

## ***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.***

### **Anonymous Referee #2**

Received and published: 10 April 2019

I have read through the paper by Reider and others entitled: “Precipitation of dolomite from seawater on a Carnian coastal 1 plain (Dolomites, northern Italy): evidence from carbonate petrography and Sr-isotopes”. I find the paper to be an interesting contribution to our understanding of the processes that led to the formation of primary dolomite in the sedimentary rock record and the authors use some innovative methods to prove a primary origin for the dolomite. However, my main criticism about the paper is that it is too long in its current state, and should be shortened. Examples of text that needs to be edited or cut out altogether include: “The authors spend approximately 4 pages

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describing their methods, which could be cut down to at least half that length by referring to similar methods in other papers and describing their methods in less detail. Related to the previous example, the paper contains data (e.g., %TOC, some of the elemental data, etc.) that seems unnecessary to the overall study. I would recommend that the authors carefully go through their manuscript and remove any data that is not deemed essential to their manuscript. The inclusion of 6 tables seems excessive. The number of tables should be reduced to one or two, with the extra tables relegated to a “Supplemental Materials” section. It is not clear to me why the authors include analyses of the Germanic Keuper dolomites (lines 359-371) in this paper. I am not an expert on Sr geochemistry, but it’s not clear to me why the authors spend so much time discussing sequential extractions (lines 425-472). It seems to me that this text could be reduced. Lines 622-638 also seems unnecessary to the paper, as the authors explain one anomalous value from one sample. This value could be explained away in just a few words. Lines 639 – 662: This text seems more pertinent to a geochemical methods paper and does not seem to be needed here. The discussion concerning the origin of Sr is interesting, but again, does not seem pertinent to the paper. Overall, the authors should spend time editing and rewriting the sections dealing with Sr isotopes and the origin of Sr in the dolomite in order to make them shorter, but should still use the Sr isotope results in their paper (these results could be included in the text from lines 757-763). This section is interesting, but much of it seems tangential to the current paper, and should be removed and incorporated into a separate paper.

A second major criticism of the paper is the use of the the term “non-actualistic” when describing the conditions that led to the precipitation of the dolomites. “Non-actualistic” refers to periods when environmental conditions were so different from today that there is no modern analogue. For example, the occurrence of epeiric seas, or the resurgence of microbial carbonates following several mass extinctions. The conditions cited to have led to the growth of the dolomites only require minor modifications to modern models of dolomite precipitation (i.e., the occurrence of clay-rich aquitards preventing the input

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of meteoric waters), and so the authors should use different terminology here.

Overall, I recommend that the paper be accepted with major revisions, and that the editors work with the authors to cut down on the amount of text.

Specific comments:

Mud clasts vs. mudclasts: The authors use both spellings throughout the manuscript. The authors should separate the 2 words so that it reads “mud clasts”.

Line 28: The use of the word non-actualistic is typically associated with unusual facies or intervals in Earth History. While the model proposed by the authors is certainly unusual, I would avoid using the term non-actualistic and instead perhaps state that there is no modern analogue for a similar system.

Line 43: Competing theories of what? I assume dolomite formation, but the authors need to be specific.

Lines 56-58: I would draw the attention of the authors to a recent paper published in Geology by Li et al. that documents the widespread precipitation of primary dolomite around the Permian – Triassic boundary.

Lines 61-62: What are the signatures indicative of a burial diagenetic overprint? The authors need to be specific.

Lines 68-70: This text is vague, and the authors need to explain what the dolomite phases are that are documented by Frisia and Wenk (1993) so the reader can better establish that these are burial diagenetic features.

Lines 88, 99 and 782: The authors use the term “Carnian platform”, which is incorrect, as “Carnian” is a time term and a platform is a physical object. I would change the text to “Carnian-aged” and also add a modifier to state where the platform was. So, “Carnian-aged western Tethyan platform”.

Lines 102-103: What is the evidence for seasonally wet conditions? The authors state

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in the abstract that the seasonally wet conditions make these dolomites special (non-actualistic in their terms), and so they need to provide evidence of the seasonally wet conditions.

Line 103: Which facies? Dolomite or clay? Or is this the entire sequence?

Line 104: Use of the term “extended” is confusing. Do the authors mean laterally-extensive? Extensive over time? Both?

Lines 104 and 107-109: Use of the term “a Germanic Keuper facies” is confusing. It’s not clear to me if the authors are discussing a general facies type or a formational name, especially in lines 107-109 where they discuss paleogeographic separation between the Travenanzes Fm and the Germanic Keuper facies. Overall, the text in lines 107-109 is confusing and needs to be rewritten.

Line 111: One facies zone is the Germanic Keuper facies. What is the other facies zone? If it is the Travenanzes Fm, then the authors need to word this differently, since it is confusing to compare facies to formations. This can be solved by referring to “dolomitic facies of the Travenanzes Fm”, for example.

Lines 114: I would replace “carbonate” with “dolomite” since the authors are attempting to prove that the dolomite is primary in origin, and use of the term “carbonate” here is not specific enough.

Lines 148-149: It’s not clear to me why the authors interpret the Travenanzes Fm as having been deposited on a very flat surface based on the lithologies that make up the unit. The authors need to provide stronger evidence.

Line 190: Are there units that go with “. . .a spot size 5.0. . .”?

Lines 237-238: The authors need to provide units for their detection limits.

Line 292: Approximately how thick are the tempestite beds?

Line 293: I think the authors mean to state “megalodont teeth”.

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Lines 319-322: The ooids appear to have been micritized to me, based on the photo. Is this the case? If so, this needs to be mentioned in the description. If not, then the authors need a better photo.

Line 328: The authors need to be more specific in terms of what the measurements are measuring. Diameter? Thickness?

Line 329: “Pale” is not a color, it is a shade of color. The authors need to add a color after the word “pale”.

Line 362: I’m not sure if the peloids are a rare type of allochem, or if subunits made up mostly of peloids are rare. Please clarify.

Line 436: I’m not sure what it meant by “It”.

Lines 479-481: The authors state that the mud was unlithified, but also note the presence of rip-up clasts made of the same mud. The authors need to account for this difference, since rip-ups require at least semi-lithification to form.

Line 484: I’m not sure what the authors mean by “this type”. Microfacies, perhaps?

Lines 491-492: The authors should include a reference at the end of this sentence.

Lines 492-493: This sentence seems out of place here since this is a discussion of processes within a possible ephemeral lake, and the previous text is trying to establish the larger depositional setting. In addition, I’m not sure that this text is necessary, since the mud is homogenous in composition, so stating that waves are responsible for homogenizing the mud is pure speculation without other evidence of wave action, like ripple marks.

Lines 511-513: I’m confused. Are the authors stating that the ooids are marine in origin, or lacustrine (like the ooids found at the Great Salt Lake). The authors need to be more clear as to what they believe the origin of the ooids are, and if lacustrine, provide modern examples, since ooids are rare in that setting.

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Line 530: “in situ” should be in italics.

Lines 538-539: If the sediment is being plastically deformed, it must be at least partially lithified.

Lines 543-544: The authors need to cite a reference at the end of this sentence: “What is atypical for a modern sabkha is the large amount of detrital input.”

Line 544: The authors need to cite a reference to support their contention that the Carnian was seasonally wet.

Line 554: The authors need to cite a reference that red color represents seasonally arid conditions in clays.

Line 556: I think the authors mean “after burial”, not “after sedimentation”.

Lines 679-680: This sentence is confusing, and needs to be rewritten. It could probably be shortened to just a few words and added to the end of the previous sentence.

Lines 691-696: These temperature ranges seem high, and therefore a reference to the temperature range of modern sabhkhas is needed. In addition, the authors also need to consider the effect of evaporation on oxygen isotope values and therefore temperature estimates from those values.

Lines 697-699: Why is this important?

Line 700: Why do the oxygen isotopes indicate a primary signature as opposed to a secondary signature?

Line 712-715: I’m not sure how these nodules relate to the cement rims surrounding the dolomicrite grains. I do agree with the formation mechanism for the nodules, but the authors need to add references to support their proposed formation mechanism.

Line 768: What kind of isotopes?

Figure 1: Change “positive areas” to “highlands” or “topographic highs”. The

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authors need to define the following abbreviations in the figure caption: Drau., Mr, Wa, Kr, Be, Fr and Ly. I don't see any Continental/Lacustrine areas on the map, but the symbol for this facies is provided on the key.

Figure 3: 3A need to focus in on the homogenous dolomite bed, as it is currently difficult to see as the view is too far out to allow any details to be properly discerned. The calcitic vertisol in 3b needs to be labelled. Gypsum nodules and crack fills need to be more clearly labelled in 3d. The view on 3f needs to be closer to allow the soft sediment deformation, and, in particular, the isoclinal fold, to be more clearly seen.

Figure 4: The authors need to more clearly distinguish the mud clasts, as well as the coarser grained and finer grained layers in 4b. The authors need to add arrows to 4d to point out the pseudomorphs. The ooids in 4e appear to be micritized to me. This may be a reflection of the size/resolution of the photo. I would recommend that the authors show a close-up view of the ooids. The feature labelled with a "P" in 4f is supposed to be a peloid, but it's not clear to me what the "P" is referring to on the photo. The authors need to include boxes in 4g that shows the areas depicted in 4h and 4i.

Figures 8 and 9: The captions need to be more detailed for all plots in both figures. What is the significance of each plot for the study?

Figure 10: What is the significance of the circled areas on the figure? This needs to be explained in the figure caption.

Figure 11: The authors need to note in the caption that Coorong Lagoon and Deep Springs Lake are modern dolomite deposits. Also, there is no mention of the Abu Dhabi sabkha in the caption, and it needs to be added to the caption.

Table 1: I am unfamiliar with the term "laminare". The authors need to be more specific as to what this is. This text at the bottom of the table is confusing and

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needs to be rewritten: “\*Needs to be further subdivided into peloids, intraclasts, flat pebbles and clast of brittle deformation” I’m especially confused by the term “clast of brittle deformation”. Table 2: I think the authors mean “Height” and not “Depth” as they refer to “Height” elsewhere.

Table 4: “Again, I think the authors mean “megalodont teeth”. It’s not clear to me what the authors mean by “top”, “bottom” or “part”.

Please also note the supplement to this comment:

<https://www.solid-earth-discuss.net/se-2019-34/se-2019-34-RC2-supplement.pdf>

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

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