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Discussions

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 #2

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1. The main starting assumption of this study is that variations in 11B boron ratios can be directly related to changes in seawater pH. Since many of these initial studies were undertaken (e.g Pelejero, 2005; Wei 2009) there has been significant advances in our understanding of the various biologic/physiological controls on the pH at which corals calcify. Thus especially for aragonite corals there is now abundant evidence that the calcifying fluid from which coral precipitate their calcium carbonate skeleton is not in direct equilibrium with ambient seawater. As described by Trotter et al., (2011) using

boron isotopic systematics, corals not only elevate their pH relative to seawater but do so with a gradient with respect to seawater pH. 2. From experimental studies for Porites cylindrica, pHSW = (pHCF - 4.72)/0.466 was utilised here by Tarique, but without justification. This equation (by Trotter et al., 2011 based on data from Honisch 2004) is for fixed temperature and is NOT applicable to all corals species as applied in this study. A number of studies have also shown differences in the gradient term. It has also been shown that different coral species (sometimes even of the same species) will have different sensitivities (i.e both in the absolute offset from seawater as well as the relative sensitivity/gradient). Thus the starting assumption assumed here all Porities and even that the species Diploastrea heliopora analysed by Wu et al has the same 'calibration as derived for Porites cylindrica is clearly incorrect. Thus even under ideal conditions sensitivities of samples analysed is expected to be variable. 3. Additionally, it is has also become apparent, especially from seasonally resolved studies (e.g McCulloch et al 2017) that there are significant effects of temperature on ïĄď11B ratios of corals. The magnitude of this variability is significantly larger than the seasonal variations in seawater pH and acts in phase with those changes (ie higher temperature gives lower calcifying fluid pHcf (see e.g McCulloch et al., 2017). Thus ocean warming would be expected to amplify the effects of declining seawater pH due to OA. This may well explain the recent enhanced decline in 11B ratios observed since the 1970's as that generally also coincides with commencement of more pronounced warming. Obviously for comparative studies of the type presented here the effects of differences in regional warming on coral 11B tatios also need to be considered. 4. Correlation and causality: Finding significant interannual or decadal variability in a record doesn't mean ENSO or PDO modulation. For example while rainfall and terrestrial run-off, which influence inshore environments in the GBR, are partially modulated by ENSO, the link with the mid-shelf, Coral Sea and New Caledonia is missing. How other regions are influenced by ENSO, the PDO or NAO is not discussed in the text. Thus the claim "Overall, we observe that all the coral sites in this study come under the influence of ENSO and PDO in the Pacific and AMO and NAO in the Atlantic though the modulation of SST, precipitations, run-off and oceanographic parameters nutrient supply, upwelling and circulations and therefore responsible for changes in ocean pH" is not well justified. 5. The interpretation of the principal component analysis is equally problematic. The first principal component indicates that overall the trend explains 26% of the share variability between records and while this could relate to changes associated to CO2 this doesn't explain if this linked to changes in seawater pH, SST or both. Similarly, finding significant interannual and decadal modes of variability in the PC2 are not enough to relate them ENSO and PDO. Furthermore, the authors included all records from the Pacific in their principal component analysis and made general assumptions about the influence of ENSO and PDO when in reality this highly variable between the different regions in the Pacific. 6. Inconsistencies with the number of records used. Ten records are mentioned in the abstract while 11 are described in figure 1. In Figure 5 there are 10 records presented for the Pacific when elsewhere only 8 records are mentioned for that region. Furthermore Figure 5 includes 3 records from D'Olivo et al., 2015 study while in the rest of the manuscript only 1 record from that study is shown/mentioned. Assuming the composite inshore record was used in Figure 3 and Figure 4 why were the mid-shelf records from D'Olivo et al., 2015 excluded and only 3 out 5 records by that author used for the PC analysis (Figure 5).

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