

Interactive comment on “Coral records of reef-water pH across the central Great Barrier Reef, Australia: assessing the influence of river runoff on inshore reefs” by J. P. D’Olivo et al.

Anonymous Referee #1

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The paper focuses on interpreting the boron isotope records of several corals in terms of environmental pH and tries to link that with coral growth and aragonite saturation state. Boron isotope work in corals is a relatively young field and this paper is a significant application of the tool to improve our understanding of the geochemical changes in seawater of a lagoon-reef complex. The authors have produced a great data set from a network of corals and gathered a substantial amount of ancillary data to help interpret the results. The final conclusions are robust but I have some serious concerns about how the work has been communicated in the current manuscript.

I felt that the authors attempted to do a thorough interpretation of the data but I am concerned that they under emphasized a very important factor. The paper reads as
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if the authors are trying to directly link seawater pH with aragonite saturation state, however the saturation of aragonite is a function of both the Ca concentration and the carbonate ion concentration. The carbonate ion concentration can be influenced by pH, but it is also impacted by total dissolved inorganic carbon (DIC). River inputs to the reef can impact both Ca ion concentration, DIC and pH, but the text seems to ignore all but pH. The authors seem to be good geochemists, so I think this may just be an oversight in the communication of the work, but it should be explicitly and prominently addressed. If there is no data that can be brought to bear on this topic, the many sections of the text that refer to saturation state and pH should be carefully re-written to account for the fact that pH is not the only driver of saturation state and that other variables are likely important in this system.

The analytical work for this data was not trivial and the authors generally did a good job of concisely explaining the work. The only thing I was left wondering was what their recoveries were from the ion exchange resins and how do they know that there is no fractionation if they do not have complete recovery. An important detail in this section and throughout the paper was a lack of consistency with significant figures. For example the $\delta^{11}\text{B}$ value of the JCP carbonate standard (is that Jcp-1 or Jcp-2?) measured by MC-ICP-MS was $24.3 \pm 0.34\%$. It should be reported by the correct $24.3 \pm 0.3\%$ or by $24.30 \pm 0.34\%$ with the extra sig-fig that many geochemists conventionally use.

My impression was that the style of the writing lacked a cohesive narrative that led to a perceived lack of scientific gravitas, though I think hidden in the text are some valid interpretations and some interesting scientific findings. The discussion section explored different aspects of the data, but the important points seemed to me to be lost in the details. For example after reading section 4.1, I was left wondering what the point was except for the basic facts of the mean pH from both rivers and the fact that there was no regular seasonal cycle of pH in the river data. Both facts seem more at home in a Results section. The choice of the Elbe estuary as a comparison seemed random. The reader was left wondering what are the primary differences between the

Elbe River and the Burdekin River watersheds and hydrology.

To remedy the perceived lack of focus and narrative in the writing, I suggest that the authors focus the text on making arguments for the points made in the Summary and Conclusion section and limit the amount of text dedicated to digressions/explorations of the data that don't pan out. I do think it is important to include negative and inconclusive findings, but it is important to keep them from disrupting the narrative of the primary conclusive findings. It is very valid to explore multiple working hypotheses and eliminate some of them in the discussion, but this needs to be done explicitly so that the reader can follow the thought-process of the authors.

I read this paper more for large-scale issues rather than small-scale ones, but I did notice two things: 1) the text describing Figure 6 indicates the units are z-scores, but the units on the graphs are the units of the individual parameters, 2) Table 2, the numbers are described as "Mean annual values" but my understanding is that this is reporting average values from 1973-2000 using annual data. The way it is currently written can be misinterpreted that the data represent the average of a single year.

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