Dear Dr. Treude and co- editor of Biogeosciences,

I am happy to submit today a revised version of our manuscript (BG-2015-572). The revised manuscript will be uploaded as usual using the electronic system of manuscript submissions.

On behalf of my co-authors I wish to express my thanks to all of the editorial staff and reviewers for their efforts in increasing the quality of our contribution.

This new round of review has risen a number of important topics again that we have used for preparing the updated version submitted. Below, we give a point-by-point statement of how we have dealt with the reviewer´s comments.

Reviewer´s comments and our response (red)

1. The authors added a reference (Bocker, 2014) to geochemical evidence for sufficient preservation of one sample but not similar evidence for the other samples. The scale of the micrographs in Figure 2 relative to the density bands shown in Figure 3 does not provide strong enough evidence of preservation. I would like to see geochemical evidence for all samples, but defer to the decision of the Editor.

Geochemical data other than those given by Böcker (2014) are not available. We wished there were more data, but but this can simply not be achieved in a reasonable time and without a budget collapse. We needed at least one other PhD study doing nothing else than just performing and evaluating these analyses. In contrast, we feel, that our data represent a sound basis to our conclusions: Radiographs of all corals do not show any evidence for alterations (dissolution, cementation or recrystallization), XRD provides no evidence of calcite (secondary cement), the stable isotope data exhibit a systematic signal for all corals used, and the visual inspection of the skeletons give no evidence as to problems. Available geochemical data (LA-ICP-MS) from one specimen selected at random does not suggest there to be any alteration of the original signal. We consider this sufficient as a test.

The organization of figure 2 has been modified. We have exchanged some of the pictures to give a more comprehensive overview of the preservation of the skeletons. Showing the entire coral or overview of several growth bands is simply not possible at the SEM scale.

2. Stable isotope methods should not be included in this manuscript. As the data are presented in the companion paper, the authors should simply reference that paper in the discussion and not try to include the data from a previous pub as a result in this MS.

The description of the stable isotope methods has been extremely shortened. We refer to the companion paper for details. We have also deleted the paragraphs on the LA-ICP-MS methodology because it has been described previously with sufficient detail by Böcker (2014).

3. I continue to disagree with the authors that this manuscript represents the ‘first record of calcification rates from fossil corals’. Lough & Cooper 2011 do not represent the definitive definition of calcification rate. Perhaps this work represents the first record of calcification rate under this definition. But there are certainly other definitions of that rate widely used in the literature.

We do not understand this comment. With regard to extension rates, it is true that there is a wealth of studies describing growth banding and extensions rates of corals from reefs of Paleozoic and Mesozoic age (e.g. Geister, Insalaco, Kershaw, Stanley…). We also agree that there were early

attempts of describing the “skeletonization” of corals by the Flügel group but all of them are not providing calcification rates. For recent corals, various approaches for describing and quantifying skeletal growth in reef corals have been presented. To account for these works, we refer to the recent review by Pratchett et al. (2015).

4. The authors could be clearer about the novel nature of this manuscript as separate from their BGS companion paper while still including discussion of, for example, high pCO2 vs upwelling and low temps, that were described in that paper.

We state more clearly in the introduction that this paper provides quantitative calcification data (in contrast to the companion papers) and puts the calcification into the context of global patterns. The companion paper is a regional study on some Pliocene and Pleistocene interglacial units of Florida platform.

5. Tables 2 and 3. The authors could indeed clarify the sources of coral data in these two tables without major typesetting issues. Superscripted numbers detailed under each table would sufficiently take care of this.

We provide a list of the data sources below the tables.

We hope you will find our modifications of the manuscript and replies to the reviewer´s comments appropriate as to finally publish our scientific contribution.

Yours sincerely

Thomas Brachert