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

C. Brenhin Keller et al.

cbkeller@dartmouth.edu

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We appreciate the thorough and positive review by both reviewers, and respond here to any comments from Prof. Schaltegger that required changes to the text. Quotes from the review are italicized.

> *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*

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Discussion paper

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 the analytical workflow.

More controlled zircon microsampling would certainly be an excellent avenue for future work. Though one can exert *some* control with a sufficiently fine carbide tip by scoring the zircon where you wish it to break, slicing by laser or FIB would allow much more precise (if more expensive) selection of zircon domains for TIMS analysis.

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

We have modified the text accordingly.

3/29: My point, uncontrolled fragmentation

Yes, it largely was. Next time!

5/20: Mark the 4004.2 Ma point with an arrow on Fig. 2B

We have added rectangles highlighting the concordant residues to make this point more visible.

8/7: 90% of the observed "total" cation budget?

We have changed "observed" to "measured" to reflect that this only includes cations whose concentrations were determined in our analyses.

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...

Good suggestion, thank you. We have attempted to clarify.

8/30: Here lacks somehow an argument in-between these two sentences: the double 238, 235 decay allows determination of age of lead loss through the Discordia – why then you want to get rid of lead loss through chemical abrasion?

We have expanded on this in the revised text.

9/caption figure 4: L1 leachates are mainly enriched in LREE, not MREE. You also don't mention inclusions of REE-enriched minerals.

We have attempted to clarify: while MREE are much less enriched than L1 leachates, they are still enriched substantially above residues, and Eu is *less* enriched than the adjacent MREE. This apparent preference for, e.g., 3+ Gd over 2+ Eu would not seem to be expected if the host were a purely structureless, metamict glass.

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.

We have expanded these sections. For the latter, yes: Monte Carlo resampling.

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?

True!

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 decomposed through the annealing, I would argue, only the ones above. Arguments you may find in Widmann et al. 2019.

We have attempted to clarify and reduce the speculativeness of this section.

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.

Presumably not ever fully metamict I would agree! Just somewhat damaged..

14/25: This is entirely consistent with the improvement of the Raman parameters found by Widmann et al. 2019.

Agreed!

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