

Interactive comment on “Divergence of seafloor elevation and sea level rise in coral reef regions” by Kimberly K. Yates et al.

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AR: Many thanks to the reviewer for a very thoughtful and constructive review of our manuscript. We believe that we will be able to greatly strengthen our manuscript and broaden its impact by re-writing the methods (as all reviewers have recommended) and by providing more detailed discussion and clarification in the areas addressed by Reviewer 3. We have addressed individual comments below (reviewer comments indicated by ‘R3’, author responses indicated by ‘AR’).

R3: Overall, I am impressed by this paper and think that it is an interesting attempt to elevate monitoring to something more than “counting corals”. However, I am concerned that the likely variability in sources of substrate change were probably much more different from site to site than has been characterized. I could be wrong, but I

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suspect that bioerosion is less of a factor than is represented here. . . and is more likely declining at most Caribbean sites. While there is an effort to address site-to-site variability, I am not convinced that the relative roles of simple bioerosion, large-scale rugosity loss and export by storms have been adequately considered. I would like to see this paper appear in print, if only for the valuable data set. However, I am concerned that the explanations of the measured patterns is a bit oversimplified and relies too much on the mechanisms proposed. I, therefore, provide some over-arching thoughts below in the hope that the authors can perhaps think a bit more about other possible explanations for the patterns they observed. Accordingly, I make a few general observations below that will hopefully be useful.

AR: We appreciate the reviewer recognizing the value of our work from the monitoring standpoint. Our primary intent for this paper was not to focus on a detailed analysis of the processes causing elevation change. Our intent was to focus on introducing this type of monitoring approach to the coral reef scientific community as the first application of seafloor elevation change methods to whole coral reef ecosystems, to demonstrate the value of this approach, and to report the substantial regional-scale net change in seafloor elevation that has occurred over the past several decades and has gone undocumented until now. The concept of our work is based on the premise that elevation change analysis measures the net result of all of the constructive and destructive processes on whole reef systems including those processes for which rates are known (e.g., carbonate production, biological erosion, chemical erosion, etc.), as well as those processes that haven't been well characterized (for example physical erosion and export). While the method does not attribute change to cause, it does provide a measure of the net result of all impacts (natural or anthropogenic) to seafloor structure in these ecosystems.

We agree that our explanations of the sources of substrate change are very general. Our results indicate that erosion has been largely underestimated in these regions. Our discussion of mechanisms of substrate change (page 14, line 30 through page 15, line

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15) was an exercise to demonstrate the magnitude of net erosion that we measured by elevation change (that accounts for all processes) relative to individual processes for which process rate estimates were available including bioerosion among others. Additionally, it was a demonstration of how these regional scale elevation change analyses might be used to help attribute change to cause by way of identifying and quantifying changes that have been unaccounted for by known process rates. We do see how that exercise can be mistaken as an attempt at a comprehensive process analysis. We can clarify our intent for this comparison both in the section that discusses processes on pages 14 and 15, and modify our statement in the abstract regarding the results of that comparison.

In our brief discussion of bioerosion (on page 15 beginning on line 1) we purposefully selected a maximum bioerosion rate from the literature for that discussion to show that, even assuming maximum rates, bioerosion alone cannot account for the elevation changes we observed. We believe the reviewer perceived that we attributed much of the erosion we observed to bioerosion because we applied a maximum bioerosion rate to our estimates in the exercise discussed in the above paragraph. In fact, we agree with the reviewer that bioerosion is likely less of a factor than physical erosion and transport. We attribute much of the volume loss we observed to evidence for physical erosion and export in a discussion on page 12, lines 25-28. We will clarify with additional references, bioerosion rates, and text that bioerosion rates are likely much less and, in some areas, likely to decrease where bioeroder communities are decreasing.

We had hoped to convey to the readers the value of knowing net whole system change in trying to account for and attribute that change to individual processes as well as identifying missing gaps in erosion/accretion budgets. As an analogy, it's easier to put the puzzle together (and to figure out which pieces are missing) if you know how the whole picture looks. Additionally, we recognize that predicting effects of coral reef ecosystem degradation on hazards to coastal communities in these regions (sea level

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rise, storms, tsunamis, etc.) has been largely limited due to a lack of whole system seafloor change analyses such as we performed for this study. Our results quantify substantial seafloor elevation loss in these regions and are (on their own) a significant finding that suggests risks from coastal hazards may be underestimated in these coral reef regions. These data sets provide a foundation for improving numerical models of present and future coastal hazards in these regions. We feel that a rigorous discussion of processes for each study site would be best addressed in individual manuscripts for each study site, and that a detailed process analysis of each study site is beyond the scope of our paper. We do, however, feel that our results will encourage and help improve future studies that develop comprehensive erosion budgets and account for process rates. We appreciate the very thoughtful and constructive comments from that reviewer that will help us add text to better explain the intent of our study, the need for these types of whole system change analyses, and to clarify that the point of our general assessment of processes was to demonstrate the magnitude of that change relative to change caused by a few individual processes for which rates are available, rather than a comprehensive process analysis. We will also expand our discussion to recognize the limitations of our examples, and add additional references and discussion to support that discussion.

R3: Comments:

Like Reviewer 1, I am not well versed in the GIS and data transformation methods utilized in this study. However, I am familiar with the vagaries of older hydrographic surveys. On the latter front, I am willing to accept their characterizations of (the direction of?) change in substrate level as the differences between sites are probably sufficient to overcome any stated errors. However, in my experience, the notes on smooth sheets leave us with a need to make defensible assumptions about a) the reliability of substrate characterization (and its stability) and b) how processes that potentially influence elevation change might differ from site to site. I have limited my comments to the latter, based on areas in the manuscript where I have experience in either the

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specific habitats or the processes that might contribute to the patterns described.

Before I start, I do have one comment on style. I am not qualified to comment on the statistics of the methods or the assumptions made in the GIS transformations and map algebra. Nevertheless, a more reader-friendly explanation on that front would make the paper more accessible to a broader audience. The paper in its present form is a wealth of information on methods for those inclined to apply them to other sites. However, those people are probably going to be less well informed on the evolution of carbonate substrates. Conversely, those with intimate understanding of carbonate cycling are going to be unable to tie their knowledge to the details of the methodology used here. I am in that latter group and would suggest that the minutiae of the transformations and GIS tools could be better placed in the Supplemental Materials.

AR: Thank you for pointing this out. We have learned through the review process that we need to rewrite the methods section so that a broader audience can more easily follow along. As indicated in our response to Reviewer 1, we have developed a flow diagram that we will also include that summarizes the methodological steps. We will also move the detailed GIS methods to a supplementary section. Again, our aim was to introduce this approach to the coral reef community, and we want them to be able to use it. We provided detailed methods to help others pursue this type of work. We have come to realize through our reviews that the cross-pollination of expertise in elevation change analysis and coral reef studies is new enough to both areas of expertise that we must be more rigorous in defining terminology and explaining procedures.

R3: The following are my general thoughts based on elements of carbonate cycling that could lead to conclusions other than those drawn here. While I am willing to accept the numerical changes in substrate elevation, I am somewhat less comfortable with assumptions about the degree to which they are related to bioerosion and the ensuing removal of sediment.

Biorosion versus structural reorganization – In the discussions, there is an apparent

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conflation of bioerosion and spatial heterogeneity. The paper by Alvarez-Fillip et al. (1990) that is cited to document the role of increased bioerosion focused on the loss in architectural complexity (aka rugosity) and its causes – not bioerosion. In the paper, they attributed the initial reduction in reef rugosity to the loss of acroporids and the second decline in rugosity to a loss of massive species following bleaching. It seems reasonable to assume that an increase in susceptible substrate could increase bioerosion. However, Alvarez-Fillip et al. focused on the loss of rugosity which, in the case of *A. palmata*, is more easily explained by physical toppling/breakage and incorporation of fragments into a broad, cemented pavement.

AR: The Alvarez-Filip et al. (1990) reference should have been listed earlier in the sentence to which the reviewer is referring. We thank the reviewer for catching this mistake. On page 1, line 30 through page 2, lines 1-3, the sentence reads:

“Local and global, natural and human-induced stressors have caused the loss of reef-building organisms and reef structure, a decrease in biodiversity, a transition to algal-dominated communities (Pandolfi et al., 2003), and an increase of bioerosion (Alvarez-Filip et al., 2009), placing coral reefs around the world in a state of rapid decline (Madin and Madin, 2015).”

The Alvarez-Filip reference should have been placed after the statement “. . .have caused the loss of reef-building organisms and reef structure. . .”. The reference that was intended to follow the statement “. . .and an increase in bioerosion. . .” should have been Enochs et al. (2015), see reference list in manuscript. The Enochs reference discusses experimental and modeling results showing that ocean acidification increases bioerosion of coral by the boring sponge *Pione lampa*. We will correct this mistake and clarify in this sentence that we are referring specifically to some species of boring and endolithic bioeroders. We will also include the following references as other examples:

Wisshak M, Schönberg CHL, Form A, Freiwald A. 2013. Effects of ocean acidification and global warming on reef bioerosion – lessons from a clonoid sponge. *Aquat Biol.*

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19(2):111–127. <http://dx.doi.org/10.3354/ab00527>

Wisshak M, Schönberg CHL, Form A, Freiwald A. 2012. Ocean acidification accelerates reef bioerosion. *PLoS ONE*. 7:e45124. <http://dx.doi.org/10.1371/journal.pone.0045124>

Tribollet A, Godinot C, Atkinson M, Langdon C. 2009. Effects of elevated pCO₂ on dissolution of coral carbonates by microbial euendoliths. *Global Biogeochem Cycles*. 23(3):1–7. <http://dx.doi.org/10.1029/2008GB003286>

Reyes-Nivia C, Diaz-Pulido G, Kline D, Hoegh-Guldberg O, Dove S. 2013. Ocean acidification and warming scenarios increase microbioerosion of coral skeletons. *Glob Change Biol*. 19(6):1919–1929. <http://dx.doi.org/10.1111/gcb.12158>

DeCarlo T., Cohen, A., Barkley, H., Cobban, Q., Young, C., Shamberger, K., Brainard, R., and Golbuu, Y. 2015. Coral macrobioerosion is accelerated by ocean acidification and nutrient. *Geology* 43, 7-10, doi:10.1130/G36147.1

R3: The interval of measured elevation changes included the loss of *A. palmata*. It, therefore, seems likely that this could have played a greater role than the removal of bioeroded sediment in the changes described in the manuscript. Alvarez et al also pointed out that the loss of *Diadema* logically reduced bioerosion despite the greater availability of “bioerodable substrate”. Likewise, in many (most?) Caribbean and western Atlantic sites, parrotfish populations have been decimated, further reducing the potential for bioerosion by grazers. The remaining option is infaunal bioerosion by sponges, worms, etc. However, unless there is a very significant increase in organic availability, the likelihood of that being significant seems unlikely.

AR: We agree that loss of *A. palmata* and other framework building species likely contributed to elevation loss at these study sites, and we address this issue in section 3.2 beginning on page 12. To estimate the contribution of this process to total net volume loss, we calculated the percent of each study area classified as coral-dominated

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substrate (note coral-dominated habitat classes were denoted in Tables 2 and 3 in manuscript with an asterisk). For example, the Upper Florida Keys coral-dominated habitat classes included scattered coral rock in unconsolidated sediment, aggregate reef, reef rubble, individual or aggregate patch reefs, and spur and groove habitat. The other study sites included similar lists of coral-dominated habitats. We included reef rubble as a coral-dominated substrate in our calculations because, for example, in the Florida Keys many areas of reef rubble contain large skeletal fragments of *A. palmata* and other species. We calculated that 91% of the Buck Island study area was covered by coral-dominated substrate types that accounted for more than 90% of the total net volume loss at this site. We stated that loss of framework building coral at this site may be the primary contributor to volume loss (page 12, 28-30), but we do not attribute loss of the framework building corals to bioerosion or any other process. We suspect that physical toppling of coral colonies from (e.g., from storm and wave impacts) likely contributes here as well, but do not have adequate data to quantify that (see additional discussion on this topic in comments later in this review response).

However, the areal extent of coral-dominated substrate in the Upper and Lower Florida Keys and St. Thomas was only 8% to 15% of the total study area. These coral-dominated habitats contributed up to only 26% of the total net volume loss that we measured for these study sites. Most of the volume loss in these areas was associated with sediment loss from non-coral-dominated habitats such as areas of unconsolidated sediments, seagrass, and uncolonized pavement (Table 3 in manuscript). The Maui study site was characterized by 57% coral-dominated substrate that accounted for 50% of the net volume loss. We do suggest that physical erosion and removal of sediment is a likely driver of much of the volume loss at these study sites rather than degradation of large framework building coral because the total areal extent of coral-dominated substrate cannot account for the loss, and most of the volume was lost from non-coral dominated substrate types.

R3: It is interesting that at one of their sites (Buck Island), Bill Gladfelter proposed two

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threats to reef building in a 1977 report to the Park Service: 1) the loss of carbonate production if WBD increased, and 2) the possibility that protection of parrotfish might significantly increase bioerosion to the point where it could overwhelm even productive reefs. This would suggest that increased bioerosion by grazing fish could lead to detrimental increase in bioerosion. In the latter scenario, increased grazing becomes a problem only in protected areas where grazing fish have increased (like the FKMS, one of the described sites where increased bioerosion might be a reasonable culprit). Elsewhere in the Caribbean, parrotfish populations have been decimated. In combination with the loss of the major grazing urchin, a wholesale increase in bioerosion capacity seems unlikely. Lost calcification ability would decrease accretion, but does not seem like a driver of net erosion unless bioerosion increases – a pattern that has not been documented at all sites.

AR: We agree that losses and gains of grazing fish will directly impact rates of bioerosion by those species. We realize we need to clarify in our discussion that there are a number of different types of bioeroder species including grazing fish as well as sponges, bivalves, microendoliths, etc. We will note the potential impact of increases or decreases in grazing fish on rates of bioerosion; and we will clarify that recent work, cited earlier in our response, indicates that elevated pCO₂ and ocean acidification (and especially OA combined with increased nutrients) accelerates bioerosion by endoliths and other boring bioeroders.

R3: So, that leaves us with export. As the paper points out, good data on export are rare. On page 2, Moses (2009) is cited for measuring sediment export from reefs, but I could find no measurements in that paper. Kench and McLean provide an estimate of transport potential through hoas in Indian Ocean atolls. However, the results are based on theoretical calculations and there is no effort to tie sediment to specific sources (e.g., bioeroded sediment, beaches, lagoons) or sinks (loss to lagoons vs export from the platform).

AR: The reviewer is mistaken regarding the Moses (2009) reference. Moses (2009)

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is cited with respect to regional-scale chemical erosion measurements of carbonates (page 2, lines 17-20). In a sentence after that (beginning on line 21), Morgan and Kench (2014) and Kench and McLean (2004) are cited in a sentence that reads “Very few studies have quantified sediment transport and export on reef systems”. We note here that many of our references in the paper have typos in them from our End Note conversion that we did not catch prior to submission. For example, many were abbreviated (e.g. Morgan and Kench 2014 got shortened to Morgan 2014, and Kench and McLean 2004 got shortened to Kench 2004), and others reverted back to numbers (as was the case in Supplementary Table 1 in which the source references reverted back to numbers). All of these will be corrected in a revised paper, and we apologize for the confusion this may have caused.

Morgan and Kench (2014) is one of the few studies that estimated sediment flux and off-reef sediment export from direct point measurements using arrays of bi-directional sediment traps for an atoll reef (Vabbinfaru reef, North Male’ Atoll) in the Maldives. They showed high off-reef export rates for both gravel and sand (annual export estimated at over 120 metric tons per year), as well as high sediment flux rates even during non-storm conditions.

Kench and McLean (2004) estimated transport potential from current meter and tidal gauges located in the HOAs, but also measured sediment fluxes in HOAs from sediment traps ranging from approximately 2kg to 268 kg per day. Based on daily rates of sediment flux, they estimate that from 44 to 223 metric tons per year of sediment may be transported by HOAs.

We recognize that there are other previous studies (e.g., work by Ogston, Presto, Storlazzi, Fletcher, Hubbard) on sediment transport on Caribbean and Hawaiian reefs that can be included, and will add these along with more detailed discussion of sediment transport and export to a revised manuscript. We note that the reviewer performed very detailed studies of sediment transport and export at St. Croix in the U.S. Virgin Islands including assessment of the physical and biological processes affecting sedi-

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ment transport across reef zones (Hubbard et al. 1981), the impact of storms on sediment transport and export (Hubbard 1992), detailed carbonate budgets that account for export (Hubbard et al. 1990), and the effect of sedimentation on reef development (Hubbard 1986). Many of these studies suggest that physical processes (waves and storms) dominate with respect to transport and export of sediments from this reef system. Results from these (and other studies) provide evidence that very large amounts of sediment are transported within reef systems and exported from reef systems by physical transport of materials. These results support our suggestion that physical erosion and export of sediments could account for much of the volume loss we observed at our study sites (page 12, line 25-28).

R3: What is, therefore, critically important is a reliable estimate of export inas- much as volume must be exported from the system to trigger system-wide elevation loss. . . bioerosion just converts carbonate from solid substrate to sediment. In the latter case, we must remember that sediment has a much lower bulk density than solid carbonate substrate. Thus, increased bioerosion without export would reduce the volume of solid substrate but would turn this into a sediment pile with something akin to twice the net volume. Thus, increased bioerosion without export would result in substrate elevation; not lowering. A scenario based solely on increased bioerosion seems inadequate to explain the measured patterns.

AR: We agree with the reviewer, and make the case in our results and discussion that the volume loss we observe over the large scale of our study sites is an indication of export of materials from these systems. We do not suggest that any scenarios based solely on increased bioerosion could account for the volume losses we observed. We will reword the sentence on page 15 line 3-4 regarding bioerosion to clarify that export of that amount of bioeroded sediment per year could only account for as much as. . .” as we suspect the reviewer may have misinterpreted our meaning in this sentence. The point we are trying to make here is that much more sediment is being physically eroded and exported from these systems than the amount that is annually generated by

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bioerosion alone and exported. This is likely why we are seeing a broad-scale decrease in sea floor elevation over all habitat types rather than stability or accumulation. We will reword our discussion regarding physical erosion to more directly convey that message, and include additional references (as discussed above) to support our observations.

Note, we specifically do not convert our volume measurements to mass of carbonate because we recognize that the volume change we measure encompasses loss of sediments as well as framework building coral colonies of different porosities. We recognize the challenges in estimating the conversion of volume of framework building coral colonies (including the coral itself as well as the pore spaces in the colony framework) to volume of bioeroded sediment of potentially varying porosities. . .and these exercises are worthy of papers themselves.

R3: Unfortunately, there has only been a few careful measurements of sediment export in the context of a reef-wide budget. Perry and various co-authors use our ratio (Export ÷ Lij 50% of total bioerosion) from the north coast of St. Croix to characterize this in every one of their budgets. It is naïve to think that all reefs in all oceans have the same energy regime (the driver of export) – or that changes in energy regime is offset by proportional shifts in bioerosion to maintain the 50% value that is used throughout. With increasing storminess, sediment export looms as the single largest unquantified variable. Therefore, export can only get more significant in the budgeting attempted in this paper.

AR: Again, we agree with the reviewer. Note in our discussion that we suggest that much of the volume loss we observe in these regions may be attributed to physical transport and export of sediments, and that export has been largely unaccounted for in previous studies (page 15, lines 10-12). We also note that the much greater seafloor elevation and volume losses observed over shorter time periods at the Maui study site could be caused by higher sediment export rates due to a combination of higher wave energy and physical erosion as well as a narrow shelf surrounding the island (page 14, lines 1-3). We feel that ecosystem-scale elevation change studies such as those

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we performed will be instrumental in better quantifying large-scale sediment transport and export by way of accounting for total volume change in sediment budgets. These types of data sets will also assist with mapping elevation change patterns that suggest offshore movement of sediment such as the example we provide in our comment to Reviewer 1 showing movement and accumulation of sediment offshore and downslope along the Florida reef tract.

R3: In section 3.2, the paper acknowledges the difference between bioerosion and changes in structural complexity. How good the conclusion will be is going to depend on how well one can distinguish between the two as potential drivers of elevation change. The conclusions presented here seem to suggest 1) an ability to reliably distinguish between the two mechanisms and 2) an overwhelming importance of simple bioerosion over combined changes in export and reduced structural complexity following the loss of biological constructors.

AR: We are confused by this comment. We do not discuss bioerosion anywhere in section 3.2. We do discuss that greatest mean elevation losses in the Florida Keys were associated with shallow-coral dominated substrate and that this observation is consistent with observations of general flattening of reef topography and decreasing abundance of reef-building corals in previous studies. We make no claims as to the cause of the elevation losses in the coral-dominated habitats, nor do we claim to be able to distinguish between bioerosion and loss of structural complexity. Additionally, we claim that physical erosion and export is the likely driver of much of the volume loss that we observe. We do suggest that, at the Buck Island study site, much of the volume loss may be due to degradation of framework building corals because 91% of the study site was characterized by coral-dominated substrate. We do not attribute degradation of framework building corals to any particular cause in this discussion. We do, however, note that Buck Island showed the lowest net volume losses of all of our sites suggesting that much less sediment has been exported.

The main conclusions of our work focus on the large magnitude of elevation and volume

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loss from these study sites that has been previously undocumented, how that has impacted local sea level rise, and the observations that most substrate types within these ecosystems are losing elevation and volume, not only coral-dominated substrate. As previously stated, we compare our measured rates of net erosion to rates for individual processes that cause erosion (including bioerosion, chemical dissolution and sediment export) in a paragraph in our discussion. We feel, from the reviewers comments, that the intent of that exercise has been misunderstood. We will reword this section to clarify our intent to demonstrate the magnitude of erosion we observed relative to known rates for individual erosion processes.

R3: Anthropogenic drivers of change - Using population as a proxy for anthropogenic impact seems overly simplified. Numerous recent papers have shown that some of the greatest reef losses occur due to warming/acidification at great distances from any recognizable urban stressors. I can't find the specific papers, but there has been quite a bit of discussion on the NOAA listserv about papers that show just this. While I am not in the midst of the debate over local versus global drivers of change and their implications for management, this proxy seems a bit simplistic.

AR: We define relative anthropogenic impact within each study site based on the simplest first order parameter of population and note that the historic data sets for each site were from time periods characterized by approximately half or less the population than the date of the modern data set. We believe the reviewers would agree that local anthropogenic impacts have increased over the historic to modern time periods of each data set for each of our study sites where population has doubled. We agree that this is a very simplified proxy, and state that full analysis of anthropogenic impact factors is beyond the scope of the paper (see page 3, lines 14-21). We could, reluctantly, remove reference to anthropogenic impact and simply state that population has doubled at each of the study sites (except for Buck Island) over the historic to modern time periods. But suspect that others would then criticize that we don't recognize that there has been anthropogenic impact. Our preference is to leave this as-is.

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We do not attempt to attribute reef ecosystem degradation to either local anthropogenic or global climate impacts, and we don't disagree that reef losses due to global stressors have occurred in geographically isolated, low population areas (we can add some of these references). We do state that projections indicate that impacts from these stressors are likely to increase in the future. We cite Perry's (2015) work showing that reef systems in the Indian Ocean that are geographically isolated and lack human influence show heavy impact from bleaching events, but are able to recover very rapidly (see discussion on page 16, lines 1-4) and show very high accretion rates. We note that Maui is geographically isolated but shows very large erosion rates suggesting that it has not been able to recover from reef loss, possibly due to the fact that it is not isolated from a large human population. We also point out more than one case where lower losses of elevation and volume coincide with areas further removed from large population centers or associated with natural refuge zones (page 16, lines 4-6).

R3: On a more specific point, the manuscript discusses the idea of proximity to anthropogenic areas to explain the positive elevation change in the lower Keys. Couldn't this also be due to separation from the inimical cold bank water allowing for higher calcification rates? In this vein, limited core data from the Keys seem to suggest that the "demise" of the reef tract likely started 4-5,000 years ago as Florida Bay flooded, triggering inimical (cold) water export onto the reefs. In contrast, the reefs around Buck Island enjoyed continuous building throughout this period as there was no similar source of stress.

AR: The reviewer misunderstood this discussion located on page 13, lines 8 – 12:

"Mean total elevation loss was lowest at the UFK study site. However, mean elevation losses decreased from upper (-0.4 m) to central (-0.3 m) sub-regions of the UFK, and mean elevation increased slightly in the lower sub-region (0.1 m) primarily associated with seagrass habitat (Fig. 1a, b). Notably, the lower sub-region is further away from high-density population areas north of the study site and near an area of the middle Florida Keys identified as a possible refuge from ocean acidification due to seagrass

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productivity (Manzello et al., 2012)."

The area that showed the elevation increase was in the lower sub-region of the Upper Florida Keys (not in the Lower Florida Keys), see Figure 1a in the manuscript. The mean elevation change in the Lower Florida Keys was -0.3 m (see Table 1 in manuscript), and was similar to the elevation loss we observed in the central sub-region of the Upper Florida Keys. These data would suggest that the inimical waters could be less likely to have had an impact on this sub-region of the Upper Florida Keys.

R3: All of this would suggest that these two areas have had very different exposures to natural stresses; this would presumably make for very different susceptibilities in more recent times when increasing anthropogenic stress is set up as the main driver. It may also be noteworthy that the sediment thicknesses in these two areas are different and there is evidence that sediment retention around Buck Island (much higher wave energy and susceptibility to both storm damage and sediment export) may tend to be less than is the case in the Keys. If the latter is true, then changes in substrate elevation might be sediment export in one place, bioerosion in another and wholesale loss of rugosity in all.

AR: Interestingly, our volume calculations showed the lowest 'maximum net volume' loss (Table 1, column 3 in manuscript) and 'mean elevation' loss (Table 1, column 10 in manuscript) at Buck Island, suggesting that sediment/material retention was greater at Buck Island over the study period than in the Florida Keys. We suggest that this could be due to loss of rugosity (large framework building coral) but less export of materials at Buck Island. Recent photographic surveys (2015) along the eastern coast of Buck Island (unpublished, C, Storlazzi, USGS) show large stands of *Acropora palmata* coral colonies that have died and toppled over, but remain largely in tact and in place on the seafloor along the northeastern side of the island from approximately the eastern-most point to at least the mid point of the island (see Figure 1a, b, and c in this response), consistent with our observation of greater elevation loss in that area (see Figure 2c in manuscript). The southeastern side of the island from the eastern-most

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point to at least the barrier break was characterized by much more live *A. palmata* coral (see Figure 1d, e, and f in this response), also consistent with our observation of increased elevation in that area (see Figure 2c in manuscript). These photos illustrate how elevation and volume can be lost without a large amount of export. The live coral colonies clearly show relatively high elevation and colony volume that consists of both coral branches as well as very large open (pore) spaces between the branches. As large colonies topple, the coral branches break and the colony compacts causing a loss in elevation and volume as the open spaces between the branches are minimized. Large coral fragments are much harder to transport than sediments, typically require high-energy storm events for movement, and are, thus, not as easily exported from the system. This type of physical coral degradation is very different from degradation due to bioerosion by grazers whereby the coral is more directly reduced to sand that can be more easily transported and exported. Additionally, coral rubble fields generated from toppling corals can create a rubble pavement on top of sand making it more difficult to transport. Our observations of much lower elevation and volume loss at Buck Island relative to our other study sites combined with our observations that 91% of this study site was covered by coral-dominated substrate that accounted for over 90% of the volume loss is very consistent with this type of physical coral degradation process. The reviewer notes that parrotfish populations have decreased in the Caribbean, so it is also possible that less sand sized material is being generated for easy export at Caribbean sites than at sites with higher rates of bioerosion; and that may also help account for the lower export rates at this study site. We can include this discussion and the photos in Figure 1 (in this review) in the revised manuscript in support of our suggestion of physical degradation as a key process here; but, again, we do not have adequate data to fully quantify the contribution of physical degradation.

R3: I assume that the substrate type and sediment thickness was not consistently noted in older surveys. Given the points above, this could be an important driver of how quickly substrate elevation might change in one place versus another. The wholesale loss of architecturally complex acroporids and the subsequent reduction of these to

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pavement could be construed as “degradation of framework-building corals” as could bioerosion. Which was the main agent in each case?

AR: Unfortunately, substrate type and sediment thickness were not consistently noted in the older surveys.

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-407, 2016.

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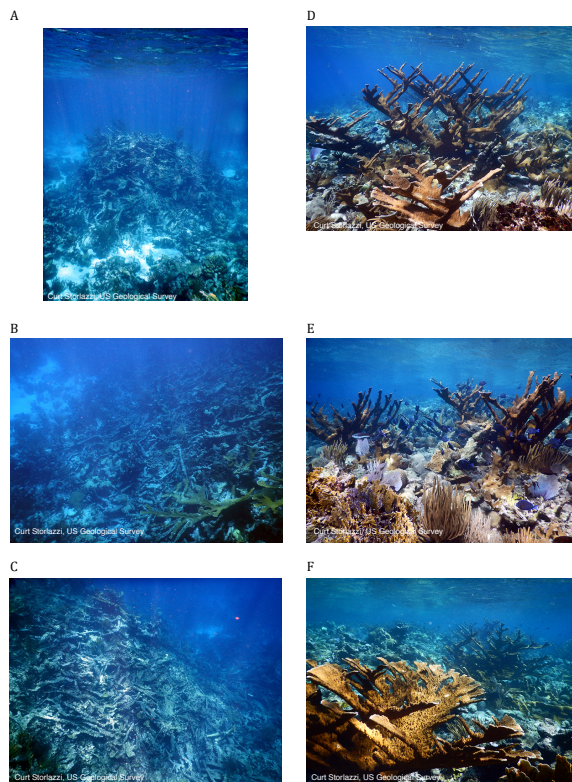


Fig. 1. Toppled (a -c) and live (d - f) *Acropora palmata* coral colonies along the coastline of Buck Island, USVI

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