

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

Anonymous Referee #2

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General comments:

Both the scientific quality of the paper and its presentation quality are generally insufficient.

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 vulnerability of coastal communities to sea-related hazards” is confusing. The title of the paper itself is also unclear. The authors announce that they address the coral reef issue and then they provide results on various habitats, including non-coraline habitats and even deep water offshore habitats (e.g., page 13, lines 3-5). 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

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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). 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).

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.

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 considered 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?). This raises several key questions. Firstly, how can the “magnitude 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. Authors first indicate a 1 to 4 m horizontal spatial resolution (this is a low resolution that do not allow the calculation of changes in the reef level) and then indicate a 11-12 cm vertical resolution (which is questionable given the data used).

A second methodological problem is raised by the way anthropogenic impacts are con-

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sidered 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 extracting 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 authors, 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 exist” (page 13, lines 22-23) clearly indicate that not taking into account these human activities is problematic when assessing changes in shallow water depth.

Generally, this paper mainly appears as a “technical” paper that describes the GIS procedure 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 incomprehensible 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.

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 authors 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.

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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”.

Page 2, 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.

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

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 moderate 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”.

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 4 m) applying to this study. More generally, I do not understand how vertical error estimation was conducted.

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.

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

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the interpretation of the results (sediment export) are unclear to the reader.

Page 14, lines 20-25: how can the authors convert “changes in elevation” into a “number 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 carbonate production).

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.

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.

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

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.

Page 16, lines 3-5: an assumption like “Modern carbonate production rates are an order 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.

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.

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

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that the results obtained are contrasting (e.g. between the Central Sub-Region and the Lower Sub-region of the Florida Keys). 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.

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).

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