that put such an extrapolation into question:

1) Net vertical accretion rates of framework are highly variable and not directly related to coral or CCA growth rates – in general, net vertical accretion rates are generally one order of magnitude lower than coral or CCA growth rates. Thus the measurement of contemporary accumulation rates are likely lower, and maybe up to an order of magnitude lower, than net vertical accretion rates over decadal timescales.

2) Most records of coral framework accretion are composed of not in-situ growth, but rather broken corals and sediment (rubble down to silts) bound by CCA or recrystallization processes. That, along with the long-held intermediate disturbance hypothesis for coral reefs (Connell, 1978, and updated by Rogers, 1993; Arnsen, 1995; etc) suggest that intermittent (decadal+ return interval) storms play a huge role in the development of coral reefs.

3) Numbers 1&2 suggest that contemporary measurements of coral and/or CCA growth demonstrated by the authors and presented in many of the references are not translatable to net vertical accretion rates, for that would neglect carbonate losses. For example, most of the references on contemporary assessment of carbonate production then make the assumption that all carbonate production gets incorporated into the framework, with no loss of carbonate from the reef, which we know occurs as offshore transport of sediment in grooves of spur-and-groove formations and net accumulation of carbonate sediment aprons at depths below active coral growth. Furthermore, it neglects the role of disturbance that can provide more material to a given location on the reef to help accumulation and/or remove that material from the reef, causing erosion. Thus the erosion values presented in Figure 3 and 4 are indicative of the measurements during the time period of unknown forcing (see below) and thus possibly (likely?) are underrepresented.

The presentation of monthly average wave information in Supplementary Figure 3 and the comparison of that average data...which of course could be a 2 m average for the month of July, with the vast majority of the time being below 1 m and a very small, but geomorphologically important percentage of the time being above 5 m....to the 2 months of data in Supplementary Figure 4 is inconclusive. I would strongly suggest replotting Supplementary Figure 3 with the mean plus the 95% waves (H95) and then create a new Supplementary Figure 4 showing those same monthly statistical values for the 2-year study period. Such data would put the measurement period into the longer-term patterns and relative to disturbance events.

4) Lastly, and as shown in Figure 4b, the projections of the algal reef crest accreting faster than then reef flat would cause a pattern of an elevated algal reef crest relative to the reef flat. If this process is projected to occur, why has there been no evidence of the same on the many reef crests in the western tropical Pacific where sea level has increased by 30 cm over the 1990-2010, per Merrifield (2011)? Furthermore, where is evidence of such processes and the resulting imbalance of reef crest to reef flat accretion in the Holocene?

Again, I think the measurements presented in the manuscript are sorely needed and a great advancement in our understand of short-term development of algal reef crests. Stating that, I do not feel the geologic record, our contemporary understanding of reef net accretion processes, and the limited forcing data support the extrapolation of the outstanding but limited contemporary data to decadal timescales the support the main point of the manuscript that algal reef crests might be able to keep up with projected sea-level rise.

Response to Reviewer Comments

Reviewer #1 (Remarks to the Author):

General Comments

We thank Reviewer 1 for their thoughtful comments on our manuscript and we believe our responses have improved/clarified the intent of the manuscript. We note that the Reviewer does appreciate the high value of the measurements this new approach can yield in considering contemporary reef accretion rates.

The topic of coral reef response to sea level rise is one of considerable scientific and informed public interest so this paper is highly appropriate for the journal. It is well written, generally (see below) well argued, and well structured.

We thank the reviewer for their recognition of the importance of our study. Understanding the implications of future sea-level rise, and the role of continuing reef growth to mitigate wave exposure, is critical for tropical small island nations, such as the Maldives and those in the wider tropical Indo-Pacific.

I do think, however, that it should be made clear that these results are derived from one location only, at a small scale (the survey areas are 0.25m²) and at only 4 sites around the atoll rim.

We have ensured the scale of sites and number of sites are more clearly stated.

This study presents the first attempt (to our knowledge) to obtain quantitative field measurements of physical net vertical change in reef growth. Previous high-profile assessments (e.g. Perry et al., 2018, *Nature*) of contemporary, and future projections of vertical reef elevation changes, are derived from census based ecological surveys with assigned calcification and bioerosion data (carbonate budgets), then scaled uniformly over total reef area. These carbonate budget studies are typically derived from 3-5 replicate 10 m transects on the fore-reef, well below the critical wave breaking zone. Furthermore, they are typically based on single surveys, yet they extrapolate well into the future. Here, we have attained measurements from a wider geodiversity of reef habitat types (4 sites) on the atoll rim – which represent the range of benthic assemblages at this site, based on expanded drone survey and benthic ecological datasets from the location (see supplementary information). In contrast to carbonate budgets, our results offer direct measures of net reef growth/change at unprecedented high-resolution. We do recognise the limitations of the small spatial coverage of our approach, but do not consider this sampling approach is inconsistent in spatial and temporal scale from other published ecological research seeking to resolve questions of reef growth (e.g. Perry et al., 2018 or Cornwall et al., 2021).

I think it would be really helpful to the reader to have some more background information on the field site; there is none at all. What is the tidal range? What does the wave climate look like over a year? What is the local rate of sea level rise?

This is true in terms of the manuscript text. However, a full description of the field location is presented in the *Supplementary Information 2: Field Setting*. Given the extent of this information we took the decision to include this in an extended supplementary file (9 pages) that does include: spatial location in the Indian Ocean and along the Maldivian archipelago; physiographic setting; Reef flat topography; ecological characteristics and coral community structure of the study area, and Oceanographic characteristics which includes a summary of rates of sea-level rise in the archipelago, tidal regime, recent sea level records, incident wave climate, and data on the efficacy of the reef at the study site in transforming wave energy.

What has been the history of recent ocean temperature variations and incidence of bleaching?

The Maldives has experienced several major mass bleaching events. We have added details of recent ocean warming temperature anomalies and their impacts on coral assemblages in the text and in the supplementary file. Specifically, the 2016 bleaching event caused widespread mortality on southern Maldivian coral reefs (e.g. Perry and Morgan, 2017; *Scientific Reports*) nearby to our field sites, is well documented.

Furthermore, the authors should not over-claim the wider application of their findings and the addition of some caveats, at several points, would seem sensible.

We acknowledge the need to not over-claim the application of the findings and have been careful not to suggest our findings are applicable to reefs globally. We have injected a number of caveat statements to address this concern and included a number of statements to indicate the results are most relevant to Indo-Pacific reef settings.

This study is from an open ocean site in the Indo-Pacific reef biogeographical province where reef margins are characterised by algal rims and backed by cemented reef flats which may be several km in width and that often dry at low tide. This 'reef crest' setting is quite different from those seen on Atlantic province reefs where the reef crest is typically characterised by branched or bladed corals and is back by a shallow sand-field lagoon.

We agree that spatial variability in reef geomorphology and associated habitat zonation occurs between reef-building provinces. This is most pronounced between Indo-Pacific and Atlantic reef systems due to fundamental differences in coral populations. Although our manuscript does not aim to present a global study, the habitats and reef types we present here are very representative of atoll rim reefs in the Maldives, and other reef sites throughout the tropical Indo-Pacific – where the majority of low-lying islands are at risk of the impacts of sea-level rise. A major objective of the study was to provide proof-of-concept for the use of *in-situ* reef accretion/erosion data, which can then be scaled to more sites, and to capture a wider range of reef environments.

We have amended the text to clarify the application of results to Indo-Pacific reefs. We also note in conclusion the importance of expanding this method to other reef settings.

Some of the general literature quoted on reef growth performance in this paper is from Caribbean reef corals and therefore there is a potential disconnect between the general arguments of the paper and the specific field setting.

There is a global scarcity of Holocene reef geological datasets, with much of the available literature focused in several key geographic areas. Early studies of geological reef development were conducted on Caribbean and western Atlantic reefs, so although reef types may differ from the Indo-Pacific reefs, these often provide the most relevant (and only) data. Despite differences in reef communities that drive carbonate production, the ultimate interactions between reef bioconstruction/reef vertical accretion and wave breaking remain.

We have also amended the text to connect our data more closely with geological estimates of reef growth in the Indo-Pacific as summarized by Montaggioni (2005).

There is also a concern about the validity of an argument that is based on two years' field data. It has long been known that long-term geological rates of reef accretion are much lower than short-term measurements. In some locations, there are breaks in reef growth after the reef has been impacted by some form of disturbance – this might be the physical removal of reef by hurricanes and cyclones (presumably this Maldivian site is storm-free?..) or a hiatus in reef growth caused by bleaching (which does seem to be potentially relevant here), disease or change in nutrient status. The recognition of these longer term processes I find missing from this manuscript; the more geological perspective is lacking. These are not necessarily matters that the authors can resolve but they should certainly add these caveats to their manuscript.

We agree that inherent difficulties arise when projecting short-term measurements over longer timescales. Ecological data (based on single surveys) are now commonly being converted into future reef growth estimates. As the Reviewer points out, it is difficult to conduct measurements over the geologically relevant time-scales to capture changes in reef growth bioconstruction from ecological disturbance and recovery. Our study is underpinned by the assumption that under current ecological conditions, vertical changes in reef surface elevation measured in the critical wave breaking zones, provide a sensible basis to consider future growth. Our future projections, which are set up to align with other recent studies that have used ecological census data to derive reef growth (e.g. Perry et al., 2018, *Nature*), span ecological to societal timescales, rather than longer geological timescales. However, we have amended the text to better highlight the geological perspective.

Specific comments

Line 11: this is slightly oddly worded. It is not sea level rise per se that will limit capacity but rather the response of reefs to that sea level rise that is important. Needs re-phrasing (line 22 is much better...).

We agree and have re-phrased the opening sentence.

Line 24: is there any hard evidence from anywhere that a loss of reef elevation has actually led to quantified increases in beach erosion and shoreline retreat? (I discount modelling studies here). Is this still an untested assumption? If there is evidence it should be reported here.

We agree with the general thrust of this comment and have been frustrated throughout our research careers that many assertions of impacts have relied on numerical modelling without compelling field evidence. However, it is widely reported in the literature that increased flooding and erosion has/will have such impacts. We think it important to highlight these assertions here.

We have modified the referencing here to also include a number of studies that do have field validation of flood risks (Ref 7), and we also add the recent study by Tuck et al. 2019, who examined reef island shoreline responses to increased sea level using a scale model in a physical flume tank. Studies on this specific topic are later discussed more fully in the introduction.

Line 24-25: the control is not necessarily just elevational. It might depend also, for example, on the width and nature of the reef between the reef margin and a shoreline which might itself be of varying 'resistance'.

We have amended these lines to reflect the additional factors. We do think the primary interaction in dissipating wave energy is that between the wave and the reef structure at the reef crest that induces wave breaking. Consequently, we have focussed on this primary control, which is water depth controlled and is determined by both reef growth and sea level position. The distance of the reef flat to island shoreline is also a factor in how the hazards may be expressed on an island. We have split this sentence into two parts. The first deals with the primary interaction of ocean waves with the reef edge that induces breaking. The second deals with subsequent further dissipation and transformation across the reef flat which is dependent on relative water depth and reef width. In this section we do reference two articles (1 and 3) that cover a number of other factors that influence coastal hazards and not just elevation.

Line 27: 'cumulative vertical growth' suggests an uninterrupted, linear process. No timescale is given. But many reef surfaces have complex histories of changing level, not independent of timescale (a reef may have tracked sea level rise during the Holocene but at the sub-millennial scale have experienced periods of stasis or even elevation loss).

We have amended the sentence to better outline the reef construction process and address the timescale issue.

We also consider that we are looking at societally relevant time-scales based on current ecological composition. This is important because sea-level rise is happening now and will impact coastal communities in the near future.

Line 30: it would be helpful to have an Indian Ocean inset map for location. I think it has to be made clear in the Figure legend that this is a typical cross-shore profile for an Indo-Pacific atoll. The fore-reef slope would be quite different on Caribbean reefs and where modification of incoming waves takes place well before the reef crest (see HH Roberts) and the term 'reef flat' would only apply in perhaps one or two locations (see Glynn(1973) and Milliman (1973)

We provide a detailed location map of the Maldives, a cross-shore profile, as well as high-resolution drone imagery of the reef rim with the location of the plots superimposed in Supplementary Figure 1 A and B. While we recognize that the 'typical' reef crests and reef flats differ between Indo-Pacific and Caribbean reefs, the fore reef slopes are less different as is demonstrated in reference 4. We have made it explicit that the morphology is typical of Indo-Pacific reef flats.

Line 40: where did Holocene reef growth commence at 10K? Surely in most locations commencement would be more like 8 ka BP.

It was not our intention to suggest that reef growth commenced precisely at 10ka. However, to reduce any ambiguity we have replaced 'Over' with 'During' and deleted 'period'.

Line 46: or where changes in storminess (changes in frequency, magnitude or extension into currently storm-free areas with ocean warming) take out growth increments. Less than catastrophic mass coral bleaching may not kill corals but it will lead to a period of zero vertical growth. Meantime the bioeroders will keep bioeroding...

We have amended the sentence to also account for the role of physical and biological erosion mechanisms. We have given a specific reference here and added an additional reference that reports on structural loss of reef.

We also note that our study was taken after a period of mass coral bleaching across the archipelago. Additionally, we show that secondary carbonate production by crustose-coralline algae (CCA) can also maintain and even lead to vertical accretion of the reef crest within the critical wave breaking zone. This type of carbonate production appears more resilient to both storm and heat stress given the encrusting nature of CCA. These CCA dominated areas in the Indo-Pacific also tend to be naturally devoid of coral growth given the environmental constraints to coral growth (e.g. high hydrodynamic stress) under normal conditions.

Line 62: repetition from lines 40-41. Not required twice.

To minimise repetition, we have deleted the second part of the sentence that refers to rates. The critical point in this section of text is not the rates (which were repetitive in the text) but the reference to the technique used, that is 'geological reconstructions from reef cores'. This is not covered in lines 40-41.

Line 67-70: not entirely true. What about the application of the 'alkalinity depletion' method by Steve Smith and Don Kinsey, at a range of scales, up to and including the 'whole reef system'?

Smith and Kinsey's work is detailed later when we move from reef slope measurements to the more critical reef crest and reef flat zones relevant to this study.

It is accepted that the hydrochemical method referred to by the reviewer (also known as the total alkalinity method) provides direct measurements of net carbonate production. However, net carbonate production rates are distinct from reef accretion rates (or direct measures of changes in reef elevation). Census-based studies that have estimated net carbonate production rates were intentionally singled out by the authors because these studies commonly use estimates of net carbonate production to predict reef accretion potential. To date, hydrochemical studies have not undertaken this additional step. Our intent with this sentence was to demonstrate that direct measurements of changes in reef elevation are rare, not to summarise the methods available for measuring net calcium carbonate production within reef habitats.

Line 70-72: there is a fundamental methodological issue here of comparing point-in-time census measurements with rates of sea level rise assessed on, at best, decadal timescales and where the sea level rise record may be at best regional and not site-specific to the site of the census.

We do not disagree with this comment. However, comparisons are constrained by the resolution and scale of sea level records available, which is beyond the scope of our study to improve. Examining how contemporary reefs are responding to changes in environmental properties should be based on high spatial resolution datasets, which we do present. The supplementary information (Supplementary Figure 3) does show the actual sea level record for the southern Maldives.

Line 72-73: I thought that one of the findings of the Smith and Kinsey research was that there were not regional variations...

The references in this paragraph deal with data from reef slopes at various locations and refers to the results from carbonate budget studies that are converted to reef growth models. As reference 4 demonstrates there are significant regional variations based on the methodology used in these recent studies. No change was made to the manuscript.

Line 85: 'lesser importance'. that may be true in the Indo-Pacific but it might not be the case elsewhere.

The 'lesser importance' refers to the comparative difference between reef slope data and that of the reef crest and reef flat which are 'more important' for at-shore wave activity and erosion potential. See also Figure 1. We have amended the sentence to highlight this is most relevant in the case of Indo-Pacific reefs.

Line 88: only partially true in the Caribbean. Here the reef crest typically does contain corals, in encrusting and bladed forms, as well as coralline algae.

We have amended this sentence to align the differences more closely to Indo-Pacific reefs.

Line 93: in the absence of a reef flat, wave re-formation can take place (although the energy distribution is quite different from a typical incident trade wind generated wave (see Roberts))

We agree that in the absence of a reef flat a more complete surf-zone may evolve enabling wave reformation. However, in this study we are dealing with reefs that have distinct reef crests and reef flats. Consequently, we have reinforced this reef type in the amended sentence.

Line 104: it seems odd to only bring in Smith and Kinsey here...

The reasons for including these classic references here (34-36) is that they relate to the reef crest and reef flat rather than the outer reef slope or drop-off.

Line 106-116: I think it is important to point out that these are very local measurements – in the context of the scale of the reef margin, point measurements only. And I think it should be said here that they are from a short time period also.

We have amended these lines to better reflect the spatial and temporal dimensions of the analysis. We believe these two points are very evident and we have not attempted to disguise them. The measurements are high resolution in both space and time. We are not aware of another study that has 3 annually repeated measures of changes in reef surface topography. We also later address the fact that measurements were from localised sites, as are most carbonate budget studies.

Line 122, 127: are these differences statistically significant?

We thank the reviewer for raising this point. Yes, the rates identified are statistically significant. We have now added a statistical analysis to the manuscript including a section in the methods and a results table in the supplementary information (Supplementary Table 3). The sentence has also been amended to indicate that differences are statistically significant.

Line 161: were environmental conditions over this two year period typical of the site in the longer-term? The manuscript gives no information on the environmental setting of this reef. Does it undergo strong seasonal shifts in hydrodynamic forcing as a result of the Asian monsoon? The measurements were made in February 2018, 2019 and 2020. What are conditions typically like in this month at the field site?

A detailed description of the ecological and hydrodynamic characteristics of the site, in relation to its regional context, is included in the Supplementary Information.

Line 349 Is the site affected by longer-period water level oscillations associated with the Indian Ocean Dipole?

Discussion of oceanographic processes, including interannual variations is presented in the Supplementary Information. Interannual variations in water level are apparent, forced by such processes as the Indian Ocean Dipole.

Line 167: The reefs of St Croix are quite different in terms of their general ecology and topographic characteristics, the latter in part due to a fundamental different sea level history over the last 6,000 years. This is not a meaningful comparison.

The authors consider the comparison is useful because the accretion rates referred to at St Croix reflect studies undertaken on experimental surfaces on reef crest and reef flat environments. These were glass and plastic plates placed in these environments and were not the natural reef surfaces and biota. Therefore, we consider the results provide useful comparisons for annual rates of reef accretion. They are not influenced by the reef's geological history, which the reviewer refers to as diminishing the value of the example. In addition, we have added a comparison to other Indo-Pacific growth reefs based on geological evidence (Montaggioni, 2005).

Line 172-173: it is odd that the reader is not provided with any general information on typical cover percentages in the quadrats for different groups of organisms but only at-a-point data when this coincides with a transect pin. Just how much 'bare space' is typical? The detailed data reported here only have real value if they are seen in a wider, entire reef margin context.

Details of the reef flat ecology and benthic organisms are presented in depth in the Supplementary Information. We agree this information is essential to the study and hence included it in the Supplementary Information.

Line 182-183: this is interesting. But it is difficult to interpret further without information of those levels of rugosity and coral cover.

Levels of benthic cover, coral genera, coral growth form, living and non-living cover and rugosity are detailed on pages 5 to 7 and Tables 1 and 2 in the Supplementary Information.

Line 185: was there a well documented storm event/s in the study period? Or is this pure speculation?

This not pure speculation. However, this statement refers to the normal range, and variation, of wave breaking processes that impacts the reef crest across the measurement period and may for example, result from gravel clast impact on the reef surface. The comment was not referring to major storm events, of which none occurred during the experimental period. As the field site is close to the equator, major storm events are not common. Further details on the wave climate and storm processes are presented in the Supplementary Information section on Oceanographic Characteristics.

Line 188: but these echinoids were lagoonal?

In terms of the study results echinoids in burrows/cryptic spaces were common around CRAF sites 1 and 2. The reference cited (Bak, 1990) did survey densities and erosion rates of echinoid populations in 2 reef lagoons in the Indo-Pacific region (northwest Moorea and Takapoto atoll, Tuamotu Islands). In the absence of any site-specific or reef zone-specific estimates, we think it useful to cite this reference that shows the potential of urchins as bioeroding agents on hard reef structures, and is located within the Indo-Pacific.

Line 210-211: so are these comparisons really meaningful?

We believe they are meaningful. Previous estimates generally refer to the fore reef slope not the reef crest or reef flat. The comparison is considered meaningful because there is a paucity of research on analogous shallow reef habitats that are subject to energetic wave conditions. Consequently, the comparison to sheltered fore-reef habitats provides a benchmark to compare the performance of a relatively unstudied habitat. We also believe it is instructive to compare our data with available datasets and we think these are the closest comparators.

Line 217 – 229: these points are valid and well argued but in the longer-term the challenge is to come up with estimates of reef performance at the 'whole reef' scale because this is the scale that affects island lives and livelihoods. There is every likelihood that controls change as time and space scales change.

We agree that the long-term challenge is to develop estimates of broader reef performance. Such scale issues also challenge recent reef budget studies. We also note that the focus of this study is on reef rim and reef flat growth performance in the context of wave interactions and coastal hazards. Drone and ecological surveys (presented in the supplementary information) do suggest consistencies in eco-geomorphic zones examined across the atoll rim. We believe that the paragraph is appropriate and does not require any change notwithstanding the scale issue raised by the Reviewer.

Line 242: in a) are these global sea level rise scenarios rather than regional ones? If so, that point should be made clear. In b) the green / gray / yellow shadings are not explained. Again, it should be made clear that this is an Indo-Pacific reef setting.

Yes, in a) the sea-level rise scenarios are global rates. We have clarified this in the figure caption.

In b) we have added these colours to the legend to ensure these geomorphic zones are clarified. Green signifies reef structure, gray denotes conglomerate outcrop, and yellow is the island sedimentary materials.

We have also amended the caption to reinforce the changes are applicable to Indo-Pacific reef settings.

Lin 262-263: so you are arguing from a two year study to a 'multi-decadal transition'? Ok – but I think that you need to add some caveats at this point.

Agreed, we have amended text to tighten wording and make clear that this prediction is based on the growth patterns observed between 2018-2020 persisting under future environmental conditions.

We would note that such extrapolations of reef growth are regularly undertaken based on single ecological surveys (observations that span a shorter timeframe than our study). Such studies tend to make wide assertions about future global reef growth performance. It is also

worth noting these studies typically do not measure reef growth, but ecological cover, so arguably are more tenuous in projecting contemporary or future reef growth.

We consider our data, that measures actual vertical change in the reef crest – reef flat surface, provides a more robust basis to consider how this might propagate into changes in reef growth and structure over the coming decades.

We do recognise that we still do have a relatively short period of monitoring data. Consequently, we have added a statement that acknowledges this point. In lines 268-278 (old) we present a list of further caveats on our projections. Lastly, in the concluding sections of the manuscript we also advocate to maintain such monitoring over the medium-term, to accumulate longer datasets, and expand to different reef environments.

Line 269: addition of boulders. Is this likely? Has this happened in the past? Or is this speculation?

We consider this probable over decadal/centennial timeframes. Field surveys have identified a distinctive boulder tract on the outer reef surface, which is identified, mapped and shown in plan and profile in Supplementary Figure 1D and 1E. The location of the boulder tract is also shown in Figure 4B.

Line 277-278: more detail of the bleaching event please. Did this event compromise the results from study years reported here? If not, why not?

We have added additional context to this statement.

We did discuss the bleaching event and its reported impact on reefs (forereefs) (old lines 217-229) and reflected on those findings in the context of our study, which the Reviewer suggested was well argued (see above). Further details on the bleaching event are outlined in references 4, 31 and 57.

Line 279-289: the degree to which site measurements can inform 'whole reef' future trajectories surely is still debateable. There must be scale effects.

We agree and have amended this passage to reflect this idea in expansion of future studies that attempt to scale-up site-specific measurements.

Line 301: how were the 4 sites chosen on such a large diameter reef margin?

Site selection is representative of the seaward reef flat around the atoll as outlined in the Supplementary Information 2: Field Setting. Sites were randomly located in distinct eco-geomorphic zones across the outer reef flat as determined by topographic surveys, and ecological/coral community structure surveys.

Line 306: does this mean 5 m behind the spur and groove? Does the presence of spur and groove affect the patterns of flow and wave breaking at the 4 sites?

Local wave data (H max, H sig) from three sites (immediately offshore, reef crest and reef flat) for the field season are given in Supplementary Figure 4. Due to the turbulent nature of

wave breaking it is difficult to determine short spatial variability on the reef crest. However, homogeneity in ecological cover and topography would suggest the process regime along the reef crest is consistent.

Line 328: it is very good to see a proper estimation of error!

Thank you.

Line 349: I think the methodology here needs some support from referencing.

The CRAF measurements are original and the formula is unexceptional. The density value of 1.576 kg m³ falls midway between range values used in the ReefBudget Methodology and those identified from calcification studies in the Maldives. References have been added to the text to support this.

Reviewer #2 (Remarks to the Author):

Review of "Coral reef growth in the critical wave breaking zone can retain the wave protection function of reefs under moderate sea-level rise" manuscript by Kench et al.

Overall, the paper is well-written and the figures are of good quality. This study presents a new type of measurement of current reef crest accretion, which is greatly needed to understand the future of coral reefs in protecting shorelines and one of the only, if not the first, of its kind. The methodology is fantastic to make such measurements, and I believe these are the first of their kind and thus an extremely valuable contribution to the scientific literature.

We thank the reviewer for their recognition of the innovative and valuable dataset generated in this study, the importance of such data in understanding reef growth and the extremely valuable contribution to the scientific literature.

Stating that, the leap from measurements over 2 years of unknown forcing to decades of net reef crest accretion to suggesting that reef crests can keep up with projected sea-level rise seems to a step too far based on our understanding of net coral reef accretion. Lucien Montaggioni's (2005) Earth-Science Reviews paper and the references included therein note a few main points that put such an extrapolation into question:

Thank you very much for this comment. We address these comments in depth below but recognise the review comment is not critiquing the substantive data generated in the study, but how this data can be extrapolated and how the data can be interpreted in the context of inferences drawn from geological datasets, such as those of Montaggioni. We have amended the text to provide a greater context of the geological observations of reef growth (Specifically the Montaggioni review of Indo-Pacific reefs). Indeed, we argue that our observations provide the necessary bridge between short-term ecological inferences and longer-term geological interpretations from cores.

The focus of our study has been to introduce a new method to document physical changes in the reef surface. We note that researchers are using ecological data (often from single surveys) and converting this to reef growth estimates to make geological projections of reef growth (e.g. see Perry et al. 2018; Cornwall et al. 2021). The increasing number of such projections in top publishing outlets would suggest such extrapolations have become accepted. We argue that our data, which is a direct measure of actual reef surface change, is better quality record of geomorphic change in the reef surface, for the basis of making near-future projections. Furthermore, our dataset has more repeat surveys (with three annual surveys (yielding two annual increments) to date) than the majority of ecological driven studies. We believe that using such geomorphic datasets provides a sound basis to consider near-future changes in structure of the reef surface.

We acknowledge that our dataset represents net growth over two years and have added additional statements to the manuscript to highlight this. However, we strongly argue that consideration of current rates of change in the growth of the reef surface, from critical zones important to wave breaking (ie reef crest, reef flat) is a far better basis to make projections over the coming decades than those based on a census of living calcifiers on the forereef (generally at depths of 5m and below).

Our approach has been to align our direct measurements (geomorphic data) to correspond to these recent ecological-based studies for better comparison. We do compare current rates with the current known and projected rates of sea level change. We also state that if the current rates were to persist over the coming decades we might expect to see certain shifts in reef structure (our Fig. 4).

We would also stress that we are not projecting our data across geological timescales of thousands of years. We do find the data presents an opportunity to consider what reef growth may do over societally relevant timescales of decades. This timescale is of high interest to the reef science community and for improving coastal hazard analyses.

1) Net vertical accretion rates of framework are highly variable and not directly related to coral or CCA growth rates – in generally, net vertical accretion rates are generally one order of magnitude lower than coral or CCA growth rates. Thus the measurement of contemporary accumulation rates are likely lower, and maybe up to an order of magnitude lower, than net vertical accretion rates over decadal timescales.

We agree that vertical accretion rates of framework are variable. Indeed, our data show this between plots and between survey transects within plots (see Supplementary Figures 6-9). We make two further points of relevance to our data. First, our data does not infer reef growth derived solely from Coral or CCA, it is essentially a net measurement of change, and incorporates all of the physical and biological erosion and accretion process, including sediment infill and redistribution that results in net change. We discuss how closure and opening of fissures creates pore space and encases detrital sediment in a narrow temporal window. We consider this provides modern analogues for interpreting reef cores. We also recognise that our data cannot possibly represent all reef types examined in geological studies and have strengthened statements in the text to identify the results are most applicable to Indo-Pacific reef settings. However, the rates identified do sit within the published range of reef accretion rates based on geological core analysis.

In essence, we see our data as providing a bridge between short-term ecological and long-term geological analyses. The Review comment is also a strong argument to maintain the monitoring we are doing over longer timescales so we can observe how the reef surface continues to change over the coming decade(s).

2) Most records of coral framework accretion are composed of not in-situ growth, but rather broken corals and sediment (rubble down to silts) bound by CCA or recrystallization processes. That, along with the long-held intermediate disturbance hypothesis for coral reefs (Connell, 1978,

and updated by Rogers, 1993; Arnsen, 1995; etc) suggest that intermittent (decadal+ return interval) storms play a huge role in the development of coral reefs.

Many reef core records do show that in-situ growth is not common and CCA is a common binding agent of material. However, an examination of Montaggioni's (2005) data does show close to 50% of Indian ocean reef cores have framework facies. Our records are from the outer reef crest, a CCA dominated calcifying zone, which is an analogue of the 'Coralline algal framework facies' of Montaggioni. Where we do have live cover on our transects our data is documenting the role of CCA growth in the accretion process (also see comment above). We also consider that cementation of detrital sediments into the reef surface would be captured in our point measurements and believe we have identified and discussed this occurrence in Figure 3.

As the field site is close to the equator, major storm events are not common. However, we do appreciate that episodic events may contribute material to the reef surface that may be incorporated into the reef surface. Indeed, we identify a rubble tract that is likely storm deposited among our plots. Continued monitoring is necessary to capture the signature of such events in our monitoring record.

Lastly, we see an interesting extension of our study in which we intend to take shallow cores adjacent to our plot sites to examine and compare accretionary signatures. We intend to pursue this avenue after we have compiled another few years of monitoring data.

3) Numbers 1&2 suggest that contemporary measurements of coral and/or CCA growth demonstrated by the authors and presented in many of the references are not translatable to net vertical accretion rates, for that would neglect carbonate losses. For example, most of the references on contemporary assessment of carbonate production then make the assumption that all carbonate production gets incorporated into the framework, with no loss of carbonate from the reef, which we know occurs as offshore transport of sediment in grooves of spur-and-grove formations and net accumulation of carbonate sediment aprons at depths below active coral growth. Furthermore, it neglects the role of disturbance that can provide more material to a given location on the reef to help accumulation and/or remove that material from the reef, causing erosion. Thus the erosion values presented in Figure 3 and 4 are indicative of the measurements during the time period of unknown forcing (see below) and thus possibly (likely?) are underrepresented.

It is precisely for points 1 and 3 that we developed the CRAF system. Many ecological-based assessments do not appropriately or fully account for erosion and sediment recycling processes. Our measures of net reef surface change integrates all processes that have resulted in change in the elevation/topography of the reef surface (constructive and destructive). Where our individual measurement points coincide with live cover (e.g. CCA) we are able to comment and compare rates of vertical extension of those calcifiers. However, the majority of our plot data points do not coincide with living calcifiers. Consequently, it is not correct to indicate that our measurements are only of coral or CCA.

The dataset to this point has not been subject to a major physical disturbance, though we do discuss this caveat in the third-to-last paragraph. As with erosional processes, it is also possible that major physical disturbance could deposit material onto the reef surface and cause a step change in accretion. We also discuss this possibility. However, only continued monitoring will enable the data set to incorporate these future physical events in the record. We do discuss the measurements in the context of a recent severe bleaching event which impacted the region and added a number of relevant references.

We have amended the manuscript to make more explicit connection to the geological context, specifically the conclusions of the review by Montaggioni. In particular, we highlight that our reef crest is a modern analogue of the 'coralline algal' framework facies, typical of windward reef crests and flats, identified by Montaggioni from analysis of Indo-Pacific reef cores. Our accretion rates fit within the range of growth rates identified from the geological record for this facies type. We further highlight that the conditions conducive to this facies development was within a keep-up growth mode during the Holocene. We consider, the similarity in our growth rates to the geological record represented in Montaggioni's review further supports the validity of our projections that this zone may well keep pace with low to mid-range rates of sea level change.

The presentation of monthly average wave information in Supplementary Figure 3 and the comparison of that average data...which of course could be a 2 m average for the month of July, with the vast majority of the time being below 1 m and a very small, but geomorphologically important percentage of the time being above 5 m....to the 2 months of data in Supplementary Figure 4 is inconclusive. I would strongly suggest replotting Supplementary Figure 3 with the mean plus the 95% waves (H95) and then create a new Supplementary Figure 4 showing those same monthly statistical values for the 2-year study period. Such data would put the measurement period into the longer-term patterns and relative to disturbance events.

We thank the reviewer for this comment which requests additional site-specific environmental data. We have updated the wave information contained in a new ensemble Supplementary Figure 4, which contains the daily wave data throughout the monitoring period. Our intent in also presenting the shorter period data (Supplementary Figure 5) was to provide quantitative data on the role of this reef in filtering incident wave energy, based on a shorter several month deployment. We have also included an additional figure of the sea level behaviour over the two-year window of analysis which does show an elevated and variable pattern. However, we think it very difficult to assert any causal link between our results and this specific dataset.

4) Lastly, and as shown in Figure 4b, the projections of the algal reef crest accreting faster than then reef flat would cause a pattern of an elevated algal reef crest relative to the reef flat. If this process is projected to occur, why has there been no evidence of the same on the many reef crests in the western tropical Pacific where sea level has increased by 30 cm over the 1990-2010, per Merrifield (2011)? Furthermore, where is evidence of such processes and the resulting imbalance of reef crest to reef flat accretion in the Holocene?

We cannot address the query on western Pacific reefs. However, it is well known that there are many studies where the prominence of the algal rim has been described. However, few research groups are undertaking active topographic surveys of reef flats, reef crests and forereefs to quantify such changes at the scale of a few cm. Close inspection of a number of the figures in Montaggioni also indicates a significant imbalance between reef crest and reef flat growth during the Holocene.

Lastly, the ability of the reef crest to vertically accrete will be contingent on the productive cover, which is likely to vary between reef sites. Our data shows that where reef crest zones do have some productive cover, vertical accretion can occur.

Again, I think the measurements presented in the manuscript are sorely needed and a great advancement in our understand of short-term development of algal reef crests. Stating that, I do not feel the geologic record, our contemporary understanding of reef net accretion processes, and the limited forcing data support the extrapolation of the outstanding but limited contemporary data to decadal timescales the support the main point of the manuscript that algal reef crests might be able to keep up with projected sea-level rise.

We thank the Reviewer for this concluding statement, which highlights the value of the dataset, and the stimulating discussion concerning the geological implications. We believe we have articulated how the data provides an informative bridge between ecological (shorter-term) data and geological datasets in the above comments. We have amended the text to better reflect this. We also think the manuscript has articulated the caveats required in using the dataset to consider near-future change in reef structure, and believe it is the best data available to make such projections.

12th Nov 21

Dear Professor Kench,

Your manuscript titled "Coral reef growth in the critical wave breaking zone can retain the wave protection function of reefs under moderate sea-level rise" has now been seen by our reviewers, whose comments appear below. In light of their advice we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment, provided you tone down your conclusions to acknowledge the spatial and temporal limitations of your study and remove the extrapolation to the future. If you meet this condition, your manuscript will be published under the open access CC BY license (Creative Commons Attribution v4.0 International License).

We previously asked that you provide robust support for your projections and wider conclusions in light of the temporal and spatial limitations of the study or, alternatively, that you tone down, caveat or remove these conclusions. In light of the reviewer reports, we cannot conclude that you have met this request. In particular, for publication in Communications Earth & Environment to be appropriate, we will need you to remove your conclusions regarding the comparison of reef accretion rates and sea level rise and discuss the assumptions and limitations of your estimates. In addition, we allow unlimited space for Methods and so please move relevant material from your Supplementary files to the main text, as per Reviewer #1's earlier suggestion.

At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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Best regards,

Dr Clare Davis
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REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

I appreciate that the authors have made a strong effort to address the concerns raised by the two through referees' reports. Some matters simply cannot be resolved because of the nature of the comparative data available to the authors. I appreciate the much better focussing on Indo-Pacific comparators. But perhaps too many responses rely on saying 'the answer is in the Supplementary Material'. I really do think that the 2016 bleaching event has to be mentioned in the main text.

Reviewer #2 (Remarks to the Author):

The authors have made a number of edits to the manuscript to address a number of my concerns with the extrapolation of the extremely high quality but limited duration (2 years) data to long-terms (decadal- to century-long) accretion rates. I still believe such extrapolation of the results need to be couched a bit more in the Abstract and Discussion as not to mislead readers. My suggested modifications are as follows:

In the Abstract on Line 17, I think the sentence, "Results indicate...." should be changed to, "If these short-term results are maintained over decades, they indicate..."

In the Discussion around Line 300, I think there needs to be some acknowledgement that these measurements were during "normal" conditions as noted in the Supplemental Figures 4-5 presenting wave data but do not include infrequent storm events the authors note do occur in citing Supplemental Reference #13 (Wadey et al., 2017, Natural Hazards). Such infrequent events not measured during the limited study period could cause net reef degradation, as Puotinen et al. (2020, Global Change Biology) noted that infrequent cyclones can damage coral reefs 1000s of kilometers away, causing degradation of the reef surface and thus meeting Connell's (1978) intermediate disturbance hypothesis of development punctuated by infrequent events. Because such infrequent degradation events do occur (Wadey et al., 2017) at the field site and could cause degradation (Puotinen et al., 2020) not measured during the study period, it would result in long-term projections based on these short-term measurements skewed to higher accretion values than if the (negative) impact of the infrequent events not measured were incorporated into the

projections.

Again, this does nothing to detract from the wonderful, high-quality measurements that are sorely lacking and provide critical new insight into the role of coral reefs in coastal protection of tropical islands communities now and into the future. I just think it is necessary to make it clear that the extrapolation of these short-term measurements to long-term future projections make some assumptions that are questionable.

Another note: Because the authors note in some of their data and in the geologic records that some, if not most, of reef development is through the binding of coral rubble by calcifiers, I would suggest the use of the term, reef "accretion" instead reef "growth".

I am excited to see this manuscript published, as it will be a great contribution to not only the field in terms of a new methods, but some of the first, high-quality results giving the larger community insight into the future of coral reefs and the communities that depend on them for critical ecosystem services.

Response to Reviews

We thank the Reviewers for their second set of comments on our manuscript. We are pleased to read the positive comments and have outlined our responses to each comment in detail below. We also respond to the Editor comments below. We have retained review comments (in black text) and our responses are in blue text.

Editor Comments

Your manuscript titled "Coral reef growth in the critical wave breaking zone can retain the wave protection function of reefs under moderate sea-level rise" has now been seen by our reviewers, whose comments appear below. In light of their advice we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment, provided you tone down your conclusions to acknowledge the spatial and temporal limitations of your study and remove the extrapolation to the future.

We have removed the future extrapolations of reef growth from the ending of the manuscript including deletion of Figure 4 panel b. We have amended the abstract in alignment with Reviewer #2's suggestion. We have also substantively increased a section on data limitations following the suggestion of Reviewer 2.

In addition, we allow unlimited space for Methods and so please move relevant material from your Supplementary files to the main text, as per Reviewer #1's earlier suggestion.

We have restructured the Methods and moved relevant material from the Supplementary files to the main text (Methods). This has removed a substantive amount of text from the Supplementary files.

At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

We believe we have edited our manuscript to comply with the format requirements.

Reviewer #1 (Remarks to the Author):

I appreciate that the authors have made a strong effort to address the concerns raised by the two through referees' reports. Some matters simply cannot be resolved because of the nature of the comparative data available to the authors. I appreciate the much better focussing on Indo-Pacific comparators. But perhaps too many responses rely on saying 'the answer is in the Supplementary Material'. I really do think that the 2016 bleaching event has to be mentioned in the main text.

We thank the Reviewer for their comments.

We may have misunderstood the initial reviewer's comments. Nowhere in the initial review did Reviewer 1 indicate they had read the Supplementary information (it was not referenced once in the initial review comments of Reviewer 1). The Reviewer posed a number of questions related to the field site that were contained in the supplementary information. Consequently, in our response we highlighted where this information was located.

In alignment with the Editor's advice we have moved a considerable amount of material from the Supplementary information to the main text, particularly the Methods. We note that the 2016 bleaching event was explicitly referenced in the main text in lines 234-243. We have also included reference to the event at the end of the introductory section.

Reviewer #2 (Remarks to the Author):

The authors have made a number of edits to the manuscript to address a number of my concerns with the extrapolation of the extremely high quality but limited duration (2 years) data to long-terms (decadal- to century-long) accretion rates. I still believe such extrapolation of the results need to be couched a bit more in the Abstract and Discussion as not to mislead readers. My suggested modifications are as follows:

In the Abstract on Line 17, I think the sentence, "Results indicate...." should be changed to, "If these short-term results are maintained over decades, they indicate..."

We thank the Reviewer for this suggestion, which we have implemented and also highlighted the spatial temporal constraints of the data. The sentence now begins, 'Recognising the spatial and temporal limits of this high quality dataset, if these short-term results are maintained over decades, they indicate...'

In the Discussion around Line 300, I think there needs to be some acknowledgement that these measurements were during "normal" conditions as noted in the Supplemental Figures 4-5 presenting wave data but do not include infrequent storm events the authors note do occur in citing Supplemental Reference #13 (Wadey et al., 2017, Natural Hazards). Such infrequent events not measured during the limited study period could cause net reef degradation, as Puotinen et al. (2020, Global Change Biology) noted that infrequent cyclones can damage coral reefs 1000s of kilometers away, causing degradation of the reef surface and thus meeting Connell's (1978) intermediate disturbance hypothesis of development punctuated by infrequent events. Because such infrequent degradation events do occur (Wadey et al., 2017) at the field site and could cause degradation (Puotinen et al., 2020) not measured during the study period, it would result in long-term projections based on these short-term measurements skewed to higher accretion values than if the (negative) impact of the infrequent events not measured were incorporated into the projections.

Again, this does nothing to detract from the wonderful, high-quality measurements that are sorely lacking and provide critical new insight into the role of coral reefs in coastal protection of tropical islands communities now and into the future. I just think it is necessary to make it clear that the extrapolation of these short-term measurements to long-term future projections make some assumptions that are questionable.

We have included a more explicit paragraph (lines 252-281) that outlines the limitations of our measurements and discuss how a number of processes may alter the accretion rates. This paragraph includes explicit consideration of storms and the possible impacts on the accretion rates.

Another note: Because the authors note in some of their data and in the geologic records that some, if not most, of reef development is through the binding of coral rubble by calcifiers, I would suggest the use of the term, reef "accretion" instead reef "growth".

Thank you. We do note that any process that contributes to vertical accretion of the reef surface adds to the growth of the reef. However, we have amended to reef accretion in appropriate places.

I am excited to see this manuscript published, as it will be a great contribution to not only the field in terms of a new methods, but some of the first, high-quality results giving the larger community insight into the future of coral reefs and the communities that depend on them for critical ecosystem services.

Thank you.