Review of Jonckheere et al, Geochonology 2026

The authors report a series of new experiments and observations examining the etching rate of fission tracks compared to the surrounding crystal. Overall, though I disagree with some of the authors' methods and conclusions, I believe the science will be moved forward by reporting their data and analysis. That said, I have a number of comments and suggestions that I hope can be used to improve the work, with some nudges that I hope can move the discussion forward. I also strongly urge that the authors publish their raw (track-by-track) data in tabular format.

Part 2 is a very ingeniously constructed experiment to try to measure along-track etch rate, and is a valuable contribution. One very interesting result is that they document the possibility of an etching time deficit in step-etch experiments, where time is essentially lost by the etchant having to re-traverse the tracks; this is a new insight, as far as I can tell, and will be a necessary consideration going forward to experimentalists studying etching.

However, in terms of inferring whether along-track etching rates vary, the authors' interpretation is disputable. The residuals about the 20 s line in Fig. 3a appear to be clearly structured, with most points below the line at 5-10s and above it at 15s. Assuming the surface point is well-anchored as the authors claim, it appears that the data would be better fit by a concave-upwards curve, which would correspond to a diminishing along-track etch rate. The authors may disagree, and I would not hold up this paper if they do, but I would (again) strongly recommend that they publish the data for Fig. 3A so others can evaluate the possibility.

There are also some aspects of the experiment/data presentation that would be good to clarify. The authors use the variable "l", which usually means etched track length, for an extrapolated point based on the diameters of the etch figures. As shown in Fig. 2a, these extrapolated points appear to lie beyond where there is a visible track (though this may be in part due to the plunge of the tracks), and for a normal fission track that has an ending point the extrapolated length will always exceed the real etched one. Accordingly, I suggest using a different variable name (E, le, or el for extrapolated length?) to avoid this potential confusion. It's a little surprising in Fig. 2a, b that all "etch figures" appear to be in focus at the same time, given that the interior figures are up to ~5 µm deeper than the surface ones; is this just the way it was, or are these images composited depth stacks or something like that? It would also be good to specify exactly how d_S and d_D are measured, given that a circle is being used to measure a diamond-shaped feature. For d_D, is the circle drawn so it is tangential to the diamond faces (which looks to be the case in Fig. 2b, but it's hard to be certain), or is it drawn so its equator ends at the diamond tips? In the surface tracks, the beginning of the track is above the midline of the circles, by perhaps subtly differing amounts; what criteria are used to locate d_S circles? It's noteworthy that the assumption is that etch figure size is a linear function of etching time, but d_S increases at 0.088 µm/s in the first 20-s step, and 0.110 µm/s in the next two steps; this deserves some discussion and explanation. Finally, are the points A*, B*, and C* equivalent to l_M in Table 1? If so, it would be good to make this clearer by stating it directly in the figure and/or table caption.

Figures 4 and 7 need scale bars.

The unchanging angular frequency observed by the authors in Fig 5d is only slightly surprising (line 151-2). Ketcham (2003) observed no significant change in angular frequency until "accelerated length reduction," the faster relative shortening of tracks at high angle to the c-axis (Donelick et al. 1999) set in.

This effect only appeared in pooled experiments with mean c-axis projected lengths of 12-13 µm. The authors' results for experiment 21-3 (their Figure S01d) are in the midst of this range (lc = 12.4 µm), and shows only a handful of these tracks, but considerably more on the "elliptical" trend. Evidently, accelerated length reduction is in a sufficiently incipient state in this experiment that there was not a significant loss of high-angle tracks.

The intersection point distribution is also only somewhat surprising (line 154-155), but maybe not for the reasons the authors state. Any deviation from a uniform distribution of intersection points should mainly a function of the analyst avoiding tracks where a tip is obliterated or obscured by the etchant pathway. The overall pattern in Fig. 6a bears this out, with a flattening of the CDF as the intersection points approaches zero (the track tip). From an unannealed starting point, as the track shortens, this near-tip flattening zone should expand (in the normalized along-track coordinates used), as a higher proportion of the track is near a tip. There might be a hint of this in Fig. 6a, but it seems smaller than expected, given that mean track length has fallen almost 40% (from 16 to 10 µm). So, it appears there must be some countervailing bias. The most likely culprit is etching rates and track length; in a track that is longer and/or has slower etching, the far tip might be less likely to be fully etched, which would have the effect of expanding the size of the low-probability, near-tip region. This is fully consistent with the analysis in Ketcham and Tamer (2021).

In Figure 5b and line 247 (etc.), l_P evidently means c-axis-projected length of a single confined track, whereas in section 2 (Fig. 2c; line 74) it corresponds to surface-projected extrapolation of length inferred from etch figure sizes – an example of the confusion in variables that should be avoided.

Some would argue that change in median Dpar from 1.89 µm to 1.80 µm (line 215) is unlikely to be significant, given the limitations imposed by using visible light (wavelength 0.38-0.75 µm, leading to a measurement resolution limit of ~0.2 µm). I'm willing to believe there probably is a real change, but the uncertainty of the ratio of two numbers so close to each other, with substantial measurement imprecision, will be very high, which makes it very questionable to use that ratio for any sort of kinetics calculation; the interpretation (lines 209-228) seems like a stretch. At the very least, uncertainties should be propagated to show the range of possible values, but I have a feeling that it will demonstrate that the analysis is uninformative, and thus does not add anything to the paper. Additionally, the figure reference on line 212 should be to 6d, not 5d.

To conclude my comments on sections 2 and 3, I've said in previous reviews of the authors' work that their v_T comparisons are somewhat compromised because their v_T measurement method presumes that it is unchanging along the track (which I don't believe their results demonstrate, including in this study). That said, it's very reasonable to argue that the particular etching structure(s) proposed by Ketcham and Tamer (2021) may be inconsistent with some of the data presented here, but it should be kept in mind that the variable etching structures examined in that work were intentionally simple end-members, and other structures are possible, particularly in how etching evolves with annealing. Figure 6b indicates that individual along-track etch rate, as determined by the authors, can be anything from 25 to >250 µm/min, or a range of a factor of 10 or more. They cite a prior work claiming that track orientation may be partly responsible, but even within a confined c-axis-angle range a factor of 4 variation is observed (Jonckheere et al. 2024, Fig. 5c). Is this remaining dispersion attributable to measurement uncertainties, or does it truly reflect a variation in etching rate, and if so what might be its cause?

The large change in etch figure size between fossil and induced tracks in the Duluth samples, which the authors hypothesize is due to radiation damage, is interesting and unexpected (e.g., line 338). We have looked for such an effect in my group, and not found it. In our example with the highest dose, Tioga apatite, which has an AFT age of ~220 Ma and has ~35 ppm U (and thus a similar dose to Duluth, with ~10 ppm U and an AFT age of ~900 Ma, though we have not measured Th), we observed absolutely no difference in Dpar (2.50 µm induced, 2.48 µm fossil). It's possible that our preannealing (450 °C for 48 hours, twice the time the authors use) was nevertheless inadequate because of the higher resistance to annealing of Tioga apatite (with respect to fission tracks). We've also not seen an effect in Durango and Fish Canyon apatites; though the doses in these are much less, they imply that any radiation damage effect on etching will be limited to infrequently-encountered doses. Also worth noting is that Iwano et al. (2018) report unstoichiometrically high halogen contents (F ~ 5 wt%, where the maximum possible should be 3.88 wt%, and a standard deviation of 0.7 wt%, plus 0.6 wt% of Cl to boot), and so there's considerable uncertainty as to the possible OH content, and its homogeneity, which could also lead to varying etching properties in different aliquots. Figure 1 in Iwano et al (2018) shows appreciable zoning in both apatites.

Accordingly, it would be good for the authors to do more to document and test their analysis. Measures might include reporting the individual Dpar data on a grain-by-grain basis, and perhaps seeing if there is a relationship between Dpar and track density; Iwano et al. (2018) report U concentrations varying by a factor of 3-5 from grain to grain, which might be a large enough range to detect a relationship between Dpar and (inferred) radiation damage, if there is one.