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Comment on gchron-2022-21

Graham Edwards (Referee)

Referee comment on "In situ U–Pb dating of 4 billion-year-old carbonates in the martian meteorite Allan Hills 84001" by Romain Tartèse and Ian C. Lyon, Geochronology Discuss., https://doi.org/10.5194/gchron-2022-21-RC1, 2022

Overview

In this manuscript, the authors describe an analytical approach to measuring U-Pb isotopes *in situ* in carbonate minerals of the Martian meteorite ALH 84001. They report U-Pb and Pb-Pb dates that overlap with prior bulk isochron dates of the Rb-Sr and Pb-Pb systems. Based on this intra- and inter-system concordance, they concur with prior studies that ALH 84001 has experienced minimal disturbance since its primary carbonate system "closure." In addition, this study provides a compelling proof-of-concept discussion on the precision capable with U-Pb dating by SIMS in multi-Ga carbonates, and the authors thoughtfully comment on where analytical and standardization improvements are needed for further improvement of these approaches.

The methods and interpretations in this study are overall rigorous and sound. My major criticism of this manuscript is that the authors offer only limited interpretation of the geologic (or areologic, if preferred) implications of the uneventful history recorded by ALH 84001 carbonates. In particular, I believe that this manuscript would benefit from expanded discussion on the implications of low common Pb content in the carbonate-forming fluids and the implications of multi-system concordance for the geologic and impact/post-impact history of ALH 84001. While not strictly necessary, I think the manuscript falls short of its potential without broader discussion/interpretation of the results.

I recommend this manuscript for publication in *Geochronology*, so long as the following comments are sufficiently addressed.

Respectfully,

Graham Edwards

Specific Comments

- Did different carbonate lithologies/mineralogies manifest different U-Pb systematics? From a cursory look at Table S3, it appears there was not a difference, but potential differences in the two mineralogies should be examined. If indeed the systems show similar systematics, that adds further support to the authors' conclusions of the undisturbed history of ALH 84001 carbonates.
- Along similar lines, there does not appear to be a difference between the U-Pb and Pb-Pb dates of the two different mineralogies (Table S3), implying a common origin. I recommend the authors comment on how this informs the mechanisms of carbonate formation in ALH 84001.
- There is some linear spread in the U-Pb data of WC-1 in Fig. S1. Is this accounted for in the use of WC-1 as a standard for U-Pb fractionation? If so, how? If not, the authors must justify why a correction is not necessary and/or how any corresponding uncertainty is propagated. This variation does not appear to be within the 2.5% uncertainty used to account for uncertainty in the age, though I may be mistaken.
- While the authors are clearly working with the limited carbonate U-Pb standards available, the primary and secondary carbonate standards are far younger than the unknown sample (all over an order of magnitude younger than ALH 84001 carbonate). It would be beneficial to incorporate a discussion addressing potential uncertainties stemming from this and why they are (or are not) relevant to the conclusions herein. e.g. Are the effects of U-Pb fractionation (accounted for with measurements of WC-1) expected to differ for between younger and older material with different 238U/206Pb ratios? This would fit in well with some of the pre-existing discussion on methodology in section 5.1.

Line-by-Line Comments

L 93-5 – Is this linear correction factor necessary or precedented? Are there alternative models and would these have an effect on the calculated dates? I have little expertise in the realm of SIMS U-Pb, so I apologize if this is a naive question.

L 93,102 – Both regressions are stated as "anchored" and based on this phrasing and the shape of the uncertainty envelopes in Fig. S1, the authors seem to mean that they assume a fixed/anchored 207Pb-206Pb intercept for these regressions. I think the authors could be more explicit that they are assuming an initial Pb composition as their anchor. More importantly, the authors should justify the choice of this approach (over leaving the intercept a free-parameter in the regression) and comment on the appropriateness of the assumed initial Pb compositions and if these have any corresponding uncertainties.

L 127 – This is an insightful result and I think the manuscript would benefit from some speculation as to why ALH 84001 carbonates inherited so little common Pb.

L 130-2 – The Rb-Sr and U-Pb inter-system concordance and resilience to resetting at 14 Ma is another insightful finding. I agree that this confirms that "not much happened" between these events (i.e. no further impact processing or aqueous alteration). I think the manuscript would benefit from some further discussion addressing what might have differed between impact events that did and did not effect carbonate system of ALH 84001.

L 135-9 The potential application to CCs is exciting! The abundance of U in CCs is on the order of 10 ppb. Acetic acid leachates (Turner+ 2021, Science) of a CV and CM contain ~1ppm and 50 ppb U, respectively. It would be worthwhile for the authors to comment on the promise/challenge these present for *in situ* U-Pb dating of CC carbonates, compared to those of ALH 84001.

Fig. 2 – The authors do not explicitly identify the purpose of the bold black outline near 4000 Ma. Intuitively, this represents the confidence bounds on the mean date, but it would be helpful for readers to explicitly state that.

Fig. 2 – Please identify whether these 2σ uncertainties are standard deviation or standard error.

Table S1 – The binning approach of the authors excludes the 300.8 Ma date of Drost+ 2018 in their compilation. While this does not effect their interpretations, I suggest the authors use bins without gaps between them for the tabulated compilation.