

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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Received and published: 29 January 2018

We appreciate all comments and suggestions from anonymous referees. In the following paragraphs we listed our response to the comments from Anonymous Referee #2:

Overall

[RC2] The authors present interesting time-series data of pH and pCO₂ from an upwelling region of Pacific Costa Rica. These data are much needed and this contributes to our limited understanding of CO₂ dynamics in the eastern Pacific. I do believe there

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is a publication nested within this draft, but there will need to be a significant cutting down and revision based on my comments below. The paper will be considerably shortened and changed slightly in scope, but still is publishable.

Big comments

[RC2] 1) The calculation of TA and DIC from pH and pCO₂ is unreliable and prone to large errors (see Cullison-Gray et al. 2011; Gray et al. 2011, which you cite). This is probably why you are reporting such unbelievably high values of TA (>2600) and DIC (>2300). Not only do these values never occur on coral reefs (to my knowledge), even in strong upwelling zones like the ETP, you are also reporting relatively low salinities of 32.5. This makes these high values all the more suspect because if you calculate salinity normalized for a TA = 2715, you get NTA= 2923! These types of DIC and TA values are unheard of from reef environments. As such, you need to remove all DIC and TA values that were calculated from pH and pCO₂, including the model you generated from some of those values to interpret rates of metabolism driving diurnal changes. Unfortunately, this all has to be deleted from the paper.

REPLY: We understand the comment as follows: Calculations of TA and DIC are prone to errors. Our calculation of unrealistic high TA and DIC values proves this and consequently all calculated data including the model have to be erased from the ms.

First of all we agree to the first statement of Referee #2, that this hardly affects the scope of the paper, which would still be publishable. We appreciate this comment very much.

In the ms we will explain that calculation of TA and DIC from pH and pCO₂ are prone to errors and that we will therefore focus on the pH and pCO₂ values.

However, the model was produced to explain the observed changes in pH and pCO₂ and to untangle the effects of interacting processes on the pCO₂ and pH. For example, as mentioned by the reviewer (see comments below) that photosynthesis explains a

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drop of pCO₂ and increase of pH is correct, but the model also shows that the effect on the pH is insufficient to explain the observed increase of pH in 2013. As indicated by the model, changes in pCO₂ and pH can be explained only if carbonate dissolution is considered in addition to photosynthesis. The TA (2120 – 2310) and DIC (1800 – 2040) values used in the model are well within the range reported in other studies (Manzello 2010, Table 1) and far below the extremes, which raised the concern of the reviewer.

In contrast to the pCO₂ and the pH, TA and DIC values, which were calculated from the pCO₂ and pH, do not reveal a diurnal cycle as the one in the model. This could be a consequence of the errors, which according to results from other studies could range between 16 (Millero 2007, Table 9) and 250 $\mu\text{mol} / \text{kg}$ (Cullison Gray et al. 2011). We will point this out in the ms.

However, the mean DIC and TA values of about 1800 +/- 200 $\mu\text{mol}/\text{kg}$ and 2100 /- 250 $\mu\text{mol} / \text{kg}$, which were calculated from the pCO₂ and pH, are well within the range of those reported from other studies (Manzello 2010, Cyronak et al. 2013) and the one used in the model. This fitting within the range implies, furthermore, that the TA and DIC values used in our model were acceptable.

References: Cullison Gray, S.E., DeGrandpre, M.D., Moore, T.S., Martz, T.R., Friederich, G.E., Johnson, K.S., 2011. Applications of in situ pH measurements for inorganic carbon calculations. *Marine Chemistry*, 125, 82-90.

Cyronak, T., Santos, I.R., Erler, V.D., Eyre, B.D. 2013. Groundwater and porewater as major sources of alkalinity to a fringing reef lagoon (Muri Lagoon, Cook Islands). *Biogeosciences*, 10: 2467-2480.

Manzello, D. 2010. Ocean acidification hot spots: spatiotemporal dynamics of the seawater CO₂ system of eastern Pacific coral reefs. *Limnol. Oceanogr.* 55 (1): 239-248.

Millero, F.J. 2007. The marine inorganic carbon cycle. *Chem. Rev.* 107: 308-341.

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[RC2] 2) Please be clear as to what you actually measured versus what you hypothesize, or think is going on. For instance, in the abstract you discuss how calcification is enhanced during the non-upwelling season when saturation states are elevated. This may be true, but at this point it is a hypothesis as you have no data to support it. There are several other instances where hypotheses are discussed as fact, i.e., reef thickness and accretion etc.

REPLY: Writing will be modified through the text, to clarify which variables were measured in the present study and which values were taken from the literature.

[RC2] 3) What is the proximity of the instruments to an actual coral reef? You need to be clear about this because what you are measuring may actually be a result of the metabolism of the carbonate benthos directly under the dock plus the water column processes rather than anything to do with reef dynamics.

REPLY: The instruments were deployed approximately 200 m east of a small *Pocillopora* spp. patch reef. However, when compared to reefs in the western Pacific Ocean, the reefs in the ETP (e.g. in Bahía Culebra) are poorly developed. Our intention was to characterize the carbonate chemistry within the bay (Bahía Culebra) and its impact on the reefs. Our results indicate that physical oceanographic processes, such as upwelling and exchange between the bay and the open ocean waters, influence the carbonate chemistry on timescales of weeks to months, where the metabolic processes (photosynthesis and calcifications) influence the diurnal cycle. To which extend benthic and pelagic processes control the diurnal cycle, could not be studied based on our data. This will be clarified in the ms.

[RC2] 4) Finally, the paper is generally well written, but would benefit from being proof-read by a native English speaker.

REPLY: Thank you. We will take your suggestion into account.

Specific comments

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[RC2] P2, line 19: You can't say adaptation without genetics work. Reword to say "adaptation or acclimatization"

REPLY: Will be changed.

[RC2] P2, last line: Thus far there is really no data out there suggesting that OA will directly lead to coral mortality. In fact, in a seminal study, Fine and Tchernov (2007, Science) exposed two coral species to undersaturation and found they stopped growing skeletons and lived in an anemone-like state. Once conditions were raised above saturation, they began producing skeletons again.

REPLY: Accordingly, we will change "coral survival" for "coral reef development within this bay".

[RC2] P3, section 2.2: You discuss taking discrete samples, but do not say how many or how often. Please mention this. The values of TA >2600 and DIC > 2300 have to be calculated from pH-pCO₂. You show the result of 8 bottle samples pH relative to the SAMI pH – what about the SAMI CO₂ or any other data calculated from the bottles?

REPLY: We could include an additional panel in Fig. 3, showing the regression between SAMI-pCO₂ and calculated pCO₂ from measured TA and DIC.

[RC2] P5: salinity is a unitless value. PSU describes the scale and is not a unit

REPLY: Accordingly, the term "psu" will be removed from the text.

[RC2] P5, line 5: Need to say what the number after the plus and minus is the first time you use it.

REPLY: The following will be included after the 29.61 ± 0.93 : (average \pm standard deviation)

[RC2] P7, 1st line: why couldn't stimulation of photosynthesis from higher nutrients during upwelling be causing the large amplitude? You can't speak to dissolution without actual TA data and increased photosynthesis seems more likely.

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REPLY: See first comment: In principle yes. But even if we would assume that photosynthesis caused the measured decrease of pCO₂, the associated impact on the pH would be insufficient to explain the observed increase in pH. The assumption of carbonate dissolution is plausible and would solve this discrepancy. Therefore, we produced the model to test such hypotheses.

[RC2] P7, line 25: Say adapted and or acclimatized.

REPLY: “and/or acclimatized” will be added.

[RC2] P7, last line: Reword to say saturation state is one of the controlling factors in coral growth. Its likely of secondary or tertiary level importance behind temperature and light.

REPLY: Taking into account your observations and the comments from another reviewer, the full sentence and paragraph will be modified 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).”

[RC2] P8: delete sentence spanning lines 3 to 5 as well as last sentence of 1st paragraph

REPLY: We will consider removing the sentence spanning lines 3 to 5. However, we consider that the last sentence of the same paragraph is important, as point out that these coral reefs are also threatened by additional stressors resulting from human activities.

[RC2] P8, line 10: Did you measure reef thickness? Also, I'd avoid saying they are

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growing on the edge of their ecological tolerance. Its not clear what is meant by that.

REPLY: Reef thickness was visually estimated during numerous dives. We have been working in the study area for more than two decades.

Regarding the second comment, the sentence will be modified: “These reefs are growing in an environment at the limit of reef-building corals tolerance in terms of temperature, nutrient loads and pH (Manzello et al., 2017)”

[RC2] P8, last 5 or so sentences: a lot of conjecture written as fact.

REPLY: Several modifications will be included, in order to reduce conjectures and improve the text:

Original: “Challenging conditions are not restricted to the upwelling season, they occur sporadically also during non-upwelling seasons, when pH and CO₂ concentrations reach values comparable to those during upwelling events. Linear extension rates of the main reef building corals in the bay are sensitive to changes in Ω_a , suggesting that upwelling reduces coral growth by introducing acidic subsurface waters in the surface layers. 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 accretions was low due to erosion. The latter indicates a sensitivity of coral reefs to the intensity of the upwelling and other processes occurring during the non-upwelling/rainy season, such as human impacts on river discharges, the occurrence of cold events (e.g. 2012), and ultimately to OA. Threshold values of Ω_a when coral growth likely approaches zero were derived from the correlation of Ω_a and linear extension rates and this suggests that OA will seriously threat reefs in Bahía Culebra, which are already at the verge of their ecological tolerance.”

Modified: “Challenging conditions for reef development are not restricted to the upwelling season, they occur sporadically also during non-upwelling season, when pH

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and CO₂ concentrations reach values comparable to those during upwelling events. 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 supposition. Threshold values of Ω_a when coral growth likely approaches zero were derived from the correlation of Ω_a and previously measured annual linear extension rates. The Ω_a threshold values from the present study and discovery that high-CO₂ waters are occasionally hauled into the bay during non-upwelling season; suggest that coral reef development in Bahía Culebra is potentially threatened by anthropogenic OA.”

[RC2] Figure 4: your units for pCO₂ are incorrect

REPLY: They were also incorrect in figure 5. Both figures will be corrected.

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2017-459>, 2017.

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