Re-review of Bar Elisha et al., revised version

The authors have provided responses to most of the concerns raised in my first review, but I believe that further improvements could be made should the editor deem them necessary.

I welcome the addition of further references regarding application of the U-Pb dating method as applied to dolomites. However, these two new sentences, which cover both bulk and *in situ* methods have not been integrated poorly with the pre-existing text. I would suggest removing the last sentence (*in situ* references) and considering how it could be re-incorporated around lines 46-47. This whole paragraph is rather important since it sets the scene for the significance of this paper. Given that six papers will now be referenced for previous studies of dolomite using LA, and that this contribution is entitled 'Towards *in-situ* U–Pb dating of dolomites', suggesting that some further progress has been made towards this end, it would seem important now to comment here on the novel aspects being explored by this particular contribution.

Although I readily agree that we should strive for matrix matched standardisation wherever possible in LA studies, I must admit to still remaining unconvinced by the crater roughness/ablation efficiency arguments presented in Section 3.2. Although the Guillong et al paper provides for estimated shifts of ~4-8% in age due to ablation variations between dolomite sample and carbonate RM, the age mismatches seen in this study for the Cretaceous samples amount to shifts of more like 40-80%. It is hard to see how this can be achieved through variations in ablation response alone and, if it is, then the implications are enormous - we will probably need a new matrix matched RM for every type of dolomite sample that we analyse! It is also worth considering here that different types of (calcium) carbonate also ablate at different rates but nothing of this magnitude (ages so much older than expected) has ever been reported in the now quite extensive in situ U-Pb literature. I think a much more reasonable explanation, given that the problem clearly revolves around micrites (sparry calcite from the same region giving approximately correct ages), and that the micrites have rather high Pb contents, is the presence of older detrital material (not necessarily carbonate) in these samples. I think that it would be worthwhile for the authors to consider alternate mechanisms for this age shift rather than relying on the ablation argument which seems both physically implausible and (in terms of magnitude) is actually not very well supported by the Guillong et al results.