Dear editor,

many thanks for your help improving our manuscript! We have taken a big effort in modifying the text according to the suggestions of the reviewers, and this is the reason why it took so long for the re-submission. The re-submitted manuscript has received a completely new structure and many chapters have been completely re-written. In more detail, the changes include:

1. Abstract: Changes made according to review suggestions

2. Introduction:

   Re-organized: The text begins now with an introduction on coral anatomy (simplified) and coral sclerochronology. Thereafter, we discuss the problem of preservation and potential diagenetic changes in corals from the geological record. Lastly, we explain the aims and significance of the study and give a short account on the methods used. The idea behind this new structure was to put the focus correctly on the calcification aspect and not on preservation.

   This chapter also includes a short description of the Plio-Pleistocene interglacials and the Florida platform, the materials and methods (new: LA-ICP-MS methodology).

2. Results and Discussion

   This section now begins with a description of the preservation of the skeletons (2.1) and includes a discussion on their suitability for this study (no significant alteration has taken place). This section also includes a detailed account on previous studies dealing with diagenetic screenings (new: discussion of geochemical data with respect of preservation issues).

   The following chapters (2.2 – 2.6) describe the calcification parameters and discusses the data in the context of corals from present-day Florida and continues with a discussion of the patterns with regard to the entire Western Atlantic and Indo-Pacific. Importantly, the new structure of the text clearly separates the observational data with regard to regional context (Florida, fossil and recent, and the larger spatial scale) and with regard to taxonomical aspects. We also discuss in detail, why we consider a “big picture approach” legitimate for understanding calcification patterns in fossil materials.

   The text ends with a series of paragraphs (chapter 3) on possible mechanisms behind low calcification rates in the fossil corals. This text has undergone less modification from the previous manuscript, but takes into consideration all suggestions by the 3 reviewers. We thank them for their effort!

Because of the far-reaching changes made to the original submitted manuscript, we stopped using a file containing mark-ups of the changes made. The submitted file showing mark-ups is, therefore, does not the preserve the complete history of changes and is an outdated version. For the same reason, it does not make sense to explain all changes made, word by word. We are sure that you will understand. We reduce our comments to the letter sent after the last round of review (attached below).

Yours sincerely

Thomas Brachert

Review 1

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. 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 not the case for an approach suggested to be adopted (Gothman et al, 2015, GCA; Anagnostou et al., 2011, GCA). Both studies list complex screening methods but do not use them systematically either; rather they applied them in a random way to selected samples. Second, we found theses test to be helpful only when dealing with specimens having high contents of secondary calcite. This is a situation not compatible wit 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 $\delta^{18}O$ variability over the entire record and document seasonal SST changes (not necessarily in phase with the density banding). B/Ca is a very sensitive system (not used in those recommended studies) which does not provide any evidence for diagenesis, whereas Fe and Mn were not measured. This makes sense, because the latter two 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 sea water or an active diagenetic system sensu Longman (1980). In sum, we found our data 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 inferring upwelling 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 most authors consider single-species (or genus) data to be the goal for understanding overarching systematics of calcification. We are of the opinion, however, that single-species studies have a number of disadvantages as well and suffer from regional acclimatization effects. While one taxon shows certain calcification patterns in a given SST regime of a given region, the patterns may be same under a different SST regime of another region. This has been shown clearly in the Literature (Carricart-Ganivet, 2004, JEMBE)! For us, this means that environmental variability (within certain limits) which is well within the spectrum of ecological tolerance of a given taxon will be buffered away by acclimatization. If acclimatization is no more possible because certain environmental thresholds have been crossed, the biotic response to environmental variability will change. For understanding calcification records of the geological time in a semi-quantitative way, we consider, therefore, averaged, global datasets more helpful than very regional studies. We have added a new figure to show that our data are rather homogeneous and do not provide any evidence for the three different taxa investigated to have had significantly different calcification systematics. Also, for the sake of material limitation due to preservation issues, we decided, to integrate data from different genera. Since this aspect in our study represents issues risen in all reviews of our manuscript, we will discuss the significance of the data from *Orbicella* and *Porites* in the context of all data available and we also add a new figure showing only the fossil data.

3. Genera used not clear. Discussion also involves [*Pseudo*-]*Diploria*.

Answer: We have only presented data from the three genera listed above (bullet 2). The inclusion of [*Pseudo*-]*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 concise in this discussion.

4. Mention of $\delta^{13}C$ 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 form an environmental gradient and are also rather taxon specific: While the IP data are entirely from *Porites*, WA data is from *Orbicella* and *Pseudodiploria*. 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 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.

Review 2

This review is acknowledging the potential of our study but is rising some doubts on our methodological approach. These doubts concern

- the use of the modern analgoue data from the WA and IP (which are “from an ecological gradient” – we agree) for explaining deep-time data.

Answer: This is not an issue because we consider the recent as a key to the past. Non-analogue situations are quite common in the geological record, but in our study we are dealing with genera that still exist in the modern ocean and in the region of study today. Therefore, we consider to infer a non-analogue situation is taken quite from afar. We agree, nonetheless, the WA and IP to represent an environmental gradient, and this is exactly why we consider the IP worth to be discussed as well.

- the depth of growth of the corals compared.

Answer: It is true that we have no clear constraints on the water depth at the site of growth of the individual specimens. We assume the corals to derive from the same depth window because all *Solenastrea* (and other taxa) have the same growth form. Although this fact gives no quantitative water depth information, we consider the depth window to be the same. The hint on the Bosscher paper (Bosscher, H., 1993, Coral Reefs) is very good.

The calcification changes with depth are important, and we will use it in the discussion, but complicated also by turbidity, as the same author has found out (Bosscher in Schlager, 1991). We inferred density and extension to be strongly linked with variable turbidity; details of the reasoning may be found in http://www.biogeosciences.net/13/1469/2016/ and should not be repeated here. We only say that water depth has likely not changed significantly during the growth of a colony and that, therefore, variable turbidity or SST are substantially more likely drivers of calcification changes. Apparently, our text is not sufficiently clear in this part and we will check if it needs modification.

- the growth strategies differ due to the type of skeletal architecture (porous vs. solid skeleton), and therefore, differ between *Porites* and *Orbicella*, and correspondingly also between related taxa (*Solenastrea*, [*Pseudo*]*diploria*).

Answer: We agree with this concept, but do not find evidence of this aspect in our data. It may represent an artifact of the small numbers of specimens (*Porites* n = 1, *Orbicella* n =2). We will insert a new figure and discuss the problem more clearly.

Review 3

"I fear the authors may have overinterpreted their data".

Answer: We hope we have not overinterpreted our data. We fear this impression may arise because the environmental reconstruction in the manuscript is strongly simplified and summarizes the contents of a companion paper on the environmental constraints of coral growth during the Pliocene and Pleistocene interglacials (http://www.biogeosciences.net/13/1469/2016/).

- Multiple species of corals (incl. [*Psedo*-]*Diploria*) are combined to generate "big picture" means for modern growth rates. The rationale seems to be that taxa differ not significantly from each other. Does this make sense?

Answer: We say "no", because it is true that the different genera may have different calcification systematics. We say "yes" because most modern studies have described calcification within rather localized and small temperature windows and are, therefore, by their nature subject to small-scale acclimatization effects. It has been shown by studies the small-scale acclimatization responses to repeat on a larger geographic scale and temperature window. We argue, that a large-scale view is needed for understanding the big picture of processes beyond small-scale acclimatization patterns. Within the larger context of interregional comparisons and the geological record, the picture is no more linear and *Porites* and *Orbicella* (WA and IP) seem to have more and more trends in common (see also Carricart-Ganivet et al., 2012, PlosOne).

- Biggest concern: Huge mismatch in recent/fossil data with regard to number and composition of taxa.

Answer: We agree, but this is the data available. We will, as explained elsewhere, use more explicitly the data from recent *Solenastrea* and *Orbicella* (which should be compatible) and compare them with our *Solenasatrea* (n = 14), *Orbicella* (n = 2) and *Porites* (n = 1) separately.

- The authors say that no calcification records are available from *Orbicella* of the reef tract although one of the co-authors (Helmle) has published a dataset (Helmle et al., 2011, Nature Communications). There are also other inshore – offshore datasets (Manzello et al., 2015a,b and there is also a comparison of *Porites* and *Orbicellla* from WA and IP (Carricart-Ganivet et al., 2012).

Answer: We are sorry for the confusion. W have re-read the manuscript and found this to be a misconception of the reviewer who seems to confuse our discussions of Florida Bay and the Florida Reef Tract. Nonetheless, we learn that our text seems to need some clarification. We will re-write the relevant passages of the manuscript. In this respect we will also go more explicitly into the effects of effluxes of saline, nutrient-rich lagoonal water ("inimical bank water") on calcification.

- Why are these corals so well preserved? More discussion on this!

Answer: This is a paradox! To our knowledge, the preservation of aragonite corals involves normally an enclosure in an impermeable sediment (e.g. calcareous clay). This is typically the case in "deep-water" sediments and allows for the preservation of azooxanthellate corals (and other biota such as ammonites) in sediments as far back as the Triassic. But the Florida fossils are from more or less porous, unlithified grainy carbonates with variable contents of matrix. We have no clear explanation for this exceptional preservation - suggestions are welcome!

- Acute events of cold stress in Florida – resolved in data? More discussion needed.

Answer: The resolution of the subannually resolved stable isotope series is not high enough as to resolve events of less than two months in duration.

- What is the factor limiting reef development in present-day south Florida and how does this compare with geologic time?

Answer: Limiting factors of reef growth in Florida are manifold. We consider extreme temperatures (events or periods of very cold or hot temperatures), efflux of “inimical” bank waters and nutrients the most important. An overview of this subject is given by Manzello et al (2015: J Exp Mar Biol Ecol). We will improve this kind of information in the introduction paragraphs. The limitations of reef growth have been described in the companion paper cited above.

Specific comments:

1. The paper has been fully re-structured and yxour suggestion has been included in the new concept

2. The number of specimens used has been corrected. In the new manuscript we have omitted the data from the dissertation by Böcker (2014) having no stable isotope record. We have decided to do so, because we reconstructed extension rates from the d18O cycles whereas he was using the density bands. This way, reconstructions are more consistent.

3. Why are bulk densities presented instead of annual data? We decided to use bulk density because this is more compatible with bulk isotope values.

4. This has been checked. We compare Diploria and Porites.

5. This was a terminology problem – has been be modified.

6. “Fossil Porites is an outlier” within the context of the recent Porites and should not be included in the regression.

Answer: This is what we say – fossil Porites does not fit the recent Porites (or is an outlier, as you say) but is inconspicuous as compared to the other fossil z-corals (new Fig. 4).

7. Error bars were be checked. They depend on the data available. Methodologically, we have no standard deviations from all of the data.