

Interactive comment on “Stepwise chemical abrasion ID-TIMS-TEA of microfractured Hadean zircon” by C. Brenhin Keller et al.

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This is a great and long overdue study. We are aware since long that SIMS analyses will never be able to resolve single orogenic events and associated multi-episodic zircon growth/overprints in the Hadean. I suggest publication with minor revision and tried to make a series of comments, coded by page/line number below.

The only generally negative aspect I would put forward is the random fragmentation technique using a tip. Controlled, CL image-based fragmentation into single growth domains would have brought more control on the potential mixing of growth domains and would potentially help to interpret the 50 Ma age dispersion of the concordant residue analyses of sample z6.10. But, yes, you can always go to higher complexity of

C1

the analytical workflow.

Comments indicated by Page/Line number: 2/33: “early Pb-loss”: maybe explain this, non-zero? 3/6: Eight orders, this is too long ago, pure history. I suggest to stay rather with sound, present-day comparisons. 3/10: 1 μg fragment, add a normal U concentration range for non-specialists 3/13: I would suggest to cite Widmann et al. 2019, sorry for this. . . 3/23: refer to Figure 1 3/29: My point, uncontrolled fragmentation 5/20: Mark the 4004.2 Ma point with an arrow on Fig. 2B 8/7: 90% of the observed “total” cation budget? 8/20: Figure 5 is misleading, suggesting that you measured leachates by SIMS on first view. Maybe only add “on residues” on the y-axis title? Lines 20–25 good be better written, is not entirely coherent. What did you measure, what did you plot in addition, etc. . . 8/30: Here lacks somehow an argument in-between these two sentences: the double 238, 235 decay allows determination of of age of lead loss through the Discordia – why then you want to get rid of lead loss through chemical abrasion? 9/caption figure 4: L1 leachates are mainly enriched in LREE, not MREE. You also don’t mention inclusions of REE-enriched minerals. 11/5: There is quite a bit of literature explaining this phenomenon, especially for monazite. Watch out for papers by Seydoux, Poitrasson, Cocherie. . . 11/26: Would need some explanation how these modes were calculated any Monte carlo – bootstrapping? I would be curious what intercept clusters you obtain when using the Davies et al. 2018 G3 code. 11/33: Lack of tectonothermal disturbances: this would be after incorporation into the sediment, it is thus not true only for the zircons but also for the sedimentary host rock, right? 12/9-10: Don’t fully understand the logic of this sentence (Instead. . .) 12/14: There have been some other arguments in the literature, based on the zircon bulk modulus (e.g., Hazen & Finger, 1979). They are based on the fact that zircon has SiO₄ tetrahedra, monazite not (titanite yes. . .). The zircon lattice is isodesmic and very stiff. Maybe digging a bit into crystal-physics literature..? 12/18-27: Quite ad-hoc explanation and not really in the scope of this paper. Maybe my suggestion above will lead you to some more sound explanations, badd has no isolated [IV]cation tetrahedra – O bonds. 12/33: The crystal domains that remained below the first percolation point do not get

C2

decomposed through the annealing, I would argue, only the ones above. Arguments you may find in Widmann et al. 2019. 13/20: How metamict were they? They are not clustering at around 3.0 Ga, which would indicate fluid-aided lead loss at that time (erosion-deposition), which they should if they were metamict. Not sure about your statement. I agree that they were more damaged than they are now. I very much agree with the general statement of lines 24/25. 14/25: This is entirely consistent with the improvement of the Raman parameters found by Widmann et al. 2019

Well done, congratulations!

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