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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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We appreciate the detailed comments provided by Reviewer #2, and note that the reviewer indicated that he/she had a difficult time comprehending our methods and results. Many of the review comments indicate the reviewer may be unfamiliar with the technical aspects and terminology associated with the type of analyses we performed in our paper. We agree that there are areas that can be explained in more detail, and we also discovered a few typos that occurred during upload and conversion of our manuscript file that may have also caused some confusion. We greatly appreciate that this review allows us the opportunity to clarify and correct any misunderstandings or confusion, and to expand our explanations and discussions so that our presentation can better address a broader audience of readers. However, we, respectfully, disagree with the reviewer's general conclusion regarding the scientific quality of our paper. In-

dividual comments are addressed below (reviewer comments indicated by 'R2', author response indicated by 'AR').

R2: General comments: Both the scientific quality of the paper and its presentation quality are generally insuffi- cient. The scope of the paper, as it is formulated pages 2 and 3, i.e. "measuring changes in seafloor elevation to assess and predict the impact of reef degradation on the vulnera- bility of coastal communities to sea-related hazards" is confusing.

AR: The statement to which the reviewer refers is located on lines 29-31 of page 2 and reads:

"Therefore, measures of total system change in seafloor elevation and volume are required to accurately assess and predict the impact of reef degradation on the vulnerability of coastal communities to hazards caused by storms, waves, sea level rise and erosion."

This is not a statement of the scope of our paper, but rather a statement regarding the need for the type of comprehensive elevation change analysis we performed. This sentence concludes a paragraph that summarizes the numerous types of studies that have been performed to look at individual accretion and erosion processes, states that no prior studies have accounted for the net result of all of these processes combined, and points out that vertical accretion and erosion is a function of total mass balance. The point we are making in the referenced sentence is that accurate predictions of coastal hazards depend on accurate measurement of changes in seafloor elevation (from a modeling standpoint). While many studies have examined individual processes that contribute to accretion and erosion in coral reef ecosystems, none (of which we know) have provided a measure of total system change due to combined accretion and erosion processes.

The scope of our paper is clearly stated in the abstract on page 1, lines 11-13 and lines 18-19 as:

"Here, we provide a comprehensive assessment of the combined effect of all of the processes affecting seafloor accretion and erosion by measuring changes in seafloor elevation and volume for 5 coral reef ecosystems in the Atlantic, Pacific and Caribbean over the last several decades."

"We show that regional-scale loss of seafloor elevation and volume has accelerated the rate of relative sea level rise in these regions."

The scope of our paper is then stated again, immediately after the sentence referenced by the reviewer, on page 2, lines 32-33 as:

"Here, we quantify the combined effect of all constructive and destructive processes on modern coral reef ecosystems by measuring regional-scale changes in seafloor elevation."

We will rewrite the sentence on lines 32-33 to read "The aim of our study is to quantify the combined effect of all constructive processes that cause accretion (or increases in seafloor elevation) and destructive processes that cause erosion (or decreases in seafloor elevation) on modern coral reef ecosystems by measuring regional-scale changes in seafloor elevation" to distinguish the scope of our study from the statement of need for this type of work.

R2: The title of the paper itself is also unclear.

AR: We chose this title because it summarizes the major finding of our work, namely that seafloor elevation is decreasing (rather than increasing) while sea level is rising in the coral reef ecosystems we studied (thus, we use the term divergence); and the combination of seafloor elevation loss and sea level rise has accelerated the relative increase in water depth at these locations. The title of the paper is derived from our concluding statement on page 16, lines19-21 that states:

"The divergence between rising sea level and declining seafloor elevation has already increased the risk to coastlines in these regions from long-term, persistent oceano-

graphic pressures and periodic events such as storms."

We prefer to re-write the title to read: "Divergence of seafloor elevation and sea level rise in coral reef ecosystems" to more accurately limit our conclusions to the sites we studied based on comments from Reviewer 2 regarding page 14, lines 26-28.

R2: The authors announce that they address the coral reef issue and then they provide results on various habitats, including non-coralline habitats and even deep water offshore habitats (e.g., page 13, lines 3-5).

AR: Correction, we clearly state that our study sites encompass coral reef ecosystems, not only coral reefs (e.g. page 1 lines 12-13, page 2, line 32). We recognize that the definition of a coral reef and the term coral reef ecosystem have long been debated by coral reef ecologists and geologists. However, it is generally accepted that coral reef ecosystems include many non-coral dominated habitat/substrate types such as seagrass, hard bottom, sand bottom, macroalgal-dominated communities, as well as coral-dominated substrate including framework building reef structure. We chose to perform our analyses at the ecosystem scale because accretion in non-coral dominated habitats, as well as off shore habitats (and beaches which are excluded from our study) is supported by sand/sediment production from the breakdown of carbonate produced by corals and other calcifying reef ecosystem organisms. It is also well known that coral reefs have been degrading rapidly over the past few decades. However, little is known as to the effect of reef ecosystem degradation on accretion/erosion of non-coral dominated habitats within coral reef ecosystems. Our results suggest that the balance of erosion versus accretion has tipped enough that carbonate sediment production in these coral reef ecosystems is no longer sufficient to support accretion of adjacent habitats as indicated by the broad scale loss of seafloor elevation (erosion) that we observe across all habitat types. We will add a statement to our revised paper clarifying what we define as a coral reef ecosystem for the purpose of our study.

R2: Authors are unclear on what they measure, and on my view they fail in generating robust data (as both the data and the methods used lack accuracy. Thereafter, the presentation of the results and their interpretation are confusing, as various types of processes are invoked to explain changes, with no specific process being robustly studied (e.g. page 13, lines 30-34).

AR: We understand that the methods used in our study to collect, process, validate and analyze the data are complicated. However, the general concept of the method is relatively simple, based on calculating the difference between elevation data points at the same location but from different time periods. We have created a flow diagram that depicts the core processing steps (see Figure 1 in our response to Reviewer 1), and we will include this figure in our revised manuscript to help non-experts better understand the process. We will also move Supplementary Tables 1, 2 and 3 (currently in the supplementary material section) to the main paper. These tables provide details regarding the data sources, data conversions and study periods for each study site to help further clarify our methods.

We measured the differences between historical and modern seafloor elevation using data sets from the time periods indicated for each study site (see Table S1 in the supplementary materials). This method allowed us to comprehensively measure the net change in seafloor elevation due to all of the constructive processes that cause accretion (or increases in seafloor elevation) and the destructive processes that cause erosion (or decreases in seafloor elevation), which was the scope of our study, the results of which represent a major finding on their own. It was NOT the scope of our study to robustly study the individual processes contributing to the changes we observed. However, we discuss the various accretion and erosion processes (and their rates as measured in previous studies) that are known to contribute to seafloor elevation change. We then place our results in context with published rates of accretion and erosion from these previous studies as is standard practice for rigorous discussion and interpretation of new results.

Although the general concept of our work is straightforward, a number of very rigor-

ous analyses were performed to test the validity of comparing historical and modern data sets; very conservative methods were used to calculate error associated with the methods; and the effect of that error on our results was quantified and reported. We provide a detailed discussion of these analyses in our Response to Reviewer 1. We believe that our very rigorous analysis of the data and conservative computation and reporting of error has resulted in a robust data set and quantitatively significant results.

R2: For example, the first paragraph of the Discussion Section clearly illustrates the wide (and unprecise) area covered by the paper (see page 14, lines 5-15).

AR: We do not discuss the area covered by our study sites in this paragraph. However, our study sites are very clearly defined on the maps in figures 1-3 of the main paper, and the exact size of each study area as well as each habitat area within each study site is provided in Table 2 (in manuscript). The paragraph on page 14, lines 5-15 discusses the large number of processes that cause seafloor elevation and volume changes and the very general time frames over which they occur. We are, therefore, confused by this comment.

R2: Both the concepts ("change in seafloor elevation and volume" – in fact, it seems that the authors address "changes in shallow waters depth") and the method used (method "traditionally used to monitor seafloor changes", "use of historical bathymetric data from the 1930's to 1980's and LiDAR DEMs from 1990's to 2000's) –which are presented firstly in the introduction of the paper (pages 2-3) and then in the Methods Section (page 4) – are questionable and not accurate.

AR: We used methods practiced and tested by coastal engineers (including the U.S. Army Corps of Engineers) that have long been used to quantify changes in seafloor elevation over time (page 3, lines 2-5). We encourage the reviewer to read the references provided in this section to better understand the rigor with which these analyses are performed. The method is as accurate as the data sets, we provide a detailed error analysis of the data in section 2.5 of the manuscript, and account for that error in our results. We have also provided further discussion in our response to Reviewer 1. With respect to the reviewer's concern regarding "changes in shallow water depth", we converted the sounding data (water depth data) to seafloor elevation using the NOAA National Ocean Service VDatum version 3.6 vertical transformation tool (see page 4, lines 15-18), which is standard practice for this field of study. This allowed us to compare historical elevation data with modern elevation data. Additionally, we used the locations of the historical soundings to extract modern elevation values from the seamless LiDAR digital elevation models which is more accurate than determining elevation change from two interpolated elevation surfaces (see page 5, lines 1-5).

R2: Concerning data and methods - How can bathymetric data from the 1930's to 1980's (constituting a single coherent period reflecting low anthropogenic impact?) be con- sidered as a starting point (or reference) to "measure changes in seafloor elevation" and then be compared with data from the 1990's to the 2000's (= period reflecting high anthropogenic impact?).

AR: Each of our five study sites was analyzed independently over a specific time period using the oldest reliable data that we could find for each site as well as the most recent bathymetric data available for that site. We did not measure change over a continuous historic range of 1930's to 80's and modern range of 1990's to 2000's. For example, the Upper Florida Keys historic data set was from 1934 and 35, and the modern data were from 2002. The dates for the individual data sets are available in Table S1 of the Supplementary Materials. The aim of the study was to look at change in seafloor elevation over several decades. Within the time frame of the study, population approximately doubled at each of the populated study sites (with the exception of uninhabited Buck Island). We clearly state that we use population as a first order approximation of relative anthropogenic impact. We make no claims of comprehensively analyzing all anthropogenic impact factors (see page 3, lines 16-21).

R2: This raises several key questions. Firstly, how can the "mag- nitude of erosion" (page 3, line 9) be measured using such an approach that poses serious questions

relating to the scientific quality of both the datasets and the method used. In other words, how can historical bathymetric data be compared with LiDAR data? The former (bathymetric data from the 1930's and next decades) do not have the required resolution for comparative measurements to be undertaken with LiDAR data. The low resolution of historical bathymetric data may generate significant errors in the results generated. Incidentally and curiously, no clear and complete information is provided by the authors on the resolution of the various datasets used in this study at the various study sites.

AR: Thank you for pointing out that we did, inadvertently, omit the vertical resolution information for our data sets. This information is available in the published metadata for each data set as referenced in Table S1 of the Supplementary Material. We will include that information in the revised manuscript and we discuss it below.

During these surveys several methods were used to collect soundings by USC&GS/NOS including lead line (0.15 m resolution), graduated sounding pole (0.15 m resolution) and fathometer (0.3 m resolution), and were noted in each NOAA hydrorgraphic data sheet (H-sheet) descriptive report. LiDAR data resolution is also reported in the metadata for each data set and ranged from 0.135 to 0.15 m. Therefore, the vertical resolution of lead line and sounding pole methods was similar to LiDAR methods. In general, for the 1930's surveys, sounding poles were often used in depths approximately less than 3 m and replaced with lead lines at greater depths. Descriptive reports from these data sets indicate that often the seafloor was visible due to high water clarity during pole and lead line data collection (which improves accuracy of the measurement). The most common error likely to occur during use of lead lines or sounding poles was overestimation of water depth due to angling of the line or pole as currents move the boat past the point of measurement. Overestimation of historical water depth would erroneously decrease elevation losses calculated using our methods. Therefore, it is more likely that our erosion estimates are underestimated rather than overestimated due to error associated with lead line and poling techniques. Additionally, we performed an analysis of differences between historical and modern elevation for areas of pavement that showed little change over the time periods (see response to Reviewer 1 for a detailed discussion). These results showed that average differences between modern and historical data sets for locations where little change has occurred range from 3 to 6 cm, providing evidence of the validity of comparing these data sets. Also note that we performed an independent, and more rigorous, error analysis of original repeat point measurements for the historical data sets, and used these error values (RMSESounding, Table 4) in our calculation (Equation 1, in manuscript) of total vertical error (RMSETotal, Table 4 in manuscript).

R2: Authors first indicate a 1 to 4Âa ÌĘm horizontal spatial reso- lution (this is a low resolution that do not allow the calculation of changes in the reef level) and then indicate a 11-12Âa ÌĘcm vertical resolution (which is questionable given the data used).

AR: The 1 to 4 m horizontal resolution applies to the LiDAR data sets only. We calculated horizontal error for the oldest historical data using published values for the methods (pages 10 and 11, section 2.5.2) of 4.8 m. To estimate the effect of horizontal error on our seafloor volume results, we performed a horizontal shift analysis. For this analysis, we doubled our calculated horizontal error to 10 m, then shifted the historical data set relative to the modern data set by 10 m in each of the four cardinal directions (N, S, E, W) and recalculated volume change for each scenario. These results indicate that horizontal error of up to 10 m and the resulting offsets in sounding points affects our volume calculations by 10% to 21% (depending on density of data points) and does not change the outcome or conclusion of our study. These results are consistent with reports that, over large areas (such as in our study), random errors largely cancel-out relative to change calculations derived from two surfaces (Byrnes et al. 2002). Additionally, this analysis provides further evidence that seafloor elevation loss is occurring at a very broad-scale across all habitat types.

With respect to vertical resolution, the reviewer's statement is incorrect. Nowhere in the paper do we claim vertical resolution of 11-12 cm. We believe the reviewer may be

making reference to the VDatum transformation error that we report for each data set that is only one parameter used to calculate total error (see page 8, lines 30-32 and Equation 1 in manuscript). We state the maximumum cumulative uncertainty for operational VDatum regions of South Florida and the Virgin Islands are 9.6 and 11.8 cm, respectively as reported by the NOAA National Ocean Service (page 8, line 31-32). We also report the VDatum error that was calculated specifically for each individual study site under the RMSEVDatum column in Table 4 of the manuscript that ranges from 8.1 to 11.4 cm (note no VDatum error was reported for Maui because no VDatum transformation was required). Total vertical error for each study site was calculated using equation 1, reported in Table 4 under the column heading RMSETotal, and ranged from 20 to 37 cm (average of 29 cm for all sites). As discussed on page 9, lines 30-31 and page 10, lines 1-6 (and in our response to Reviewer 1), we used a very conservative approach in our consideration of vertical error by multiplying our RMSETotal by a factor of 1.65 to encompass 90% of the variance in our data. This approach generated a more conservative RMSE of 0.48 m that we rounded up to 0.5 m; and we used this value to set minimum and maximum bounds in our volume calculations (Table 1 in manuscript). The minimum volume change values that we report in Table 1 were calculated by only including elevation changes that exceeded the range of -0.5 to +0.5 m to provide a very conservative estimate of volume change. These very conservative minimum volume change values also support our conclusions of net seafloor erosion at all study sites (Table 1 in manuscript).

R2: A second methodological problem is raised by the way anthropogenic impacts are considered in this study. (1) How can the "anthropogenic impact" only be measured by population numbers? In the present case (changes in water depth), it mostly depends on coastal and maritime human practices (sustainable/not sustainable). Major human activities, such as dredging in the substratum (should it be coralline or not) and extract- ing aggregate in particular, which may have occurred over the study period at some study sites and may have changed water depth, are not considered at all by the au- thors, which introduces a serious bias in the "elevation changes measured".

Some parts of the paper, such as “However, greatest mean elevation losses occurred in coral-dominated habitats and near the central coastline where harbour and shipping channels existÂa ÌĘÂz ÌĞ (page 13, lines 22-23) clearly indicate that not taking into account these human activities is problematic when assessing changes in shallow water depth.

AR: This claim is incorrect. In fact, on page 14, lines 8-18 (first paragraph of our Discussion), we specifically state that we DID include changes due to episodic events including, for example, dredging and infilling of channels and coastal harbors. This paragraph is quoted below:

“Our results include elevation and volume changes caused by chronic erosion processes that occur slowly over time frames of months to decades such as changes in carbonate production rates, bioerosion, chemical erosion from carbonate dissolution, degradation of large framework building coral colonies, and physical movement of reef sediments due to persistent oceanographic conditions such as waves and currents. Our results also include changes caused by episodic events that occur over very short time frames of minutes to days, and often cause large changes in elevation. Examples include dredging and infilling of channels and coastal harbors, deposition of terrigenous materials from landslides and run-off, slumping and relocation of seafloor materials at steeply sloping locations, storm erosion and deposits. We included large elevation-change 15 data in our calculations likely caused from these episodic events because such changes affect process modeling for hazards analysis and alter habitat distribution. We note that much reef degradation contributing to elevation change likely occurred after 1970 (Gardner et al., 2003; Bruno and Selig, 2007; 2014). Therefore, data sets containing pre-1970’s data (Table S1) could be biased toward lower annual elevation and volume-change rates.”

Additionally, we state both in the Abstract (page 1, lines 11-12) and in the Introduction on page 2, lines 32-22 that we provide a comprehensive assessment of the combined effect of all processes affecting seafloor accretion and erosion (constructive and de-

structive) on modern coral reef ecosystems. We provide a list of examples of all of the processes included in our comprehensive assessment on page 2, lines 24-28 that includes direct human alterations to the seafloor:

“However, no prior studies provide a comprehensive assessment of total seafloor elevation and volume change due to the combined effect of all of the processes affecting seafloor accretion and erosion (i.e., including physical erosion; redistribution, import or export of seafloor sediments; compaction; direct human alterations to the seafloor, carbonate production, bioerosion, chemical erosion).”

We include a figure in this response (see Figure 1 in this comment to Reviewer 2) that provides a very clear example of one of the human alterations included in our data set. Figure 1 (in this comment) is derived from Figure 2a in the manuscript and clearly shows an accretion area surrounding the airport in Charlotte Amalie, St. Thomas, USVI that resulted from infilling during construction of the airport. Again, the advantage of our seafloor elevation change method is that it does include all of the processes that cause accretion and erosion in a system.

R2: Generally, this paper mainly appears as a “technical” paper that describes the GIS pro- cedure applied to calculate changes in elevation, without addressing in an adequate way the conceptual, and the data and methods aspects raised. It seems that authors do not have the required background to address the complex scientific question that they have chosen to address. The technical procedure described on pages 4-6 is in- comprehensible to me. Despite the fact that I failed in understanding this procedure, my feeling is that the method is not robust due to poor conceptual, and data and method, bases.

AR: We feel that the reviewer has discounted the rigor and significance of our work due, perhaps, to a lack of expertise in the methods applied in our study. We hope that our responses have clarified our procedures and the conservative nature with which we have calculated and considered error in our results and interpretation. No previous work (of which we know) has performed such a comprehensive analysis of the net result of all erosion and accretion processes affecting these coral reef ecosystems at the regional scale. As researchers in a science agency program whose mission includes assessing and predicting coastal hazards from natural and anthropogenic impacts to coastal ecosystems (including coral reefs), we are well versed in the concepts and complex scientific questions in our field of study that we have chosen to address.

Our findings showing that the magnitude of regional scale seafloor erosion that has occurred in these systems has increased relative sea level rise causing water depths not expected to occur until 2100 are, in fact, very significant. We recognize that total erosion at the regional scale has likely been underestimated because no prior studies fully account for all processes causing elevation change in coral reef ecosystems; and we understand that our results may cause controversial feelings. However, we feel that we have proven the validity of our results and use of historical and modern data sets for our analyses with our expanded error analysis and our use of a very conservative RMSE for data calculations. Our conclusions regarding loss of seafloor volume are based on actual measurements of elevation-change shown to be statistically significant in over 90% of the habitats we analyzed, and that account for all of the processes causing elevation loss in these regions. We have described our technical procedures in great detail because they are complex, this is the first application of these methods to coral reef regions, and we hope that other scientists are able to apply these methods in many other coral reef regions.

R2: In different sections of the paper (e.g., page 13, line 1), the results obtained are correlated to generalities, e.g. on coral reef degradation, which is questionable. Results should be correlated to local data on reef health, including observed changes in living coral coverage, but not to worldwide observations. The interpretation of the results generated is not satisfactory: for example, the au- thors mention hurricanes as key controls of changes in depth. This raises the question of “what is measured, either long-term changes related to climate change and sea level rise, or changes due to

low-frequency high-magnitude events”? Once again, this makes the paper confusing.

AR: We are somewhat confused by reviewer’s comment and the reference to page 13, line 1 which only states the number of habitats in which mean elevation and volume losses occurred in the Upper and Lower Florida Keys. We do, in fact, cite local data on reef health and processes that support our observations. For example, on page 12, lines 32-22 and page 12, line 1, we state:

“Largest mean elevation losses occurred at shallow patch and aggregate reefs, coral-dominated and reef rubble habitats, consistent with documented declines in abundance of large framework-building corals over the past several decades (2014).” We note that part of this reference was missing due to a typo. The 2014 reference that should have been included here (and is in our reference list) is:

Jackson, J., Donovan, M., Cramer, K., and Lam, V. (Eds.): Status and trends of Caribbean coral reefs: 1970-2012, Global Coral Reef Monitoring Network, IUCN, Gland, Switzerland, 306 pp., 2014.

We will correct this in our revised version. The Jackson et al. study is a comprehensive analysis of coral cover throughout the Caribbean including data for change in coral cover from the 1970’s to 2000’s for both the Upper and Lower Florida Keys, for Buck Island from 1989-2011, and for St. Thomas from 1979-2010. We will include this reference in our discussion of the Buck Island and St. Thomas results. Additionally, as recommended by Reviewer 1, we will also discuss changes in live coral cover for each of our study sites in the text of our revised manuscript.

On page 13, lines 9-12, we note that a sub-region of the Upper Florida Keys showed a slight increase in elevation and that this location is near an area of the Middle Florida Keys that has been identified by Manzello et al. (2012) as a possible refuge from ocean acidification based on local data for that area. On page 13, lines 16 -19, we reference work by Lidz et al. (2007) and Shinn et al. (2003) that corroborates our observation of accretion on spur-and-groove habitat due to burial by sand and evidence for redistribution of reef materials by hurricanes that has caused erosion in some areas and deposition in other areas of the Florida reef tract. On page 15, lines 5-8, we compare our erosion rate in the Upper Florida Keys to chemical erosion rates determined for the Florida Keys by Muehllehner et al. (2016). All of these are examples of comparison of our results to local data on reef health and processes. We previously discussed that our methods and results account for both long-term changes over the time periods of our study as well as low-frequency, high-magnitude events (see page 14, lines 8-18).

We do include a discussion of our results in context with, what we believe, are seminal papers on comprehensive (large-scale) analyses of various accretion and erosion processes that are generally accepted by our scientific peers (e.g., Shinn et al., 1977; Buddemeier and Smith, 1988; Church et al., 2013; Perry et al., 2013; among many others, see Discussion Section 4). Given that we performed ecosystem-scale analyses in the Atlantic, Caribbean and Pacific regions, we felt it appropriate to place our results in context with broader observations across these regions.

R2: Specific comments: Page 2, lines 28 to 30 are incomprehensible: “measures of total system change in seafloor elevation and volume are required to accurately assess and predict the impact of reef degradation on the vulnerability of coastal communities to hazards caused by storms, waves, sea level rise and erosion”.

AR: We will clarify this passage to explain that hydrodynamic and other numerical models used to assess and predict the impact of reef degradation on coastal hazard vulnerability require accurate seafloor elevation data as well as accurate seafloor elevation change data.

R2: Page2, lines 31-32: “we quantify the combined effect of all constructive and destructive processes on modern coral reef ecosystems by measuring regional-scale changes in seafloor elevation” is incomprehensible.

AR: All accretion and erosion in coastal systems causes changes in seafloor elevation. Accretion is a constructive process and erosion is a destructive process. We will rewrite

this sentence to clarify.

R2: Page 3: “we adapted an elevation-change analysis method that has traditionally been used to monitor seafloor changes”

AR: We are uncertain as to your question regarding this statement.

R2: Page 7, line 25 “sediment thickness of the Holocene reef deposit”: what do the authors talk about? Vertical sedimentation? Vertical Holocene reef building? The results exposed page 8, lines 11 to 14 for the Lower Florida Keys case study are incomprehensible to me. I do not understand how the authors “used a moder reef age of 6000 years and a constant erosion rate” to “compute the time required to completely erode the remaining Holocene reef down to the Pleistocene layer”.

AR: The Holocene epoch began approximately 12,000 to 11,500 years ago after the Pleistocene epoch. The terms ‘recent’ and ‘modern’ reef are often used to describe the reef deposit that accreted during the Holocene. We are referring to vertical sedimentation of the Holocene reef ecosystem in the Florida Keys for these sections. The geologic history of the Florida reef tract is very well known, and the coral reefs and sediments of the present ecosystem began accumulating approximately 6000 to 7000 years ago on top of Pleistocene bedrock as described in detail in Lidz et al. (2007) that we cite. Thus, we used 6000 years as the modern reef age for this calculation. The constant erosion rate to which we refer is the erosion rate that we calculated from our elevation change analysis of the Lower Florida Keys study site. We assumed no change in this rate over time, for this calculation. We will add a few sentences to Section 2.4 describing the geologic setting of the Florida Keys reef tract for clarification.

R2: Page 8, lines 29-30: I am surprised to read that vertical errors would be comprised between 9.6 and 11.8 cm respectively, given what I know on LiDAR data and the horizontal error (1 to 4Âa ÌĘm) applying to this study. More generally, I do not understand how vertical error estimation was conducted.

AR: The reviewer has misunderstood the terminology on lines 29-30. The vertical error to which the reviewer is referring is the maximum cumulative uncertainty caused by transforming data from one vertical datum to another using the VDatum software, and represents only one component of our vertical error analysis (see page 8, lines 29-30). Please see equation 1 on page 8, line 24. This type of equation is widely used for calculation of root mean square error (RMSE) in elevation change analyses that considers multiple sources of error.

Our total vertical error analysis included error terms for:

1) modern LiDAR data sets (RMSELiDAR). LiDAR uncertainty was determined by independent validation of airborne LiDAR measurements with in-water acoustic sounding measurements performed at the time that the LiDAR data was collected and reported in the metadata for these data sets.

2) historical data sets (RMSESounding) as determined from our analysis of repeat measurements that were performed by the original surveyors at the time of data collection, and

3) uncertainty due to transforming data to a common vertical datum as calculated using VDatum (RMSEVDatum) for each individual data set.

These uncertainty values specific to each data set are reported in Table 4 (in manuscript), and were included in our calculations of RMSE (see page 8, equation 1 for RMSETotal). Our average RMSETotal (Table 4 in manuscript) for all study sites was 0.29 m. We considered the RMSETotal values from each study site as proxies for the standard deviations. In a normal distribution, data within plus or minus one standard deviation of the mean encompasses approximately 68% of the variability; and data within plus or minus 2 standard deviations of the mean encompasses approximately 95% of the variability. We note a typo on page 9, line 28 in the notation of this statement that may have caused some confusion, and we will correct this in the revised manuscript. We used the Normal Inverse Cumulative Distribution Function to

compute that 90% of our elevation change values would occur within plus or minus 1.65 standard deviation of the mean. We chose to multiply our RMSETotal by a factor of 1.65 to encompass 90% of the variance in our data and generate a more conservative RMSE of 0.48 m that we rounded up to 0.5 m; and we used this value to set minimum and maximum bounds in our volume calculations (Table 1 in manuscript). We will rewrite this passage in the revised manuscript to include more detail and clarify these calculations.

R2: Page 12, lines 13 to 27– We understand that most study are not dominated by coral reefs, which means that this paper does not in fact address the pretended issue of reef response to changing environmental conditions. This suggests that the choice of study sites is not totally coherent with the objectives of the paper.

AR: Again, we remind the reviewer that our study sites encompass coral reef ecosystems that include, but are not limited to coral reefs as discussed previously in our comment to the reviewer.

R2: Page 12, lines 25 to 27: the conclusions drawn by the authors from the study of Buck Island correlates volume loss to sediment export. Both the results (volume loss) and the interpretation of the results (sediment export) are unclear to the reader.

AR: The point that we are making here is that elevation and volume loss occurred in all of the regions, but to a lesser extent at the Buck Island study site. When materials are lost from a region (i.e. exported), that loss causes a decrease in volume of materials within that region. Therefore, this suggests that less export of materials is occurring from the Buck Island study site. We will rewrite this sentence to clarify.

R2: Page 14, lines 20-25: how can the authors convert “changes in elevation” into a “num- ber of years of Holocene reef accretion”? This is not robust as coral reefs grow and erode over a given period, as a result of the complex imbricated processes driving both reef construction (i.e. construction) and sediment production (i.e. erosion allowing car- bonate production).

AR: The point of this exercise was to further demonstrate the significance of these losses. We will rewrite this paragraph to clarify and include more detail on this calculation. Accretion of a reef ecosystem over time occurs when the balance of accumulation of reef materials and sediments exceeds erosion and loss of the eroded material from the system. The fact that we observed mean seafloor elevation loss across the whole coral reef ecosystem scales that we studied indicates that more materials are being eroded and exported from these systems than are accumulating. Our annual mean seafloor elevation losses on page 14, line 20 are calculated by dividing the total mean elevation change in meters reported in Table 1, column 3 (in manuscript) by the number of years in each time period, for each study site:

UFK = -0.1 m / 68 years = -1.5 mm/year

LFK = -0.3 m / 66 years = -4.5 mm/year

STT = -0.3 m / 48 years = -6.3 mm/year

BI = -0.09 m / 33 years = -2.7 mm/year

Maui = -0.8 m / 38 years = -21.0 mm/year

We then divided our total mean elevation losses losses (Table 1, column 3 in manuscript) by published rates of average Holocene reef accretion rates for these regions (mm per year) to estimate how many years of reef accretion was lost due to erosion:

UFK = -0.1 m / 2.6 mm/yr = 38 years

LFK = -0.3 m / 2.6 mm/yr = 115 years

STT = -0.3 m / 2.6 mm/yr = 115 years

BI = -0.09 m / 2.6 mm/yr = 35 years

Maui = -0.8 m / 10 mm/yr = 80 years

R2: Bottom of page 15-top of page 16: I do not understand how the results obtained by the authors can be compared to the results of previous studies conducted by C. Perry to attribute observed changes to specific drivers/processes.

AR: Perry's 2015 paper is one of the most recent studies showing that while remote coral reefs that are largely isolated from human influence experience severe coral mortality from climate driven impacts like bleaching, most of these reefs recover very rapidly and continue to produce enough carbonate to keep up with present and future sea level rise. The C. Perry study was based on assessment of 28 reefs across the Chagos Archipelago reef system in the Indian Ocean. The point we are making here is that, although Maui is remote, it is not isolated from human influence; and our results showing large erosion rates suggest that these reefs systems have not recovered well from degradation and are not producing enough carbonate to keep up with rising sea level. We will expand this discussion to clarify.

R2: Page 15, lines 19-21: the estimation that "the total reef volume could completely erode down to Pleistocene-bedrock-surface in approximately 1250 years" is not well-founded.

AR: We have clarified our concept and procedure for this analysis in our response to the reviewer's previous comment regarding page 7, line 25. We will expand the discussion as previously indicated.

R2: Page 15, line 33: ". . . reef systems. . . lack human impacts" is not correct in terms of style.

AR: The terminology used in this sentence is from Perry's 2015 paper, and we are not certain as to what the reviewer is recommending here. Please clarify.

R2: Page 15, lines 23-35: key references on reef islands future are not cited by the authors. See in particular the recent studies by Kench et al.

AR: Good suggestion. We assume the reviewer is referring to the latest 2015 papers listed below. We will include these in the discussion of our revised manuscript.

McLean, R., & Kench, P. (2015). Destruction or persistence of coral atoll islands in the face of 20th and 21st century sea-level rise? Wiley Interdisciplinary Reviews – Climate Change, 6 (5), 445-463. 10.1002/wcc.350

Kench, P. S., Thompson, D., Ford, M. R., Ogawa, H., & McLean, R. F. (2015). Coral islands defy sea-level rise over the past century: Records from a central Pacific atoll. Geology, 43 (6), 515-518. 10.1130/G36555.1

Kench, P. S., Owen, S. D., & Ford, M. R. (2014). Evidence for coral island formation during rising sea level in the central Pacific Ocean. Geophysical Research Letters, 41 (3), 820-827. 10.1002/2013GL059000

R2: Page 16, lines 3-5: an assumption like "Modern carbonate production rates are an or- der of magnitude lower than Holocene averages (Perry et al., 2013), and are estimated to decrease by as much as 60% by mid-century (Langdon and Atkinson, 2005)" is far too general.

AR: These statements are not assumptions, rather they are derived from the field and experimental results of the cited studies. We will add additional references to this section that support these statements. We do not use this data to quantify future responses of reefs, but rather to point out that carbonate production rates are projected to continue to decrease while bioerosion and chemical erosion are projected to increase in the future. These combined impacts are, in fact, likely to accelerate reef erosion rates.

R2: Tables 2 and 3 – The substrate categories included in these table are not presented and justified in the study. We additionally have not idea of the depth at which these habitats are situated.

AR: This statement is incorrect. The habitat/substrate maps are discussed and referenced on page 6, lines 18-27:

"We obtained benthic-habitat-map shapefiles (Florida Fish and Wildlife Conservation

Commission, 2015) for the Upper and Lower Florida Keys study sites from Florida Fish and Wildlife Conservation Commission (FWC). The Unified Florida Reef Tract (UFRT) map Version 1.2 is comprised of 5 class levels from 0 to 4. We used class level 2 for our study because the level of detail was consistent with benthic habitat data available at our other study sites. We obtained benthic-habitat-map shapefiles for the USVI and Maui from NOAA (Rohmann, 2001b, a; Battista and Christensen, 2007). We delineated USVI habitats using the 'type' descriptor in the shapefile's attribute table. We delineated Maui benthic habitats using the 'D_STRUCT' class in the attribute table. We retitled the habitat class named 'Rock/boulder' in the 'D_STRUCT' class that corresponded to the descriptor from the 'M_STRUCT' class named 'Coral Reef and Hard Bottom' to clarify that particular substrate type is a coral-dominated habitat. All classes were chosen to provide a common level of benthic habitat detail across study sites. Once the habitat classes were chosen, we exported them as individual shapefiles with ArcMap."

Both historical and LiDAR seafloor elevation data and latitude/longitude are provided for every single data point within these habitats for each study site in Tables S4 – S8 of the Supplementary materials. We can add mean water depths for each habitat type to Table 2.

R2: The maps provided page 30 indicate a complex spatial distribution of gains and losses, which is not described in the paper. They also show that shallow habitats were not totally covered, suggesting that gains may have occurred in non-covered areas that may compensate observed losses in study areas. This is all the more to be considered that the results obtained are contrasting (e.g. between the Central Sub-Region and the Lower Sub-region of the Florida Keys).

AR: We discuss the spatial distribution of gains and losses by habitat type in Section 3.2, and briefly discuss the large ranges of elevation change and the effect on standard deviations on page 11, lines 24 -28. We will expand the discussion of results in section 3.1 to more directly recognize the complexity of the spatial distribution of the data.

There are gaps in the data coverage for the Florida Keys and Buck Island data sets and we will note these in our revised manuscript. Our analysis was limited by the areal extent of available data sets. However, there is overwhelming evidence in the literature (as well as anecdotal evidence from field observations) that the trends we see in the areas covered by our data sets are consistent throughout the surrounding areas outside of the boundaries of our study sites. We feel that our study areas in these locations are at a large enough scale to be representative of the broader region. We do not argue that there are some locations within our study sites that show accretion. For example, offshore, downslope areas where sediment is infilling spur and groove formations; areas shoreward of patch reefs where sediment from degraded coral reefs has been redistributed to deeper water habitat behind the reef; and, in the case of the Lower Sub-region of the Upper Florida Keys, a small area of increased elevation primarily associated with seagrass beds (that we discuss on page 13, lines 10-12). However, the amount of total volume loss at these study sites is substantial. To put this in perspective, our results indicate that seafloor volume has decreased in the Upper Florida Keys study site by 14.6 (lower bound) to 37.9 (upper bound) million cubic meters. One million cubic meters is approximately the same volume as the Empire State Building. So, the amount of seafloor volume lost in this area is equivalent to approximately 14 to 37 Empire State Building's worth of material volume. There is no evidence from the numerous other geological, ecological, or geophysical studies throughout the Florida Keys for redistribution and deposition of this amount of seafloor material in the shallow habitats that lie between the outer reef tract and the shoreline of the Florida Keys.

R2: Concerning the Florida Keys, curiously nothing is said in this paper about the dominant modes of planform change and about Keys' landward migration. This suggests that the general context that allows interpreting correctly the results is not presented and considered when analysing the results.

As discussed above (re: Depth to Pleistocene Bedrock analysis), we will include a brief summary of Holocene reef formation and platform change in the Florida Keys to

help develop the context for the analysis described in section 2.4 (Lower Florida Keys – volume to Pleistocene Bedrock). The time period of our elevation change analysis in the Florida Keys focuses on changes over the past 64 to 68 years. We feel that a broader discussion of platform evolution and change that occurs over much longer time scales than the processes accounting for much of the change we observe (discussed in manuscript) is well beyond the scope of our paper. Additionally, all of our data were corrected for sea level rise that accounts for subsidence, etc.

R2: The results obtained in Saint Thomas, as shown by Map a page 31 mainly exhibit stability to limited elevation loss, if we consider grey and yellow areas. When we see this map, we are not convinced that elevation losses prevail, especially if we consider the error range. The same observation can be made when considering map c page 31 showing the situation of Buck Island (blue and yellow area are extensive).

AR: Unfortunately, we were required to change the color scheme of these figures to meet required color standards for the journal publication, and we agree that these figures don't do the data justice. We believe the reviewer is referring to the light yellow and light blue areas of elevation decrease and increase, respectively (the gray areas represent land). We will try to adjust the color schemes (within the color guidelines) in the revised manuscript so that the blue colors don't overwhelm the light yellows. I've included the figures from our unrevised submission (Figures 2 - 4 in this comment) that were color coded to better distinguish between elevation decreases and increases. However, we point to the actual data rather than the illustrations as proof that elevation losses exceed gains and of the significance of the results. It is important to note, that the light yellow and light blue boxes for the plus or minus 0.5 m elevation change data represent 1.65 x RMSETotal. Our RMSETotal was 0.29 m. Therefore, the light blue and yellow color areas include statistically significant data (greater than RMSETotal). We chose a plus or minus 0.5 m range for these figure categories because they represent the amount of data that we did NOT include in our minimum bound (conservative) volume calculations (see Table 1 in manuscript and page 6, lines 8-11 and page 9, lines

30-32 to page 10, lines 1-6 of vertical error analysis section). Even after we remove all of the data in light yellow and light blue (plus or minus 0.5 m) from our calculations, our results still show net volume loss/erosion of these study sites (see Table 1, column 9). The mean losses in 97% of the 59 habitats we analyzed for all study sites were greater than our RMSETotal of 0.29 m, and 77% of the habitats showed mean losses greater than 1.65 x RMSETotal of 0.5 m (see page bottom of page 11 and top of page 12, lines 30 – 6). One of 17 habitats in St. Thomas showed mean loss less than 0.29 m (reef rubble), and one of 11 habitats in Buck Island showed mean loss less than 0.29 m (seagrass), see Table 2, column 9 (Mean loss).

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

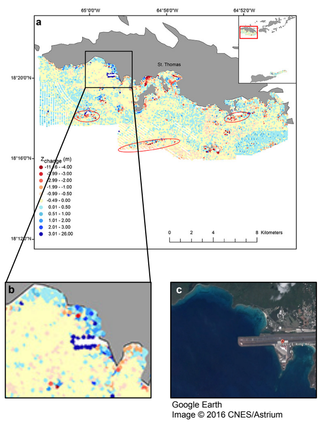

Fig. 1. Example of human alteration to seafloor included in data set. a) area of accretion near near airport, b) inset of accretion area, c) aerial imagery of airport

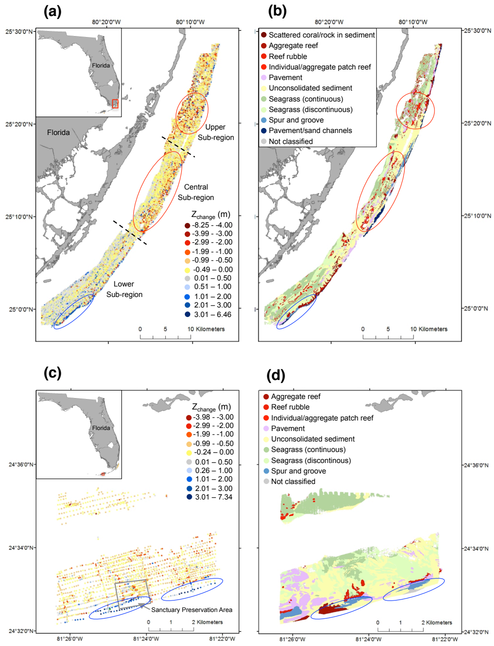

Fig. 2. Figure 1 from manuscript with original color scheme

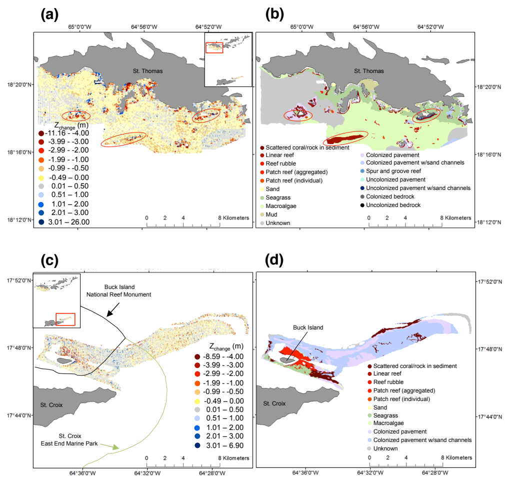

Fig. 3. Figure 2 from manuscript with original color scheme

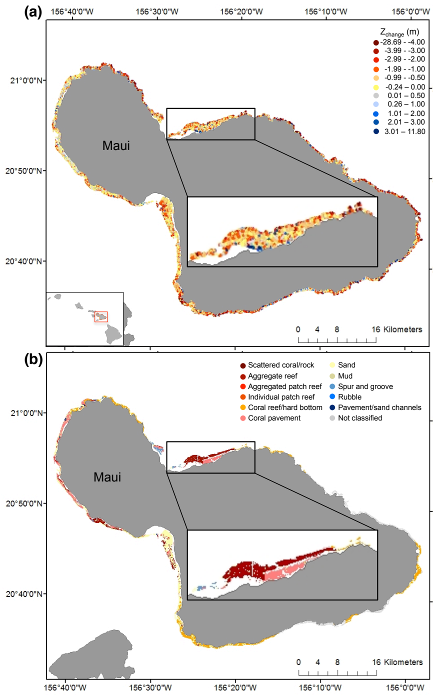

Fig. 4. Figure 3 from manuscript with original color scheme