

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

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The attempt of the authors to *in situ* date dolostones as well as the discussion they encouraged is both timely and necessary. Prof. Lynton Land once stated: “there are dolomites and dolomites and dolomites.” In this regard, I could add that not all of them are amenable to be successfully dated by *in-situ* U–Pb. And this is, in my view, the key point that the authors have attempted to highlight in their first iteration of the manuscript. After reading their report, I cannot conceive how the crystallization history of a multistep growth dolostone (comprised of, say, crystals of 50–120 micrometers in size); affected by diagenesis in multiple burial realms; which perhaps also experienced epidiagenesis or the moderate influence of burial diagenetic fluid flow, can ever be accurately resolved radiometrically by using LA-ICP-MS. In such hypothetical case, what is actually being dated? (10.1029/2007RG000246). Thus, I respectfully disagree with the statement by Prof. Woodhead in RC1, that previous bulk radiometric dolomite

C1

dating ventures have been successful by themselves, which would in turn point to a general feasibility of *in situ* dolomite dating. On this note, it must be noted that the recent work by Mueller and co-workers (10.1111/sed.12664), implemented a variety of proxies, including *in situ* radiometric age dating, to resolve a rather complex paragenetic history of a particular example in Spain. An important factor that deserves further discussion in a second iteration of this work is determining whether dolomite precipitated in an open vs. close diagenetic system. For instance, in an open early diagenetic system affected by terrigenous influxes, one may expect a shift in the radiometric ages that could be inferred. An avenue to resolve this that is not fully developed by the authors (i.e., their little attention to their REE dataset as indicated in RC1), but it could perhaps be an approach based on the systematics of trace elements (e.g., 10.2475/07.2016.03). Yet, I would wonder then whether such an additional analytical efforts would be reasonable when simpler deductive means to define the approximate age of crystallization of a given generation of dolomite might rather be applied (?). What about localized recrystallization of limestone in a fault controlled dolomitizing setting? Would the age of the dolomite as determined by its parent to daughter ratio, be that of the host limestone, would it to some extent reflect the  $^{238}\text{U}/^{204}\text{Pb}$  of rocks that have also interacted with the dolomitizing fluid (which can itself deliver or take away parent and daughter isotopes)? Perhaps the admixture of radiogenic sources can probably be resolved, again, by using the trace element systematics of dolomite should a comparative approach, host vs. dolomite body, can also be implemented (?). As envisioned by Rasbury & Cole (10.1029/2007RG000246), U–Pb dating of dolomite can likely assist our interpretations of evolving diagenetic models by placing time constraints on when a dolomitizing fluid interacted with an altered host limestone, as well as perhaps the nature of the dolomitizing fluid(s).

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C2