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Climate of the Past
Discussions

Interactive comment on "The 4.2-ka event, ENSO, and coral-reef development" *by* Lauren T. Toth and Richard B. Aronson

Anonymous Referee #2

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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. 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). 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 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.

Major points expanding on, and in addition to, those described above

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.

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.

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.

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.

Also in Figure 2, the x-axis break is misleading because some of the Sr/Ca and d18O data overlap with the axis break âĂŤ the data should be plotted on the full x-axis.

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.

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?

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.

Minor points

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.

p3, 80 ENSO impacts are not necessarily felt most keenly by marine ecosystems. Drought, for example, can have equally devastating impacts on terrestrial ecosystems.

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”.

p4, 115 Not clear how this paragraph ties into the discussion in section 2.1. Please clarify.

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. 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).

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.

p5, general comment - a map of the location of the various islands and Gulfs discuss in the text would be useful.

p5, 225-226 The “earlier model of Glynn (2000)” should be explained.

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.

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.

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.

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

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