

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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We are thankful to this reviewer for providing extensive comments throughout the 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

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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 excluded 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

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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.

Interactive comment on Solid Earth Discuss., <https://doi.org/10.5194/se-2019-34>, 2019.