Comment RC2	**Response AC2 (corrections)**
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.	We cite 1σ-errors in the revised manuscript and perform error propagation where possible. <u>Correction</u>: due to the small relative errors on the median-Dpar, the activation energy increase is known with meaningful precision: (1.22 ± 0.27) (instead of 1.22 ± 27) eV per atom.
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?	"There is, in our opinion a clear angular dependence with minima (~25 $\mu m \cdot min^{-1}$) parallel and perpendicular to the ***c***-axis, and maxima (~75 to ~125 $\mu m \cdot min^{-1}$) in between". <u>Correction</u>: the reviewer's estimates are correct; ours referred to the wrong dataset (45 s instead of 15 s etching). The data thus scatter more than we argued. This is indeed in part due to measurement imprecision. However, at the time, we used a less precise, direct measurement of the track cone angles (taper) for our v_T-calculations. The importance of the measurement imprecision can be inferred from the decreasing scatter from 15 s to 45 s etching, as the track widths and cone angles increase and become easier to measure. Nevertheless, part of the v_T-variation is real; there are differences from track to track that are observable in images even without measuring. The data confirm that there is also a definite angular dependence.

Comment RC3	**Response AC3**
In response to my comment concerning Fig. 3a and the appearance of structured residuals, the authors reply: "A concave-upwards trend means that the diamonds at greater distance from the surface are larger than for a constant v_T. Larger means longer effective etch times and shorter access times, which in turn implies a greater average v_T between their surface and the crack intersections, i.e., a v_T increase towards the tip." I stand corrected; the quantities being graphed are unfamiliar and thus a little tricky to interpret, at least for me. One insight that helped me is that the steady increase in steepness of the curves in Fig. 3a also provides a direct indication of the time lag in re-etching; if the etched track was infiltrated instantly, then the lines would be parallel. In any event, the apparent non-linearity remains interesting, even if it implies a rising etch rate, and I look forward to the authors providing the raw data as part of their published manuscript.	We are pleased that the reviewer accepts our explanation and even contributes to our conclusion. We will of course provide all our measurements if our manuscript is accepted.
To the authors' final comment ("We propose to refer to the reviewer's results (pers. com.?) and to moderate our conclusion."), the Dpar measurements are in the supplement to Tamer and	We looked up the data of Tamer and Ketcham (2020), and agree that there is no difference between the fossil- and induced-track Dpar's of the FCT, Durango and

Ketcham 2020 (Tamer, M.T., Ketcham, R.A., 2020. Is low-temperature fission-track annealing in apatite a thermally controlled process? Geochemistry, Geophysics, Geosystems, 21, e2019GC008877. DOI: 10.1029/2019GC008877), and Tioga apatite fission-track ages and U determinations are reported by Roden and Miller (Roden, Mary K., and Donald S. Miller. "Apatite fission-track thermochronology of the Pennsylvania Appalachian Basin." Geomorphology 2.1-3 (1989): 39-51.). If they want to also refer to a personal communication, these peer reviews have their own DOI, so that might be the modern way to do it.	Tioga apatites; we found no fossil-track data for Renfrew. If we read the data right, compositional differences between the grains with fossil and induced tracks are excluded in these cases too. Depending on rater uncertain estimates of the U,Th-concentrations and effective damage accumulation ages, it is just possible that the FC1 apatite is (or once was) past the first damage percolation point, whereas the reviewer's samples are (were) not. The conflict might thus point to the fact that the apatite etch rate is related to its overall defect structure, which itself is a non-linear function of radiation damage. We prefer not to include such speculation in our manuscript. We will refer to the reviewer's data in the manner suggested at the end.

Sincerely,

R. Jonckheere
F. Trilsch
Jie Liu
Pengfei Zhai
T. Nagel