

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

Kimberly K. Yates et al.

kyates@usgs.gov

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AR: We appreciate the effort from Reviewer 4 in considering all available reviewer comments and for synthesizing the most salient points. We found this very helpful in further organizing an approach to a revised manuscript. Individual comments are below (R4 indicates reviewer comment, AR indicates author response).

R4: The paper by Yates et al. analyses bathymetric data to quantify seafloor elevation changes in coral reef regions. As highlighted by reviewers 1 and 3, the dataset presented in the manuscript is especially impressive (number of sites considered, extent of the area considered). The results will be useful for coastal geomorphologists and managers concerned with the sustainability of coral reefs environments and the related ecosystem services. Despite the recommendation of reviewer 2, I think that the paper

C1

should be published after major revisions, to ensure that the amount of data analyzed in this work receives the attention it deserves.

Three previous reviews have extensively discussed the paper: overall, reviewers (1) have concerns regarding the ability of the method to retrieve seafloor elevation changes at the required accuracy; (2) made comments on the form of the paper; (3) and the interpretation of the results. The authors have already provided responses to several comments of the reviewers, and intend to implement corrections to their paper, which I think are reasonable. I would suggest that these major revisions are implemented, considering the following points:

- All reviewers agree that the paper should separate more clearly what is the overall approach (comparing bathymetric data) from the technical details of the GIS procedure used to produce this data. The authors have prepared a figure as part of their response to reviewer 1 to address this comment. However, I think that the figure remains too technical (e.g., use of the TIN surface wording), and I would support producing the detailed GIS procedure in an annex to the paper. Overall, I agree with previous reviews, who suggested that the authors should consider that their results may have a large impact beyond specialists of coastal bathymetric surveys, so that ideally, they should try to separate the main messages from the technical implementation details.

AR: We agree that separating the technical implementation details (GIS procedures) from a more clear discussion of the approach is an excellent idea, will greatly improve the paper, and will make it more accessible to a broader audience. We will rewrite the methods taking into account the suggestions by all of the reviewers, and will include the more detailed (GIS) steps in a methods supplementary section. We have included an example of a modified flow diagram showing our approach to describe the process in more general terms (See Figure 1 in this review response).

R4: - While the comparison of historical bathymetric sounding with contemporary Li-DAR is quite widespread in coastal geomorphology (as reminded by the authors, see

C2

AC2 – pages C6-C7), there is always the suspicion that the two techniques induce errors, as highlighted by reviewers 1 and 2. Such errors can arise because the techniques have not the same purpose and therefore don't necessarily capture the same proxies (e.g., highest seafloor elevation features for navigation applications vs average seafloor elevation feature for bathymetry data in support to coastal hydrographic modelling). The techniques also have different accuracy/precision (as discussed already), or because of time-sampling issues (as commented by reviewer 2). Overall, I think that the authors make a fair assessment of these errors: in the response of the authors to this comment of reviewer 2 (AC2 pages C8), information regarding the vertical resolution of the techniques is provided, while the precision issues are given in Table 4 in the original manuscript. To complete this assessment, I would suggest to provide more information on the planimetric resolution, and the vertical accuracy of the techniques in the core of the article. This includes details regarding the definition of a common reference, which incorporates sea-level rise constructions in a way, which is not completely clear to me based on the original manuscript, page 4 lines 10 and following. Nevertheless, I am confident this does not affect the results of the authors, as the RMSE in vertical datum adjustment is probably much larger than the RMSE due to uncertainties in relative sea-level changes for the sites of interest (table 4).

AR: We will use our responses to the reviewers regarding the vertical and horizontal error assessments and data resolution to expand and improve our discussion of these issues in the main manuscript. We will also include our pavement analysis and results as further evidence for the validity of comparing these data sets (with a strong caution that pavement cannot be appropriately used as a control, but can provide supporting information for proper error analyses). We will provide information and definitions (in general terms) of horizontal and vertical references (datums) for these data sets and the need to adjust data so that compared data sets are aligned to the same reference points. The data and sources we used for our sea level rise corrections were included in Table S2 of the supplementary section, and we will move this information to the main paper to help clarify our sea level rise adjustments. In general, long-term sea level rise

C3

data recorded by NOAA sea level trend stations in mm per year were used to calculate the total sea level rise at each study site by multiplying the mean rate of annual sea level rise over the study time period by the number of years between historical and modern data sets. These correction values ranged from approximately 7 cm to 16 cm and were added to the historical sounding value. NOAA reports 95% confidence intervals for these data ranging from approximately +/- 0.15 to +/- 0.81 mm/yr. The potential error from these corrections was insignificant relative to other sources of error, and we, therefore, excluded it from our RMSE calculations.

R4: - Regarding the interpretation of the results: besides the aspects discussed with reviewer 1 and 2, I think that reviewer 3 provides a very clear line for improving the discussion section, and I hope that the authors will build on it in a future version of the article.

AR: We agree that comments from Reviewer 3 were very helpful for improving our discussion section, and we have provided discussion on how we will use those comments to improve our paper in our Response to Reviewer 3.

R4: - Finally, I think that a "conclusion" section is needed.

AR: We agree that a conclusion section will improve the paper, and will include that in a revised manuscript.

R4: I hope these comments are useful.

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C4

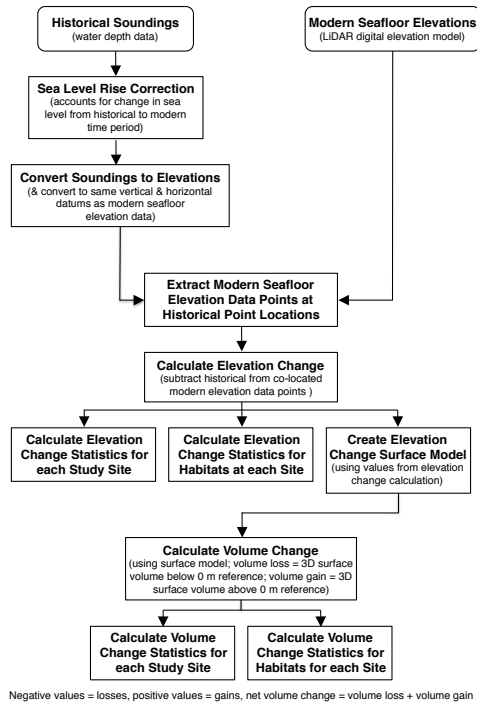


Fig. 1. Modified methods flow diagram for incorporation into revised manuscript