Toth and Aronson Response to Reviewer #2

This manuscript sets out to explore the relationship between an accretion hiatus in a Panamanian coral reef (and reef growth hiatuses in other locations) and the 4.2ka event. The manuscript puts forward that the two are linked via changes in mid to late Holocene El Niño-Southern Oscillation (ENSO) variability. The co-timing and possible inter-relatedness of the Panama reef growth hiatus and the 4.2ka event is an intriguing possibility, however the narrow focus of the manuscript on ENSO as the cause is problematic.
Please see our responses below for treatment of other causes besides ENSO.

As the manuscript discusses, two out of the three major El Niño events of the past 40 years did not result in major mass coral death in the tropical eastern Pacific, La Niña events may or may not also lead to coral death in the region, and the relationship may be indirect (e.g. via Acanthaster outbreaks).
We exclude* Acanthaster *outbreaks as a cause in section 3. Also, please see our responses below under the reviewer's point 1 for discussion of both La Niña and the variability of responses to ENSO events.

Furthermore there is mixed evidence that there has even been a change in ENSO over the time period discussed. Together, this makes it is difficult to attribute the reef growth hiatus-4.2ka event co-timing to ENSO.
The majority of the ENSO literature supports the conclusion that there was an increase in ENSO variability after 4.2 ka and our records from Pacific Panamá suggest that, at least in the eastern Pacific, those changes coincided with the shutdown of reef accretion. This evidence provides the basis of our hypothesis that there may be a connection between the 4.2-ka event and ENSO and we hope that this hypothesis will be tested by other researchers in the future. We Please also see our response to comment 3.

The manuscript then extrapolates the reef growth hiatus-4.2ka event-ENSO links to explain other Pacific coral reefs. This is a stretch, especially without a more rounded discussion of the various factors that can influence reef growth at those locations. Overall, the manuscript needs to rebalance and expand the discussion to look at what non-ENSO factors (e.g. sea level, others) or ENSO-related factors (e.g. SST gradients, others) could also explain the reef growth hiatuses in the eastern Pacific and beyond.
We have framed our ideas more clearly as hypotheses. As remarked above and in our response to Comment 4, we have also treated other possible causes more thoroughly than in our previous version of the manuscript.

1. Present day ENSO impacts on tropical eastern Pacific reefs. The discussion presented in section 2.2. 'Response to ENSO events' is well written and interesting, however it does highlight one of the key issues with the manuscript. That is, that the relationship between ENSO and coral death is complicated. Large amplitude El Niño events have not universally resulted in mass bleaching (e.g. p5 149-169) and sometime impacts are indirect. It implies that only large El Niño events have an impact on reef accretion, but what about moderate events? Conversely, La Niña apparently can also have a negative effect on coral reef growth in this region (e.g. p7 241-247), however the effects of La Niña are not discussed in this section. A broader discussion of ENSO impacts is needed.

We have reorganized the text related to modern ENSO impacts to emphasize the role of La Niña. The section describing the impacts of La Niña in Pacific Panamá has been moved from Section 3 to Section 2.2 where the impacts of El Niño in Pacific Panamá are discussed:
La Niña is also problematic for corals in Pacific Panamá, as lowered sea level in the TEP during La Niña events causes more frequent coral mortality associated with subaerial exposure (Eakin and Glynn, 1996; Toth et al., 2017). In addition, La Niña is associated with elevated rainfall in Pacific Panamá, which increases turbidity, and enhanced upwelling, which reduces water temperatures, decreases pH, and increases nutrient levels, all of which act to suppress coral growth (Glynn, 1976).

The variability of ENSO impacts is an area of active research, and a detailed review of that topic is beyond the scope of this paper.

2. Abrupt transition. p8, 255-256 the manuscript describes an abrupt transition to cooler and wetter conditions. However, looking at Figure 2 there is no data from ~4.3 to 3.9ka so we do not know whether the transition was abrupt or not. The language around the commencement of the reef accretion hiatus needs to be toned down.
We do have data from our record of ΔR during this period, which suggests a rapid increase in upwelling, indicative of intensifying La Niña-like conditions; however, we removed the word *"abrupt"****.***

Related, why are there coral records through the hiatus? Presumably some corals survived. This point may have been explained by the authors in earlier papers but some explanation should be included in this manuscript.
We have added a description of these samples to the paragraph where we begin to describe our records. The beginning of that paragraph now reads:

We tested the hypothesis that changes in ENSO activity around ~4.2 ka triggered reef shutdown in Pacific Panamá by evaluating geochemical proxy records from corals in our cores (Fig. 2; Toth et al., 2015a, 2015b). For obvious reasons, the availability of coral samples from during the hiatus was limited; however, we were able to analyze the Sr/Ca and $\delta^{18}O$ *of seven coral skeletons, which our age model suggested grew at the beginning of the hiatus (~3.9–3.6 ka; Toth et al. 2015a). We also have a measurement of oceanic radiocarbon, a proxy for ocean circulation, from one coral that grew during this period.*

Figure 2 implies that the Sr/Ca and d18O records are continuous over the intervals where data is available, which they do not seem to be in earlier publications. The full coral data points should be presented.
We have added information to the caption for Figure 2, which clarifies that the Sr/Ca and d18O data are presented as 200-year running means of the raw data:

(B & C) provide reconstructions of temperature and salinity variability for Pacific Panamá based on 200-yr running means (±95% CIs) of Sr/Ca and δ18O of corals sampled from a core collected from the reef at Contadora Island in the Gulf of Panama. The individual data points used to generate the curves can be found in Toth et al. (2015a) and Toth (2013).

Also in Figure 2, the x-axis break is misleading because some of the Sr/Ca and d18O data overlap with the axis break â˘A˘T the data should be plotted on the full x-axis
Removing the axis break would require condensing the data in the figure to the point that it would be difficult for readers to discern the pertinent trends we discuss. We do not present any Sr/Ca or d¹⁸O data during the hiatus after the axis break (at 3.4 ka), so we are not sure what the reviewer is referring to. We did switch the direction of the axis break lines, which we hope will make this clearer.

3. Mid to late Holocene ENSO evidence. The study concludes that ENSO was the prime driver of the reef growth collapse, and presents a schematic of a sequence of ENSO-related changes (Fig. 3) to support their conclusions. The issue is that the sequence of events is not as clear as the manuscript present and some evidence has not been included. For example, p8, 261-264 cites Corrége et al. (2000) as evidence of ENSO but similar-aged results from Bayes Islet, presented in Emile-Geay et al. (2016), should also be included. Furthermore, the manuscript includes data from Moy et al. (2002), however there is some controversy over whether this record solely reflects El Niño events (e.g. Rodbell et al. QSR 2008; Emile-Geay and Tingley 2016). And, it could well be argued that reduced ENSO variability was established several centuries to a millennia before Panamá reef death (e.g. summarised in Emile-Geay et al. 2016). If ENSO variability was reduced in the centuries before the Panamá reef growth hiatus, is there really a link between ENSO and the reef growth? Furthermore, p8, 274-282 presents an argument relating the waxing and waning of ENSO variability working to suppress and then initiate reef accretion, and the discussion presents the ENSO literature as if this pattern of variability is well established. However, as the manuscript states on p9, 290-302 discusses that the pattern of ENSO variability over the mid to late Holocene is far from clear. This further makes it very difficult to attribute the reef accretion hiatus and re-establishment to ENSO. There are also some discrepancies in describing mid to late Holocene ENSO. For example, p10, 356-357 manuscript states "increased ENSO variability 4.2ka". However, earlier in the manuscript 4.2ka is described as having low ENSO variability (and as illustrated in the Figure 3 schematic). The descriptions of ENSO at 4.2ka, before and after is inconsistent throughout, complicated by differences in the evidence in the literature. Overall a more nuanced discussion of ENSO over the mid to late Holocene is required and the possibility that there is no link, that a link cannot be determined at this time, or that a link may be indirect needs to be discussed.
We agree that evolution of ENSO over the mid- to late Holocene is a complex issue that has not been fully resolved in the literature. We discuss some of these limitations at the end of section 3, but we have modified the text throughout this section to make the uncertainty in past ENSO more apparent. The reviewer is correct that the majority of the paleoclimate literature suggests that ENSO variability was low in the mid-Holocene and we discuss this clearly in the beginning of section 3. The literature also suggests, however, that ENSO variability increased after this time, which we hypothesized provided the trigger for reef shutdown in the eastern Pacific. To acknowledge that our schematic (Fig. 3) cannot fully represent these complexities, we added the following qualifying statement before our discussion of how ENSO may relate to the shutdown of reef development in Pacific Panama:
The schematic provides a simplified summary of the changes in ENSO suggested by our study and the paleoclimate literature, which could have contributed to the shutdown of eastern Pacific reef development and is not meant to provide a comprehensive review of the literature.

As suggested, we have added a reference to the new record from Bayes Inlet:
At least one record from the western Pacific (Vanuatu) indicates that a multi-decadal period of enhanced ENSO variability followed just 500 years after the putative low in ENSO activity at ~4.2 ka (Corrège et al., 2000); however, there is less evidence for elevated ENSO variability in a recent contemporaneous record from nearby New Caledonia (Emile-Geay et al., 2016).

We have also added a statement qualifying the Moy record based on new analyses:
…a series of records from the same region suggest a possible peak in the frequency of El Niño events around 3 ka (Sandweiss et al., 2001; Moy et al., 2002); however, recent reanalysis has called this conclusion into question (Emile-Geay and Tingley 2016).

Finally, we have corrected the statement at the beginning of the prospectus (formerly p10, 356-357) to: *"…increased ENSO variability after 4.2 ka"*

4. Other explanations. The mechanistic link between ENSO and reef death is not fully explained. The link in the specific examples for recent intense events points to bleaching or crown of thorns outbreak but this is not a consistent e.g. large events didn't necessarily cause reef death. Overall I would suggest that at this point it is very difficult to conclude that ENSO led to reef death event at 4.2ka. While ENSO can't be ruled out (point 3) the manuscript should go further and discuss alternative explanations for the reef growth hiatus. For example, could changes be related to changes in the Pacific tropical SST gradient that could influence thermocline depth and upwelling strength (e.g. White et al. 2018 and papers therein). Or monsoon driven changes in trade wind strength (which would also affect SST gradients and upwelling). Or what about sea level changes? Other possibilities?
We have added language to Section 3 discussing and excluding other causal explanations, including sea level, tectonics, and bioerosion:
The fact that the the shutdown in reef accretion occurred in both the Gulf of Panamá and the Gulf of Chiriquí excludes upwelling and outbreaks of Acanthaster as drivers: upwelling is weak or absent in the Gulf of Chiriquí, and Acanthaster is absent from the Gulf of Panamá. Other possible factors, including changes in relative sea level, tectonics, and bioerosion, cannot explain the observed patterns either (Toth et al., 2012, 2015a).

5. Reef accretion changes elsewhere in the Pacific. p11, 360-370 and Table 1 The discussion of global reef perturbation events misses the important paper of Dechnik et al. (2018, recent but also earlier Dechnik papers) and related studies. The results of Dechnik et al. (2018) should be included since there is a potential for ENSO impacts via the teleconnection to the GBR. But really, most of the 'Prospectus' section is speculative and the manuscript would be better to consider a wider range of drivers for reef perturbation events (similar to the approach of Dechnik et al. 2018). Indeed, as I pointed out above, after devoting much of the manuscript to trying to attribute the Panamá reef growth hiatus to ENSO the second last paragraph of the 'Prospectus' only briefly discusses that there is strong evidence from the central Pacific that there has been very little ENSO variations over the mid to late Holocene. A broader discussion of reef accretion hiatuses at other locations would be more balanced.
This is a good point. We have adopted this suggestion, while remaining within the scope of the manuscript, by adding to the Prospectus language and references, including papers by Dechnik and Webster about the GBR.

6. p3, 75 The citations for the final sentence imply that these publications were the foundation papers for the use of corals to reconstruct past environmental and climate conditions. This is not the case and the citations should be revised.
We did not mean to imply that these were foundational or review papers, but rather examples of environmental reconstructions from coral reefs. We have added *"e.g.,"* ***before the references to more clearly reflect this.***

7. p3, 80 ENSO impacts are not necessarily felt most keenly by marine ecosystems. Drought, for example, can have equally devastating impacts on terrestrial ecosystems.
We have added *"often"* ***before*** *"felt most keenly"* ***to qualify this statement.***

8. p3, 100 Why and how did Wood et al. (2016) "cast considerable doubt on Richmond's proposed oceanographic teleconnection between the central and eastern Pacific".
The doubt was based on oceanographic modeling. We have clarified this point and added a second reference in support of the statement. The sentence now reads:
Oceanographic-modeling exercises, however, Wood et al. (2016), however, cast considerable doubt on Richmond's proposed oceanographic teleconnection between the central and eastern Pacific, at least so far as Pocillopora *is concerned (Wood et al., 2016; Romero-Torres et al., 2018).*

9. p4, 115 Not clear how this paragraph ties into the discussion in section 2.1. Please clarify.
This comment is spot-on. We have removed the paragraph from section 2.1 and placed the text where it fits better, in sections 2.2 and 2.3.

10. p4, 125 Confusing. Panama mortality levels are described as "intermediate" but given the percent mortality quoted I would have thought the mortality levels were high.
We changed this to *"somewhat lower"***.**

Also, why is the upwelling/non-upwelling state of the Gulf of Panamá and Gulf of Chiriquí important if the bleaching was due to thermal anomalies associated with the 1982/1983 El Niño. Perhaps further discussion of upwelling impacts is warranted (along with major points above).
We introduce the upwelling regimes of the Gulfs here so that we can discuss how upwelling has interacted with El Niño in the past. Whereas upwelling was suppressed by the El Niño event in 1982–83, upwelling during the 1997–98 event buffered reefs in the Gulf of Panamá from thermal anomalies. We have added the following sentence to the end of the section about the impacts of the 1982–83 event to clarify this point:
The similarity in the level of mortality throughout Pacific Panamá reflects the fact that El Niño suppressed seasonal upwelling in the Gulf of Panama in 1982–83 so the level of warming was similar in both Gulfs (Glynn et al., 2001).

11. p5, 145 states that the 1997/1998 and 1982/1983 El Niño events were "enhanced by global warming" but has this really been established? At the very least citations need to be given to justify this statement.
The sentence now reads: *"Both events may have been enhanced by global warming…"* ***and we have added a reference (Hughes et al. 2018) to support this idea.***

12. p5, general comment - a map of the location of the various islands and Gulfs discuss in the text would be useful.
Location maps are provided in several previous publications (Toth et al. 2012, 2015a, 2015b, 2017), and we would prefer not to duplicate those maps here. We did include a reference to where a regional map showing the locations of eastern Pacific reefs can be found at the beginning of the section in question:
(see Fig. 6.1 in Toth et al., 2017 for a map of locations where coral reefs occur)

13. p5, 225-226 The "earlier model of Glynn (2000)" should be explained.
This sentence now reads:
The tempo and causality of reef development in Pacific Panamá represent a vastly scaled-up version of the earlier model of Glynn and Colgan (1992; 2000), which suggested that poor reef development was a consequence of decadal-scale disturbance by El Niño.

14. p7, 229-231 The modelling papers cited here are not evidence of mid-Holocene low ENSO variability. Remove. Tudhope et al. 2001 should be cited.
We have removed the Clement et al. 1999 reference, which relates ENSO variability to millennial-scale changes in insolation, but does not directly speak to changes around the mid-Holocene. The Liu et al. 2014 modeling reference does, however support the idea of relatively low ENSO variability during the mid-Holocene (with variability increasing to present), so we have retained this reference. We also added a citation another modeling study (Zheng et al. 2008), which directly evaluated ENSO variability at 6 ka and we added the Tudhope reference, as suggested. Finally, we qualified the sentence to say that the mid-Holocene was *"likely"* ***a period of low ENSO variability.***

15. p7, 235 I would be wary of citing Leonard et al. (2016) here as an ENSO signal because records from the GBR reflect the teleconnection between ENSO and the climate of the GBR, not ENSO variability itself.
Leonard et al. 2016 interpret their record as being reflective of ENSO variability; however, it is true that all paleoclimate reconstructions of ENSO are overprinted by local climate variability. We have, therefore, changed the language here to say *"some studies have suggested that there was exceptionally low ENSO variability between 4.3 and 4.2 ka."*

16. p7, 239-241 This sentence refers to the 1997/1998 El Niño leading to "widespread coral mortality", however this seems to contradict statements in the paragraph beginning on p5 149.
The sentence on p7 now reads:
In Pacific Panamá, the elevated water temperatures and high irradiance (low cloud cover) associated with the strong El Niño events in 1982–83 and 1997–98 caused widespread coral bleaching, which was associated with mass coral mortality in the earlier event (Glynn et al., 2001).

17. Figure 4 appears to have the wrong label for the upper right map (should be 2015-16?).
We have corrected this typo.