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Interactive comment on "Towards *in-situ* U–Pb dating of dolomites" *by* Bar Elisha et al.

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This manuscript deals with the problems and limitations of U-Pb dating of dolomites by LA-ICPMS. It provides interesting constraints on the possibility to use this technique for this mineral. The most important result proposed by the authors is that matrix-related mass biases, due to the use of calcite RM rather than dolomite (the latter still being unavailable to the community), can be large, resulting in unexpected ages that can be questioned. The most striking example concerns samples MAM-3 and MAM-7, whose ages obtained are far much older than the stratigraphic ages. These results and reflections raise questions, and certainly deserve publication. Many comments have already been done by RC1 and SC1, so hereafter you will find some additional (or close) comments and questions that came up while reading and that could be considered by the authors before publication.

1) Sample characterization:

- I don't really understand the interest of REE, which don't bring much to the discussion. The spectra are identical to each other, and close to those expected for marine carbonates. I suggest to remove that part.

- Also, it would be nice to have higher magnifications for thin section photomicrographs (not "photomicrographic") (Figure 1), as any detail can be hardly seen here.

- I am not an expert in EBSD, but I am wondering why there are so much difference in the calcite/dolomite ratios calculated between XRD and EBSD? What I understand from your results is that XRD is not efficient compared to EBSD in detecting Mg-calcite, but you still recommend to use it (l. 251-252)? It is not very clear. From my experience I have always detected minor phases (< 5%) with XRD on bulk powders. Mg-calcite is routinely detected in recent carbonate sediments, and can usually discriminated from "pure" calcite.

- Did you perform simple analyses such as optical cathodoluminescence (CL)? This can be useful to detect several cement generations or recrystallization events on a single carbonate sample, and can be done easily in all Earth science labs. It is likely that growth bands described in Figure 7B would appear in CL. The same applies to Figures 5 and 6, where the magnification is accessible to optical microscopy. I doubt that calcite and dolomite, or distinct clasts, would not appear in CL.

- Section 3.2, l. 218-221. I agree with the sentence but this is true for all type of minerals, not only dolomite! It is obvious that one should always try to avoid analyzing impurities or several grains.

2) U-Pb dating: many comments have already been done by RC1 and 2, so I will limit myself to add a few.

- Have you tried to make spot analyses on polished slabs instead of thin sections? Some argue that working on finely polished thin sections may potentially lead to skewed

results due to the preparation process.

- From a methodological point of view, the validity of reasoning based on stratigraphic ages to deal with dolomitization can be questioned. This problem is old and well known, dolomitization is mostly of secondary origin (diagenetic, hydrothermal), and several recrystallization events are frequently reported in the literature. It can be expected to find ages younger than the stratigraphic age (as you also mention in the text). This does not mean that age is wrong, the question being to know which event is dated in a complex geological history. Finally, for most samples, I wonder if the absolute values of ages deserve any discussion (section 3.1), as they are either identical or younger than the depositional age, and seem to be precise and with acceptable statistics (except EFN-1, but here again it clearly looks like phase mixing). Of course, samples MAM-3 and MAM-7 do not fit into this perspective, as the reported bias is thus much greater than that put forward by Guillong et al (this issue). But these are the only ones of your entire data set to be anomalous. I note that these samples, as those of Guillong et al., are dolomicrites. Reading your study I am now convinced that dealing with this type of matrix should be avoided.

- I don't know if you have analyzed other dolomite samples than those presented here, but you only present results on micritic or sparitic dolomites which clearly have, according to your EBSD analyses, complex histories involving the presence of mixed calcite / dolomite mineralogy. The approach would have been more complete if you could have analyzed also larger dolomitic cements (fracture or pore-filling), if only to test the drill rates and downhole effects, compared to other dolomite matrix and to calcite.

- TW plots: I read on the figures that you always have positive error correlations. I would expect to have also negative ones, notably in a TW diagram?

3) Other:

- l. 71: 260°C is not "low temperature" in a sedimentary, diagenetic environment.

- l. 72: problem of wording ("that assumed to occur")

- l. 79: not sure this is enough to argue for a syngenetic origin. Are stable isotopes (for example) available? One can clear see bioturbations and laminae on the pictures: a more precise petrographic description would be welcomed

- l. 81-82: "which reflect continuity of reefs along fine-grained, well-bedded shelf basin rocks". Unclear

- l. 113: "Following LA analyses, we used several techniques". Generally one does first the petrographical and geochemical analyses, then the dating.

- l. 261: "Ca-rich and Mg-depleted zones". Please point to some of these areas on the figure (arrows)

- Is it intentional to have no conclusion? Or did I read a wrong version?

- Supplementary table. Please provide an excel spreadsheet rather than a pdf file for your data, so that people can play with the data more easily.

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