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Interactive comment on "Precipitation of dolomite from seawater on a Carnian coastal plain (Dolomites, northern Italy): evidence from carbonate petrography and Sr-isotopes" *by* Maximilian Rieder et al.

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Response to Reviewer Dr. Romanek

We are thankful to the Reviewer for his patience to look through our manuscript again. Moreover, we are thankful for now having the opportunity to respond directly to his comments.

With respect to Table 6, presenting the Sr isotope data, we understand that this is somewhat difficult to capture for the reader. The reason for the complexity is the fact

that Sr-extraction procedures were intensively tested. Individual steps were adapted along the way, based on the outcome of the previous step. We decided to show the full dataset for this submission, but we would be happy to provide a simplified table or plot that will be easier to grasp and which will directly correspond to the main text. The complete data table could be provided through an online repository. We we are happy to follow the instructions of the Editor.

Several other issues raised by the reviewer are related to the Sr-isotope analysis. To the comment that only a limited number of samples out of the 39 hand specimen collected in the field were analysed it is to say that several samples were not dolomite or a mixture of dolomite and clay. Upon petrographic inspection 11 samples were selected, which showed pure aphanotopic dolomite. It should be noted that the sole purpose of selecting three dolomite samples and two clay samples for elemental analysis was to test extraction efficiency. It was never intended to provide a full elemental analysis of dolomites through the section. An in-depth discussion in the sedimentological context would immediately raise the criticism that the sample selection was incomplete. We suggest to provide the data in Table 5 through an online repository. Furthermore, TOC and TIC measurements were performed on clay samples, not on the dolomites. As explained in line 208, the goal of these measurements was to select the clay samples with the lowest carbonate content, as a control. Also the data of Table 3 can be provided through an online repository. Since we have no TOC data from the dolomites, a further discussion of the organic role in dolomite formation, as suggested by the reviewer, would be rather speculative.

The reviewer further suggested that we discuss the microbial dolomite formation. This matter is currently rather controversially debated. Our manuscript does not provide much new insight on any microbial influence, nor is our interpretation affected by it. Therefore we prefer not to engage in an elongate discussion on this matter. We agree, however, that the microbial dolomite hypothesis should be briefly mentioned in the introduction and/or the discussion.

With the reviewer's conclusive statement that our study "provides an incremental step, albeit a small one, in our general understanding of dolomite formation" we do not entirely agree. Our study provides more insight into the depositional environment and mechanism in an ancient system. Our work is, hence, of importance from a palaeoenvironmental point of view, which should be valued for a geologically oriented journal as Solid Earth.

Response to anonymous Reviewer

We are thankful to this reviewer for providing extensive comments throughout the manuscript, especially also for correcting the language in the annotated manuscript. We agree with most suggestions and we will be happy to include them in a revision. They clearly help to improve the manuscript.

There are only a few points where we disagree or where we would be grateful for further clarification. Here we briefly discuss these points:

The reviewer finds the methodology description too extensive and suggests that this part be significantly shortened. We agree so far that the description of TOC/TIC and elemental analyses, which are used to test the extraction method, could be reduced and included in the description of the Sr-isotope analysis. Also considerably shortening the manuscript could be achieved by exporting data tables to an online data repository. This would particularly concur with Reviewer Dr. Romanek who also commented that Table 6 is too complex. Table 6 could be provided as a simplified table or plot.

However, we disagree that the description of the Sr-extraction should be removed or referred to the literature. We would like to highlight that the extraction method is to a great part novel and designed for this particular study. It is crucial that contamination (e.g. by clay minerals) is exluded and to make sure the Sr-isotope values are truly measured from the dolomite phase. The precautions in the methodology are highly critical if we want to find a marine signal in dolomites embedded in large amounts of clay. Furthermore, we do not agree that the discussion of the origin of ionic solutions

should be omitted or significantly shortened. The section on the origin of ionic solutions is very well embedded in the study as it leads up to the discussion that dolomite formed from seawater further below. This is the central part of this study as indicated already in the title. Removing this part would severely disrupt the context of the entire study. Furthermore, the Germanic Keuper was shown as a contrasting system, where dolomite forms in a similar setting but entirely disconnected from the sea. Therefore, this part should not be removed. To address the concerns of the reviewer, the authors are nevertheless prepared to go again through the manuscript to screen for possible parts that could be shortened, clarified or simplified.

Comment on homogeneous dolomite beds (Lines 492-493): Homogenization by wave actions is actually observed in many shallow water bodies of a few cm to dm depth. This process is very likely to homogenize the sediment, unlike in laminites showing separate clay and dolomite laminae. Clay fraction dolomite is transported in suspension and thus would not form wave ripples, unless the mud is clumped together as mud clasts.

The reviewer mentions twice that the ooids could have been micritized. However, it is not clear to me how this could be shown, because ooids very often are already micritic. So how could we know if micrite is replaced by micrite?

In lines 511-513 we are essentially saying the same as the reviewer: Ooids may occur in both marine and lacustrine settings. In the present case they are rather marine because in the same bed Megalodon bivalves (not teeth) occur.

Comment to Line 538: On the contrary: lithified sediment cannot be plastically deformed. It would show brittle deformation.

Comment to Line 700: The oxygen isotopes indicate approximately modern sabkha temperatures, even taking into account the effect of evaporation. Therefore, this is not indicating overprint during burial diagenesis (see also Preto et al., 2015).

With all other comments we agree and we will be happy to follow the Reviewer's suggestions.

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