

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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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 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

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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.

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 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.

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

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assumptions about the degree to which they are related to bioerosion and the ensuing removal of sediment.

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

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

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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.

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 *hoa* 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). What is, therefore, critically important is a reliable estimate of export inasmuch 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.

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 ~ 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

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variable. Therefore, export can only get more significant in the budgeting attempted in this paper.

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.

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. 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.

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

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

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