

Interactive comment on “Low Florida coral calcification rates in the Plio-Pleistocene” by T. C. Brachert et al.

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Review 1 We thank this reviewer for his very critical review because we find this the best way of clarifying the intentions of a publication. In the review, we identify 3 problems and 9 more special critical aspects:

General problems: 1. Overlap with our GloPaCha paper (2014) is too strong! Answer: The GloPaCha paper was intended to bringing together all sclerochronological records available from the Pliocene and Pleistocene from Florida and to make comparisons with the recent. This dataset encompasses records from bivalve mollusks and corals which stem from a broad spectrum of environments and, therefore, equally record environmental variability in open, shallow-marine, lagoonal and coastal settings.

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Discussion Paper



No data or interpretations of calcification records have been presented which is the focus of the BG publication. We find it self-explaining that the calcification data must come along with isotope data. The BG paper also includes new records which were not yet available to the GloPaCha publication because we attempt to improve the data base permanently. It must be kept in mind also, that the BG paper is back-upped a companion paper submitted earlier to BG which describes in detail the nature of every individual coral record. We detect no unnecessary overlap or useless multiplication of publications. These three publications have very different profiles instead. 2. Additional evidence needed for the fossils being not altered diagenetically! Answer: We have screened our material in a very careful way because we are aware of the problem of diagenetic alteration, also taking also into consideration subtle aragonite – aragonite transformations. But this is not a matter of “believing” in the evidence presented or not. It should be kept in mind that all screenings must be suitable to be applied on large numbers of specimens and to be performed in a reasonable period of time. This is an approach also applied in the work suggested by the reviewer (Gothman et al, 2015, GCA; Anagnostou et al., 2011, GCA). Tests not performed during our study, were also not performed routinely in those studies, but applied to very few samples only and were found necessary to deal with specimens having high contents of secondary calcite. This is a situation not compatible with our material. Nonetheless, further in depth analyses can be made, and have been made by us, using specimens selected by random. This includes LA-ICP-MS analyses of high spatial resolution (as also suggested by the reviewer/recommended literature). One LA-ICP-MS record has been presented previously (Böcker, 2014) and has not revealed any evidence for alteration. Rather, concentrations of certain elements (e.g. Sr/Ca) are in phase with $\delta^{18}\text{O}$ variability over the entire record and document seasonal SST changes (not necessarily in phase with the density banding) whereas Fe and Mn were not measured. This makes sense, because both elements are typically replacing Ca in calcite precipitated from reducing pore water but not in aragonite and are, therefore, not sensitive for diagenesis and aragonite – aragonite changes. In sum, we found our data

C10161

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12, C10160–C10163,
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[Interactive Discussion](#)

[Discussion Paper](#)



sufficient evidence for our assumption of a rather pristine state of preservation. 3. Data presented suggest environmental conditions for the Florida platform opposite to that reported by other groups! Answer: In an accepted companion publication (Brachert et al., 2016, BG; <http://www.biogeosciences.net/13/1469/2016/>), we present a new model of Florida platform environments during some Pliocene and Pleistocene interglacials. The idea of upwelling playing an important role in controlling carbonate production on the platform is not new, however, and has a long track in the literature. We also remind our readers that the presence of phosphates or abundant conspicuous shark teeth in some units is established evidence of high production on the platform responding likely with upwelling.

Special aspects (numbering follows review): 1. Reviewer not convinced of the state of preservation suitable for this kind of research. Additional information from chemical data is necessary! Answer: We reject this point! See argumentation above! 2. Multi-species datasets are not sufficiently concise (here mixture of data from *Solenastrea*, *Orbicella* and *Porites*) for making inferences on calcification systematics Answer: We agree, that single-species (or genus) data must be the goal! In our study we were material limited due to preservation issues and decided, therefore, to integrate data from different genera. We are clearly aware of the issue as many studies on the recent have shown significant differences in the systematics of calcification between taxa, especially *Porites* and *Orbicella*. Since this aspect in our study represents issues risen in all reviews of our manuscript, we will restrict to *Solenastrea* in our revision but will discuss the significance of the data from *Orbicella* and *Porites* in the context of all data available (there seems to be no difference within our dataset). 3. Genera used not clear. Discussion also involves *Diploria*. Answer: We have only presented data from the three genera listed above (bullet 2). The inclusion of *Diploria* in our discussion comes from the literature and pertains to extension rates only. It should be kept in mind that calcification rates must be inferred from the combined inputs of extension rate and density. Our study presents quantitative density measurements of fossil corals which have never been presented in the literature before. We will check, if our text is not sufficiently

C10162

BGD

12, C10160–C10163,
2016

Interactive
Comment

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Interactive Discussion

Discussion Paper



concise in this discussion. 4. Mention of $\delta^{13}\text{C}$ in methods but not addressed later in the manuscript. Answer: Will be corrected. 5. Stable isotopes: Overlap with GPC paper Answer: No duplication of data. The reasons are given above. 6. Comparisons with recent corals from WA and IP. Why IP? Answer: We use data from the WA and IP for reference, because they are also rather taxon specific: While the IP data are entirely from *Porites* (how it should be, sic!), WA data is from *Orbicella* but many other taxa also. We also want to include the environment of calcification in our reasoning which differs in between the WA and IP (as the reviewer says also). 7. Environment: high pCO_2 or upwelling and low temperatures: How does this fit the literature? Answer: This exactly fits the discussion presented in the BG companion paper cited above and needs not to be repeated here. 8. First sentence from conclusions: “first record of calcification rates from fossil corals” but the authors compare their data to previous studies (page 20533). Answer: This is apparently a misunderstanding because calcification rates derive from the combined effects density and extension rate. Our comparisons with literature data exclusively pertain to extensions rates. Extension rates are long known to be recorded also in recrystallized skeletons, and therefore, many data exist in the literature. We do not make any comparisons with regard to density simply because no data are available in the literature so far. This is the new aspect of our publication and seems to be not sufficiently clear. We will check how this can be improved. 9. Table 2 should make reference to data from the literature. Answer: All the sources for data taken from the literature are given in the text. Table 2 is indeed a mix of own and published data because we found the informations given in the text elsewhere sufficiently transparent for a backtracking of the data sources. Listing all references here poses a severe typesetting problem. 10. Table 3: authors state minimum values to be marked by bold typesets. Answer: Sorry. This formatting went lost during typesetting.

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12, C10160–C10163,
2016

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