

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 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

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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 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,

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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 palaeo-environmental point of view, which should be valued for a geologically oriented journal as *Solid Earth*.

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