

Interactive comment on “Natural ocean acidification at Papagayo upwelling system (North Pacific Costa Rica): implications for reef development” by Celeste Sánchez-Noguera et al.

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We appreciate all comments and suggestions from anonymous referees. In the following paragraphs we listed our response to the comments from Anonymous Referee #1:

[RC1] General comments: This manuscript describes the intra-annual variability in carbonate chemistry in a naturally acidified environment in Pacific Costa Rica. As the authors point out, such natural laboratories are very important to improving our understanding of coral-reef development under future climate change. The manuscript is generally clear and well-written and the implications of their findings to the future of

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coral growth and reef accretion are significant. It is, therefore, my suggestion that the manuscript be accepted to Biogeosciences after some, generally minor, revisions, as described below:

First, I think it's important that the authors make it clear at the beginning of the manuscript (Abstract and Methods) that they did not measure coral growth in this study that they used previously published data to evaluate the relationships between carbonate chemistry and coral growth, because this was not obvious to me until the Discussion. I would also say what studies you derived the ecological data from in the Methods and include a brief description of the methodologies those researchers used so that readers can evaluate those data.

REPLY: We will make this point clear in the first sections of the manuscript (Abstract and Methods). Also will include in the Methods section the corresponding references of studies from which we took the coral growth rates.

[RC1] The authors should also consider adding some additional text in the Methods/Discussion regarding the collection and interpretation of the carbonate-chemistry data. I've listed a number of specific comments below, but my most significant concern is with the statement that DIC is temperature-independent. Temperature impacts the solubility of CO₂ and directly impacts DIC. Additionally, since the DIC data are derived from pCO₂ measurements, which are dependent on T, this does not get rid of the temperature effect on pCO₂. I would like to see the author address this issue more thoroughly.

REPLY: What we wanted to estimated was to which extend the observed changes of pCO₂ were caused by changes in temperature and/or DIC concentrations. We will express this more clearly.

Specific comments:

[RC1] P2, L25: But see Toth et al. 2012. Accretion rates in Panama when reefs

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were growing were comparable to rates observed on reefs in the Caribbean and the authors did not find a significant difference in reef accretion between upwelling and non-upwelling sites. I would suggest making the language here more conservative.

REPLY: We will modify the sentence as follow:

Original: "...resulting in poorly cemented coral reefs with low accretion rates that are subject to rapid bioerosion"

Modified: "...and have the potential to produce poorly cemented coral reefs with low accretion rates that are subject to rapid bioerosion"

[RC1] P3, L23: Please provide information on the pH scale used as suggested by the OA best practices: <http://oceanrep.geomar.de/8471/1/Guide%20best%20practices%202011.pdf>

REPLY The pH scale will be included => total hydrogen ion scale

[RC1] P3, L28: How can you be sure that the carbonate chemistry at 30 cm was the same as where the instrument was (1.5 m)?

REPLY: We can't. But the samples were taken 30 cm below the surface, which is 1.2 m above the instrument and up to 7.7 m above the bottom (the distance above bottom varied with tide cycles). Since we have never seen any indication of depth related changes (e.g. in turbidity) we assume that DIC and TA changes within the water column are negligible.

[RC1] P4, L8: Did Rixen et al. 2012 use the same methodology for measuring carbonate chemistry? I think it's important to include this information so that readers can fully evaluate the results.

REPLY: The following sentence will be included: "In 2009 xCO₂ was measured by an underway pCO₂ system (SUNDANS) equipped with an infrared gas analyzer (LI-7000), and pH was measured using an Orion ROSS electrode and an Orion StarTM."

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[RC1] P4, L11-12: I would suggest including the statement "All GLM assumptions were met" here instead of in the Results.

REPLY: Will be included as follow: "The GLM was evaluated using graphical methods to identify violations of assumptions of homogeneity of variance and normality of residuals. All GLM assumptions were met."

[RC1] P4, L19: Please include a citation for the rain-ratio (Archer and Maier-Reimer 1994?) and describe what it is

REPLY: The term rain-ratio seems to be confusing since it actually describes the ratio between the export of organic carbon and calcium carbonate carbon. Accordingly, it will be changed as follows: "ROI describes the ratio between the production of organic carbon (POC) and precipitation of calcium carbonate carbon (PIC) (Archer and Maier-Reimer, 1994), and was used to link Δ POC to Δ PIC (ROI=POC/PIC) (Eq. 2, 3)."

[RC1] P4, L23: What are the ROI values of -2.6 and 0.8 based on. Please explain in the text where these numbers came from.

REPLY: These numbers were used to run the model in order to obtain the best fit between measured and calculated pH and pCO₂ values. The assumption of calcium carbonate dissolution caused the negative sign.

Modified sentence will read as follow: "To verify the results from the model, we used the output Δ DIC and Δ TA to calculate new pCO₂ and pH values, which were further compared to the measured ones. The best fit between modeled and measured values was achieved with a respective ROI of -2.6 for 2012 and 1.0 for 2013, whereas the assumption of calcium carbonate dissolution caused the negative sign."

[RC1] P5, L16-17: Temperature variability also can impact DIC. Also since these data are derived from pCO₂ measurements, which are dependent on T, this does not get rid of the temperature effect. I would like to see the author address this issue more thoroughly.

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REPLY: What we meant to say was that we tried to estimate to which extends temperature and/or changes in the DIC concentrations had an effect on the observed variations of pCO₂. This will be clarified.

[RC1] P5, L22: include error term for the aragonite saturation state

REPLY: Standard deviation (SD) will be included as follows: 3.06 ± 0.49

[RC1] P6, L5-7: It would be more informative to talk about how much DIC dropped during the "upwelling-like" event in 2012 than to talk about the overall average for the whole sampling period.

REPLY: DIC, TA and pCO₂ increased during the "upwelling-like" event. The only parameter that decreased during this period was pH. We will include one sentence describing in more detail the behavior of pH (in a similar way as how was done for pCO₂). "Similarly, this increase in pCO₂ was accompanied by a dropped in pH from 8.04 to 7.83"

[RC1] P6, L16: You can't really make conclusions about how regular these sorts of events are based on a few weeks of data from just two years. I would change the wording here to reflect this.

REPLY: We propose the following modification: Original: "The absence of such a cold-event during the non-upwelling season in 2013 shows that it is an irregular feature. . ."

Modified: "The absence of such a cold-event during the non-upwelling season in 2013 suggests that the occurrence of this kind of events might be an irregular feature. . ."

[RC1] P6, L26: The wording of this sentence is confusing. I would rephrase to something like: Although the pCO₂ cycles in 2013 followed a similar pattern to 2012, pH was more variable (or less predictable). I would also move this sentence to the end of the previous paragraph and start the next paragraph with "To characterize..."

REPLY: 1) It will read as follows: "Although the pCO₂ cycles in 2013 followed a similar

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pattern to 2012, pH cycles were less predictable." 2) Sentence will be moved to the end of the previous paragraph and the next paragraph will start with "To characterize. . ."

[RC1] P7, L3: I don't understand what this sentence means. Which cycles? Could not be observed in the data? In the model? I actually think that this whole paragraph needs to be fleshed out more. It's not clear to me what the authors are trying to say.

REPLY: The first sentence is certainly confusing, and after a careful reading we concluded that it has no relevance in transmitting the take-home message, which basically is: pH and pCO₂ show pronounced daily cycles but the interaction of metabolic processes difficult to identify a clear pattern in the daily cycles of DIC and TA.

To improve reader's understanding this first sentence of that paragraph will be removed and the remaining sentence will be modified and merged with the next paragraph. After these changes the resulting paragraph will read as follows:

"The interplay of all the metabolic processes (photosynthesis, respiration, calcification and dissolution) seems to be softening the daily cycles of DIC and TA. However, the daily patterns of the DIC/TA ratio raise some clues about the processes that are controlling the daily cycles of pH and pCO₂ (Fig. 4). To identify the dominant processes, we developed a numerical model and recalculated the daily trends of pH and pCO₂ (Fig. 5)."

[RC1] P7, L10: add a reference to Fig. 5c&d at the end of the sentence ending with "respiration"

REPLY: It will read as follows: "...during the day and carbonate dissolution was relegated to night hours alongside respiration (Fig. 5c, d)".

[RC1] P7, L10: Clarify whether you are describing changes that just happened during the cold-water event or during the entire sampling period in 2012.

REPLY: We are describing the changes during the cold-water event (7 days), not during the full period 2012 (15 days). For a better understanding, the sentence will be modified

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in the following way: “Based on the observed daily pH and pCO₂ cycles and in line with our model results, it seems that during the 2012 cold-water intrusion event (June 10-17), the diurnal cycle of the carbonate system was dominated by photosynthesis and...”

[RC1] P7, L30-31: This sentence is misleading. I think that what you mean to say is that saturation state in each of those locations predicts extension rate. I'm assuming that you used the saturation states/linear extension determined by Manzello 2010 for Panama and Galapagos, but this is not clear in either the text or the figure.

REPLY: The work by Manzello was cited in the text and in Fig. 6. However, we will modify it to make it clearer that we used those values as input for our figure.

Writing in text will be improved in the following way:

“Aragonite saturation state (Ω_a) is known as one of the main variables influencing coral growth and therefore reef distribution around the world (Kleypas et al. 1999). By integrating the data from the present study and values previously reported by Rixen et al. (2012), we estimated that the annual mean Ω_a in Bahía Culebra is 3.06. Additionally, earlier studies in the ETP measured Ω_a values and coral extension rates from locations that are under the influence of upwelling events (Manzello 2010a), whilst extension rates from Bahía Culebra were measured by Jiménez and Cortés (2003). The correlation between our estimated Ω_a with the available data from Bahía Culebra, Panamá and Galápagos indicates that coral extension rates in each of those locations are predicted by their corresponding Ω_a values (Fig. 6).”

Figure caption will be modified as follow: “Mean aragonite saturation states (Ω_a) - from present and former studies - versus previously reported mean linear extension rates of *Pocillopora damicornis* and *Pavona clavus* from upwelling areas in Costa Rica (CR) (Jiménez and Cortés, 2003), Panamá (PAN) and Galápagos (GAL) (Manzello, 2010a). Red broken line shows the regression equation as estimated by Rixen et al. (2012). Red mark represents our estimated Ω_a threshold for Bahía Culebra, when coral growth equals zero”

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[RC1] P8, L11-12: I think it would be good to compare your observation of reef thickness with reef thickness elsewhere in Costa Rica or elsewhere in the ETP, as most reader won't be familiar with how thick a reef "should" be see:

REPLY: P8 L10-11 will be modified as follows: “Despite the corals' high annual mean linear extension rates, during the period of our observation the reef frameworks of *Pocillopora* spp in Bahía Culebra hardly exceeded a thickness of 0.5 m.”

The following sentence will be included in P8, L12:

“In the Pacific coast of Costa Rica, the maximum levels of Holocene framework accumulation range from < 3 to 9 m in *Pocillopora*-dominated reefs and from 3 m to 11 m in reefs built by massive species, with thinner frameworks in upwelling-influenced locations (Toth et al., 2017).”

[RC1] P8, L14: It's not correct to talk about “coral” accretion because accretion is a net measure of the growth of a reef. I would re-phrase the beginning of this sentence to something like: Gaps in coral-reef accretion. I would also specify that these gaps are known from the geological record of the ETP. They are not common elsewhere.

REPLY: Our sentence will be modified:

Original: “Coral's accretional gaps, which are known from the geological records, document the vulnerability of these reefs. They have been...”

Modified: “Gaps in coral reef accretion at the ETP are known from the geological record (Toth et al. 2017). They have been...”

[RC1] P8, L20: It's likely that OA will threaten places like Bahia Culebra sooner than elsewhere right? It might be interesting to include a brief discussion of when/how OA may impact reefs in the ETP vs elsewhere and what the implications of this would be.

REPLY: We will include the following sentences just before the last sentence of the current paragraph:

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“Worldwide, OA is expected to reduce coral reefs’ resilience by decreasing calcification and increasing dissolution and bioerosion (Kleypas et al., 1999; Yates and Halley, 2006a; Anthony et al., 2011). Coral reefs from the ETP are affected by chronic and acute disturbances, such as thermal stress and natural acidification resulting from ENSO and upwelling events, respectively (Manzello et al., 2008; Manzello, 2010b). Historically, these reefs have shown a high resilience to both stressors by separately but their coupled interaction can cause coral reef lost within the next decades. The ETP have the lowest Ω_a of the tropics, near to the threshold values for coral reef distribution, therefore the reefs from this region may be the most affected by the increasing levels of anthropogenic CO₂ and also show the first negative impacts of this human induced OA (Manzello et al., 2017).”

[RC1] P8, L31: I would make it clear here that previous studies have shown that reef accretion is low in Bahia Culebra.

REPLY: We have modified the sentence in section 4.3 (P8, L10-11) to clarify this point; therefore we consider that this statement can be removed from the “Conclusions” section. Sentences spanning lines P8, L28 to P9, L1 will be modified as follow:

Original sentence: “Rising levels of Ω_a enhance coral growth during the non-upwelling season due to which the linear extension rates of the main reef building corals in Bahía Culebra were among the highest in the ETP; however, reef accretion was low due to erosion”.

Modified sentence: “Previous studies reported that the linear extension rates measured in Bahía Culebra were among the highest in the ETP, thus is likely that coral growth in this bay is enhanced with increased Ω_a during periods with no entrainment of low-pH waters. However, coral growth must be measured during both seasons in order to confirm this assumption. Threshold values of Ω_a when coral growth likely approaches zero were derived from the...”

[RC1] Technical corrections REPLY: All technical corrections will be included

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