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Interactive comment on "Assessment of paleo-ocean pH records from boron isotope ratio in the Pacific and Atlantic ocean corals: Role of anthropogenic CO_2 forcing and oceanographic factors to pH variability" *by* Mohd Tarique and Waliur Rahaman

Anonymous Referee #1

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This paper does not present newly generated d11B data. Instead, the authors compile all paleo-pH reconstructions from d11B in tropical corals published so far (10 records), and run several statistical and time-series analyses on them to draw a series of conclusions regarding the different factors controlling pH variability. A main target of this study is to differentiate the contribution of the uptake of anthropogenic CO2 in the recent lowering of pH from that of other climate-oceanographic processes and phenomena such as ENSO and PDO in the Pacific Ocean and NAO and AMO in the Atlantic Ocean. In

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spite of the lack of new d11B data, which would have been desirable, the approach presented sounds interesting, a priori. In my opinion, however, when looking at the whole piece of work, this paper does not meet the requirements to be published in a high standards journal as Biogeosciences.

In the first place, the paper should have been English proofread before submission. Even though the manuscript can be more or less followed pretty OK, the style is too telegraphic, often with a lack of definite articles, for example. In addition to this and, more importantly, some of the important conclusions are either not new or are not fully supported by the data (or the calculations performed by the authors to the published data).

For example, at the end of the abstract and as one of the main conclusions, the authors state: “Considering the model based prediction of the extreme events associated with ENSO in the backdrop of the increasing global warming, coral bleaching events are likely to increase in future decades/centuries.”. However, I do not find in the paper any new evidence lending support to this future increase in coral bleaching events, particularly from the d11B data compilation that the authors present. Looks to me that the authors, just by observing that the increase in pH variability of the last decades may be linked to more extreme ENSO events, infer then that, because bleaching is also associated with strong El Niño events, coral reefs will thus experience more frequent bleaching events. I believe there are already many good references that point towards increased coral bleaching, substantiated by solid modelling. See an update on this in the book “Oppen, M.J.H.v., Lough, J.M., 2018. Coral Bleaching. Patterns, Processes, Causes and Consequences” and references therein.

This conclusion on bleaching comes in fact from the last sections of the discussion, supported by figures 6 and 7, which are very hard to follow. In figure 6a, for example, the PC1 trend is compared to the pH in the Hawaii Ocean Time-series (HOT) but, although the authors state that “Overall, they follow similar trend”, both curves are pretty different. After drawing a scatter plot and separating the data that really co-vary with these that don't, the authors end up highlighting apparent good matches between PC1 derived pHs and the 'theroretical' pH. I really believe that there is too much data manipulation to reach these conclusions.

The manuscript should also be better structured. A 'Methods and materials' section would be desirable, where the authors could include their section 2.1 with Fig 1 and Table 1 and their 'theoretical pH' calculations as described in section 2.2. Then, a 'Results' section should be included, containing figures 2 and 3, but also 4 and 5! This two last figures are now part of the discussion, but they should be treated as 'Results', since they contain most of the data treatments that the authors performed, including spatial correlations, spectral analyses and principal component analyses. The 'Discussion' section could then be illustrated with figures 6 and 7, even though they are hard to follow and I am not sure they are compelling enough to be included in the paper.

A main conclusion of this work is that, from the PCA analysis, atmospheric CO2 explains about 26% of the paleo-pH variability, and ENSO and PDO about 17%. How certain are the authors that PC1 truly represents the atmospheric CO2 effect and PC2 the PDO and ENSO influence? Just by comparing the trends? In line 308, the authors state "The overall trend of the PC1 is consistent with the increasing trend of atmospheric CO2 (Fig. 5b)" but in this figure, only the PC1 record is shown. PC2 is assigned to PDO-ENSO because the periodicities found in spectrum analyses are similar, but I find all this very weak. Regarding the rest (57%) is said to be explained by the "influence of metabolic processes of corals and/or changes in micro-environments within the reefs which are often neglected in interpreting paleo-pH records". What do the authors mean with this? By just comparing the different trends in paleo-pH vs 'therotetical pH', it looks feasible that atmospheric CO2 only exerted a small role (10-20%) in all trends. However, I am not sure it can be stated that PDO-ENSO contributed with 17%. This is going to vary between locations and reefs in the Pacific Ocean, also depending on whether the coral was taken from a reef very exposed to open ocean or more to inner reef waters. I honestly find very hard to extract such conclusions from the study presented here.

In section 2.2, the authors write that “have adopted a novel approach to reconstruct paleo-pH of ocean based on the past records of carbonate system parameters (hereafter, theoretical pH).”. I don’t see the novelty in this approach. This is exactly what other studies have performed, and the basis of the widely cited global decrease of 0.1 units in pH since pre-industrial times, this is, use the data on past atmospheric CO2 concentrations to estimate what would have been the ocean pH if the concentration of CO2 in the surface ocean was in equilibrium with overlying atmosphere CO2. Perhaps the novelty resides in using Sr/Ca, Mg/Ca or d18O from the same corals to determine temperature and salinity, and estimate alkalinity, for a more precise calculation of pH from past CO2 concentrations. Starting in line 208, the authors mention ‘Theoretical pH records show monotonic decreasing trend since $\sim$ 1850 AD which is consistent with that of atmospheric CO2 forcing since the beginning of the industrial era’, but this is not surprising, since these pH calculations are based exactly on this, the CO2 forcing. Later, in line 220, the same is stated ‘This shows that the theoretical pH records overall follow the historic record’ which should not be a surprise. Maybe an interesting calculation would be to subtract the theoretical pH to the d11B reconstructed pH, to eliminate the purely anthropogenic effect from the paleo-pH record. In addition, I would eliminate ‘in CO2SYS program’ in the title of this section, and would not make much emphasis on a specific software, since it is only one of the available useful tools to do this kind of calculations.

Several calculations performed in this paper include spatial correlations of temperature and precipitation against ENSO and PDO in the Pacific, and AMO and NAO in the Atlantic. First, I would be surprised if other studies have not done this already. Second, these comparisons allow to interpret whether in terms of temperature and precipitation variability, certain areas are more or less affected by ENSO and PDO but, what if other parameters related to ENSO and PDO affect coral reefs which are not necessarily translated into an impact on SST and precipitation? Changes in the strength of ocean

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currents, for example, or nutrient content.

Section 3.1, in which the authors describe the major periodicities in all records should clearly go to a 'Results' section. In this regard, the direct attribution of these periodicities to their forcing is too straight forward. See, for example, in line 282: 'The lower periodicities less than 10 years are similar to that of NAO and therefore highlight the role of NAO in modulating ocean pH at annual-decadal scale' or in line 284 'The longer periodicities at decadal scale (~10 years and 25 years) captured in the spectrum analysis are similar to that of AMO and hence indicates its role in modulating ocean pH', and the last sentence 'In conclusion, our analysis clearly indicates that ocean pH is influenced by oceanographic factors modulated by dominant ocean-climate modes i.e. ENSO and PDO in the Pacific and NAO and AMO in the Atlantic oceans'. In my view, the authors are pushing these comparisons too much.

Finally, there is a conclusion, number IV, which I believe is stated for the first time in this manuscript: "We have estimated pH reduction in the Pacific Ocean by 0.5 unit due to anthropogenic CO2 forcing; rise of atmospheric CO2 from 310 to 380 ppm during 1950 – 2004."

Overall, even though the effort to compile all published recent paleo-pH data from tropical corals and study them in deep with different numerical tools seemed valuable, I found this paper very immature to be published and with not new or compelling enough conclusions.

Minor points:

- Line 33: Need to specify that these records are from tropical corals.

- Line 83: 'these predictions are associated with large uncertainties' is misleading. It reads as if we don't understand much about the mechanisms of acidification due to anthropogenic CO2 dissolution in the oceans, while we do understand this process very well. The 'uncertainties' come because we don't know which scenario we will follow. Depending on each scenario, it's very clear which will be the averaged surface ocean pH. To illustrate this, I recommend the authors look at Fig. 3 of Bopp et al 2013. Biogeosciences 10, 6225-6245 and compare uncertainties in pH with the other three global stressors.

- Line 87: 'The longest instrumental records of past pH are available only from two stations' doesn't read well, BATS is longer than HOT (not HOTS), and probably ESTOC should also be cited. A good summary of time-series is Bates et al 2014. Oceanography 27, 126-141.

- Line 213: SSS is a chemical and not a physical parameter.

- Lines 226-230: For a statement like this 'Ocean pH is influenced by multiple factors such as temperature, upwelling, ocean circulation, precipitation, run off, nutrients supply and productivity' there would be many other much more classical references than those that have been cited, which are mostly publications on d11B paleo-pH reconstructions which are interpreted in different ways.

- Figure 4 should be divided, spatial correlations in one figure, and spectral analyses in another one. Otherwise, everything appears very compressed and small.

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