Geochron 2023-13

On etching, selection and measurement of confined fission tracks in apatite

CC1: *comments. by Richard Ketcham, July 12 2023*

RC1: replies by Raymond Jonckheere, July 26, 2023

This is a very interesting study that builds upon previous innovative work by the author and his student. Utilizing track shape is an extremely promising way to wrest more information out of fission-track data. As the technology of data acquisition and image analysis continues to progress, some shape data will undoubtedly become routinely available with little effort, and can certainly be utilized. That time is not quite here yet, but the author's group has invested considerable effort in try to build the foundations for what is to come, both observational (Aslanian et al. 2021) and theoretical (Jonckheere et al. 2022). The future of fission-track analysis will certainly include aspects of this work.

I am obliged to Dr. Ketcham for his interest in our work and for his critical and constructive comments. It is useful to introduce some abbreviations: JT17: Jonckheere et al. (2017); TC19: Tamer et al. (2019); JA19: Jonckheere et al. (2019); TK20: Tamer and Ketcham (2020); KT21: Ketcham and Tamer (2021); AS20: Aslanian et al. (2020); AS21: Aslanian et al. (2021); JA: Jonckheere et al. (2022); TK23 Tamer and Ketcham (2023).

*That said, there are some critical errors in the present manuscript, which in my opinion make it unsuitable for publication at this time. First, the author attempts to compare his **results** to the (possibly competing) predictions of the variable along-track etching rate **model** of Ketcham and Tamer (2021), but rather than using the **equations** therein attempts to rederive them anew. In doing so, he made a **critical error** by taking the etch rate at the track tip to be zero, rather than the bulk etch rate. Simply put, his equations do not correctly reflect my model, rendering a number of plots and assertions incorrect.*

I rederived the equations because I was interested in the preferred linear v_T-model, and wished not to complicate the equations with v_B, as $v_B > v_T$ over 0.11 µm at the ends of unannealed induced tracks. I did, however, consider v_B in my calculations by setting $v_T = \max(v_T, v_B)$. I included the derivation in appendix A to allow those who wished to check my results and for possible use in future calculations. I did not draw the "v_B-wings" in Figure A1 but neither did KT21 in their Figure 9. Figure 8, does have (exaggerated) v_B-wings and shows that I included v_B in the calculation. The circular feature at the left of 8d is due to an isotropic v_B; the spike to the right grows first when it extends into higher v_T-values along the track.

*Second, he fails to "remove the log in one's own eye before removing the mote from his brother's" in not investigating the limitations of his own data. The wavelength of **visible light** (0.4-0.7 µm) induces an unavoidable limit to the precision of optical microscopy data, which is not too influential for track length measurements but is a much larger issue for track width, and larger still for measuring angles from which to infer etch rates. Thus far I'm not aware of any attempt to quantitatively investigate the **uncertainties** of these data, either by mathematical analysis or brute force repeated measurements. This makes it very difficult to critically think through the implications of these data. In addition, it appears that each **half-track** is measured once, and so it's unclear how the author would detect a change in along-track etch rate.*

For discussion, it is perhaps useful to explain the disagreement with TK20 and KT21. (1) The manner of our v_T-measurements assumes that v_T *is* **constant** along most of the length of a fission track. If it isn't, and v_T varies along a track, as the linear model requires, then our data are meaningless. (2) In contrast to TK20 and KT21, our samples provide no evidence for an **increase** of the track etch rate v_T following partial annealing. I believe that scientific progress is better served by addressing disagreements than by ignoring them.

Our microscope resolution is ca. 0.2 µm but this is not the precision of our measurements. Resolution is the least separation between two dots that can be distinguished under the microscope. Figure 1 shows the blurred contour of a horizontal confined track, but circles drawn with care for measuring v_T do not have precision errors of twice 0.2 µm on their diameters. It would appear that the error on a width is about the same as that on a length measurement. I believe our length data show that these are accurate.

That does indeed not prove that our v_T-measurements are precise, far from it, but we have thousands of tracks. Our data result from direct **v_T-measurements**, compared to **model estimates** based on mean track lengths.

We measured straight sections, often across the supposed "v_T-maximum", which are much longer than half a track (Figure 1).

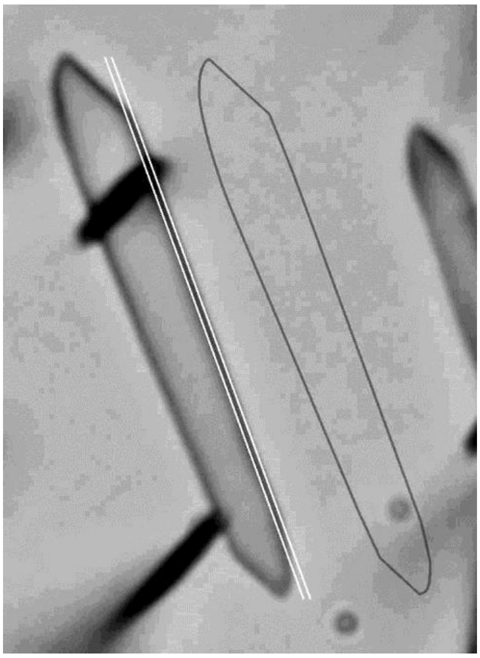

Figure 1. A straight track can look crooked but it is difficult for a crooked one to appear straight.

*Third, the author neglects to discuss data that appear plainly **contradictory** to his assertions. Tamer and Ketcham (2020) present clear indications of changes of track etch rate, such as **annealed tracks** being longer than non-annealed ones after 10s of etching, as well as the overall sequence of lengthening over a step etch experiment. As discussed in the detailed comments below, the model proposed here makes a number of further predictions about minimum or maximum track etch times that appear well tested and definitively excluded by the Tamer and Ketcham (2020) results. Given that the same individual (M.T. Tamer) produced both data sets, the **scientific process** suggests that this evident disparity in results at least be discussed, and credible ideas offered on where the problem might be, or what alternatives are available. For example, it may be plausible that the huge variation in track etch rate inferred from these measurements could be from measuring different sections of tracks – arguably a more parsimonious explanation than asserting that etch rates vary from track to track by a factor of 10. Or, if Tamer's previous data are unreliable, where might that have stemmed from?*

I thought that not emphasizing that our data contradicted TK20 and KT21 would avoid discord. Several factors indeed convince me that their conclusions are not accurate. The fitted data (TK20 Table 2; KT21 Table 1) fall into three blocks (Figure 2). Block ① are experiments aimed at determining v_B; the 10 s lengths are relevant to v_T but the longer etch times are not. Block ② shows 20-30 s data, commonly interpreted as bulk etching, but here recast as v_T, which is admissible. However, the modelled lengths (L_{lat}) of the three annealed samples are identical with their 20 s measured mean lengths. It follows that their 25 and 30 s lengths are due to v_B, not to v_T. The 20-30 s data of all the samples relate to the final ~1 µm length increase of the tracks. I thus appears that most of the v_T-model rests on the 10 and 15 s data in block ③.

The large difference between the 10s mean lengths of the annealed and unannealed samples underlies the assumed **increase of v_T** after annealing, and a good part of the linear model. I added published and unpublished data for unannealed fossil and induced tracks etched for similar duration (Figure 2). The shortest 10 s length (12.5 µm), by Murat Tamer, is >2.5 µm longer than the values in TK20 and KT21. The rest is much longer still, up to >16 µm after 15 s immersion. This suggests that the 10 s data for the **unannealed** samples of TK20 and KT21 are unsafe. I submit that this is related to the substandard microscope images (Figure 2; Figures 9 of TK20 and KT21). I cannot resist adding that this puts the preceding comment about the measurements in our manuscript in a somewhat different light. Carl Sagan said that "*extraordinary claims require extraordinary evidence*". In my opinion the evidence is insufficient. I also believe that the *length distributions* would have suited the intended purpose better than the mean lengths.

A formal point: our data consist of >2000 direct $\boldsymbol{v_T}$**-measurements** (track images can be submitted for inspection). These data contradict **model predictions** based on 27 mean track lengths. Does the scientific process not require that the model must account for the misfit with the data, rather than the other way around?

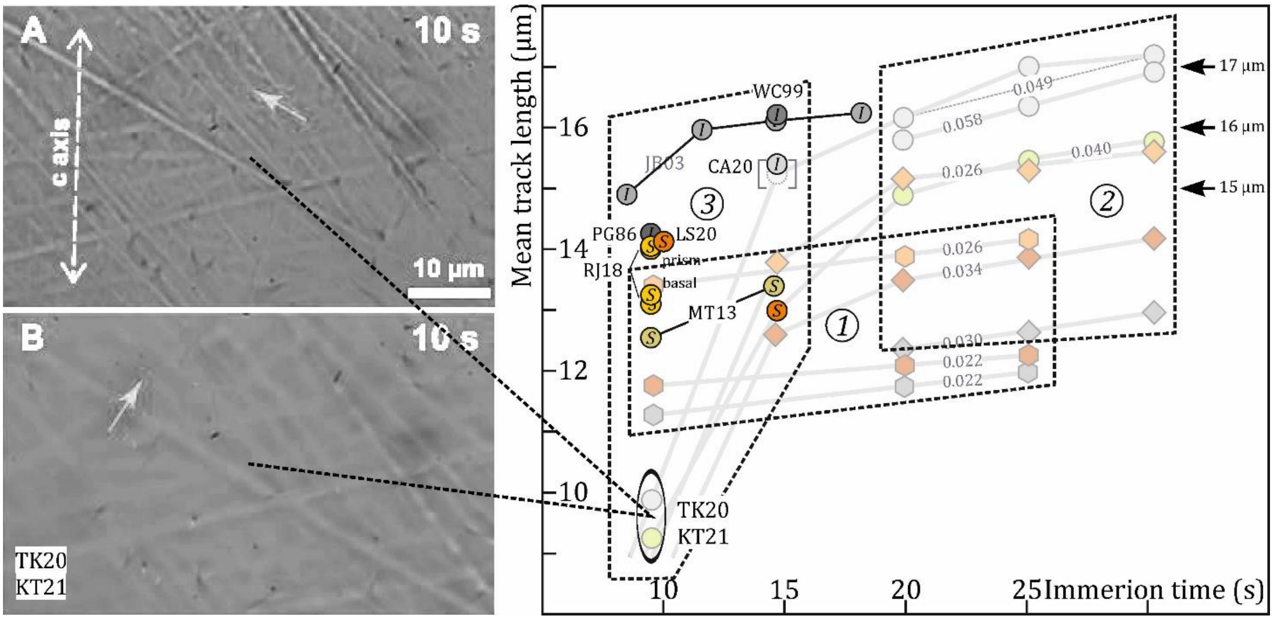

Figure 2. Left: images of unannealed induced tracks etched for 10 s (TK20); right: breakdown of the mean length data from TK20 to which the TK21 v_T-models are fitted. Additional data: MT13: Tamer (2013); RJ20: Jonckheere (2020); LS20: Sarkosh (2020); PG86: Green et al. (1986); CA: Aslanian (2020); JB03: Barbarand et al (2003); WC99: Carlson et al. (1999).

The Ketcham and Tamer (2021) variable along-track etching model is very simplified because it is based on mean confined length data from step-etch experiments, and with few data points to fit one can only test a simple model. That said, the constant-core etch rate model preferred by the author was the first one I derived and tested because I thought going into it that it would be the correct answer. It was the exercise of actually trying to fit the Tamer and Ketcham (2020) data (replicated in Tamer and Ketcham 2023, Chem. Geol.) that pointed to the linear model as adequate for most (but not necessarily all) levels of annealing. Shape data promises to be an excellent independent or complementary data source for deciphering track structure amidst the uncertainty stemming from not knowing when any given track starts etching, and it may well allow development of a more detailed and comprehensive model closer to physical reality. The author may be repeating my mistake in believing he knows the answer ahead of time, and thus limiting his field of consideration.

All models are simplified. KT21 (p. 438) "... *we neglect length and etching anisotropy* ...". Therefore all tracks have the exact same etchable (latent) length and all differences arise from when and where the confined tracks are intersected. This produces a continuum of track lengths between zero and the maximum length (I have never seen it). The greater part is culled using an operator bias function (evidence?) or tip roundedness (isotropic v_B!). That is fine, we gain valuable **qualitative** insight into various aspects of track etching from the most approximate modelling. But, I find it incomprehensible that one can expect to make accurate **quantitative** predictions, as claimed in these comments. I also do not see how TK23 supports the linear model since it shows polygonal track terminations (JA19; AS21) after ~10 s "effective etch time".

Finally, I note that the acknowledgements state that M.T. Tamer "made a substantial contribution to the measurements but desires not to be listed as a co-author." I gather that this was because he was being asked to be on a manuscript that both contradicted and ignored his own measurements, putting him in an impossible place. This is a shame, and should be remedied if possible.

I invited Murat Tamer to join me in an experiment which I had started (I had made the images), and taught him how to measure widths, cone angles, etc., all of which he did fast and well. After rather more work than I had reckoned, I submitted a manuscript with Murat as co-author. Against my advice, he requested the editor not to be listed as co-author. There was never a scientific disagreement, but I understand that there was a "conflict of interest", as described, i.e., co-authoring a paper contradicting an earlier paper with a different collaborator. Such events are unpleasant and, with age, I have become rather impatient, even intolerant, of research that is not in the time honoured tradition, but political and calculating. I would be glad if Murat Tamer would be co-author of this manuscript again if he has changed his mind, agrees with the content, and expresses the wish to do so. I understand that it requires no resubmission.

Detailed comments:

[line 10] The statement that the widest tracks "must also be the shallowest" is incorrect in two ways. First, width depends heavily on crystallographic ***orientation****, and so one needs to consider this within angular bins of some sort. Second, within a given angular bin, width depends on time of intersection by the etchant channel (i.e. effective etching time). A shallower track is more likely to be intersected early, but given a limited number tracks within a bin, one or more* ***deeper*** *tracks easily could be intersected earlier, and thus be wider. (Gleadow, 1980: but very important restrictions are imposed by the effects of anisotropic etching).*

No, it isn't; the shallowest tracks are not per se the widest, but the **widest** tracks are the **shallowest**. I had imagined that I had earned the privilege not to be lectured at on track width, anisotropy, or effective etch time.

[line 18] It's unclear whether the measurements described here actually tested for variation along the track length; only ***one rate*** *was measured per half-track in most cases.*

I can be clear that no such test was performed. Because the tracks are straight, ten measurements will give ten times the same result. What is the meaning of suggesting that we measure the variation of v_T, while implying that we cannot measure a one rate. What is the point of disputing evident track shapes to defend a mere model? I show straight tracks in the manuscript and in Figure 1, and can produce several thousand more images showing the same thing. What do these comments have to offer, apart from innuendo?

[line 102-103] Indeed, this is a significant bias. It might be clearer to just compare this to a 16-micron track (4.4 degree dip). It probably only has a small effect on this study, but is not something one would want to do when measuring unknowns.

Our data did not reveal a selection or measurement bias that might be related to the dip of the tracks.

[Figure 1] Needs scale bar or statement of image width.

Right.

[line 120] How do you know if there was a gap if you did not pierce it? It might eb better to state this as an interpretation, rather than an observation.

How do I know that the thing is a track at all? I see no reason to question the interpretations of Green et al. (1986) and Galbraith et al. (1990). In this case the absence of a normal polygonal termination is evidence enough.

[line 130] Again, if only one pair of measurements is made for a half-track, it's not clear whether or how that permits a change in etch rate along the track to be detected. It's also not clear how reproducible these measurements are with respect to (a) where one places the two circles along the track, and (b) how precise the circle margins are given the limited resolution of these measurements imposed by the wavelength of light. For example, some circles in Figure 1 go out to the edges of the blurry (resolution-limited) track

boundaries, while others are set noticeably within those boundaries. How does this affect the rate measurement? Has a ***multiple-measurement****, come-back-to-it-later study been done?*

None of that! Is it not more productive to get a grip on the basics than to indulge in virtuous statisticulating? Our manuscript stresses the fact that almost all our mean lengths and standard deviations are within <0.1 µm of their predicted values. In contrast, I understand KT21 (Figure 17) to mean that one can measure whatever one likes. Are we now debating how many circles fit on the edge of a track? Is this musical chairs?

[line 154] It's unclear what is meant by one "participant" versus the other, and what they both did. Did two people make all of these measurements (thus providing a ***repeatability*** *analysis it would eb good to report), or did one make all of the measurements and the other just check to make sure they looked OK?*

I made the images and Murat Tamer selected and measured the tracks he considered suitable for length measurement. Does it matter? Our manuscript explains that "... *the present is a one-way rate concerning a single set of images using one set of etching and observation conditions*.". His overall rejection rate was <1%. Have the track length measurements in TK20 and KT21 been repeated before a model was fitted to them?

[line 204] Unclear what is meant by "both projections", and which is the former versus latter.

The former projection is shown in Figure B1, the latter in Figure B2. I will add more explanation to avoid confusion.

[line 242] Not really; one must impose additional assumptions concerning ***monotonicity****, or let the uncertainty going back in time propagate to very large values.*

This refers to: "*This (assumption implicit in* ***c****-axis projections) allows to convert its (a sample) age and length distribution directly to a Tt-path, without the need to search Tt-space*". This describes something I implemented in a thermal history program. If all measured lengths are interpreted as mean lengths and mean lengths shorten but do not ever increase, the order of mean lengths is also the order of formation. Yes, the errors increase as the number of tracks that experienced earlier temperatures grows smaller further back in time.

[line 245] The reference is an abstract; I guess one can use it to claim that someone once said uncertainties can be taken into account, but it's not a source of information for how to go about it. In any event, how can there be a ***single solution*** *that is faithful to the uncertainty in both the length measurement (both from the measurement and from natural variation) and the time intervals (which are certainly not even)? This paragraph seems to drift off-topic.*

I agree about the reference, but I have nothing else. I will remove it, as the statement does not require much support.

One solution is no solution. However I found that, without exception, it was the backbone of a set of solutions consistent with the data within statistical limits. If it is off-topic, it is because I am astonished to realize that an assumption that I made for convenience 32 years ago is imbedded in modern modelling software. I thought it worth mentioning because at the time (like a forward model) it was the starting point for a random search or for a perturbation method in which either all or selected nodes were allowed to wander within given limits. Perhaps it has some value for alerting trackers to the consequences of ***c***-axis projection, for thinking of other algorithms for dealing with anisotropic lengths, or for developing new software.

[line 258-261] A strange statement; you've already corroborated that anisotropy is removed well at varying levels of annealing. The lengths measured for a given annealing experiment project to a narrow range, but the means are significantly different at different levels of annealing. The original length and orientations are what the projection is based on – they must matter, or else the distributions of high-angle tracks at each annealing level would not be so narrow.

This is an observation that can also be made from Figure 7 of Donelick et al. (1999). In the extreme case tracks perpendicular to the ***c***-axis, their lengths, e.g., in the interval 4.0-10.4 μm are funnelled into a 1μm ***c***-axis length interval. Length differences and measurement imprecision are therefore compressed by a factor of >6 (Figure 3). This does not contradict that ***c***-axis projection is effective at eliminating length anisotropy.

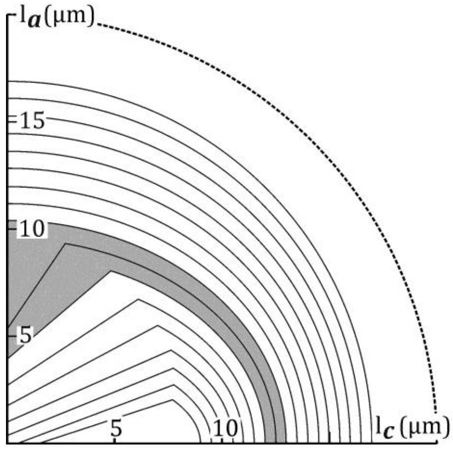

Figure 3. Illustration of the compression of the range of lengths of high-angle tracks due to ***c***-axis projection (after Donelick et al. 1999).

[line 270-271] An alternative possibility could be that the section designated l_3 may not always be completely etched, due to the delay.

Indeed, our data show that that this is the case (Figure 2d). But the difference between the total length (l_T) and that of continuous tracks is so small that we may conclude that no substantial track section is missing (Figure 4a).

*[line 275] It would be worth comparing this result with the recent report by **Li et al. (2003, EPSL)** of a gap at the center of each track upon formation. It's worth pondering whether there is really only one gap, and it's always at site of the original fissioning nucleus.*

The constriction resulting from track formation can be real or transient, but it has no effect on etching, or we would have seen it. Our data show that when it comes to annealing, the locus of the fissioned atom is not a preferred site for forming gaps (Figure 4c). I even suspect that our data are biased against gaps near the ends.

*[line 317-324] Another apparent oddity is that some many tracks at 0-20° and 85-90° apparently need to etch for the full **20 seconds**, which should be rare to impossible.*

Tracks sub-parallel and sub-perpendicular to ***c*** widen at the slowest rate (v_R) and thus require the longest etch times (t_E) to get over the threshold width ($w = v_R \times t_E$). In consequence, their etch time windows are narrow, meaning there are fewer of them, thus explaining the minima in the angular frequency distributions. In fact the range of widths ($\Delta w(\phi)$) between the threshold and the maximum is proportional to the relative angular frequencies ($F(\phi)$; Figure 4e-h). We do not measure the tracks that didn't make it, but those that did.

*[line 321-324] It's not clear whether this explanation makes sense; because etching along the penetrating channel is fast, there should be plenty of time for **deeper tracks** to grow wide and not be affected by the surface. This might be tested with the author's data – is there a correlation between track depth and width, or track depth and angle? This explanation predicts that tracks at 60-75° should be deeper, on average.*

This comment refers to the lack of tracks above constraint (4) in Figures 5a-d. There can be no discussion that they are the widest, as the vertical axis shows their measured widths. The question is why are there so few? What does it take to be overall the widest track? (1) It has to have the highest rate of widening (orientation). (2) It has to have the longest effective etch time, i.e., the shortest access time (immersion time - effective etch time). How can it have the shortest access time: how else than by being closest to the surface (assuming that the time for the etchant to bridge the gap between host track and confined track is not dependent on depth, or also increases with depth because the host track is widest at the surface)? One can also drop the first condition and conclude that the widest tracks in a given direction are closest to the surface.

We did not measure the depths; I understand that Murat Tamer has some data and further plans in that direction.

*[Figure 5, lines 327-328] The y-axis in these figures is **frequency**; it's not clear what they have to do with delta-w. Were the wrong plots put into this figure?*

The frequencies refer to (1) the histograms, (2) the long dashed line which is a fit to the combined frequencies for all samples, and (3) to the short dashed line which predicts the angular frequencies from the range (spacing Δw) between the most restrictive constraints in Figure 5a-d. Therefore, one axis fits all.

*[line 342-343] Do **longer tracks** attain a greater width before they reach their ends? Figure 5a-c seems to contradict this – tracks are shorter at all angles as annealing progresses, but average widths seem to increase.*

Yes they do. It is best to compare 5a and 5d which both have more than twice the number of tracks of 5b and 5c, and the greatest length difference. One should reflect that (3) is not a sharp boundary but depends on whether a confined track is intersected in the middle or at the end. At this stage I believe it is relevant that eq. (6) offers a first-order explanation for a phenomenon that was not even known before our width measurements. It is not helpful to obsess about details near the detection limit, unless one has a better explanation.

*[line 366] It seems like Tamer's step etch data can be used to test some of the implied assertions of the **model** in Fig. 6a-d. Is it really tenable that tracks at ~70 degrees are not measurable at effective times >13s when you can measure them after a 10s step, with some as long as 13 um (after only 3s of etching)? The model described here predicts that there should be a large deficit of tracks at 60-75° if searching for them at 10s and verifying that they are still present after 20s, but that is not the observation in Tamer and Ketcham (2020). Also, again, are tracks with effective etch times of over 18s realistic given the need to penetrate the polished surface (how deep are these >18s tracks)? This is another case where it could be interesting to check track depth varies with c-axis angle.*

Let me repeat that our length data are within ca. 0.1 µm of their predicted values. Unless one has concrete reasons to question our measurements, **the data are the facts**. That is how things are; whether that fits one's preconceptions is something each has to examine for himself; however, I would not question the data first.

Figure 6a-d shows that tracks at ~70° are measurable from 5 to 13 s, including at 10s. The proposed test is indeed interesting. However, we invested considerable effort in single-track step-etching, and I am loath to go back to mean lengths, or even length distributions, plotted against immersion times when we have effective etch times. I am the first to admit that our data are noisy, but that does not make them wrong. I believe that experience has shown that 10 s immersion times are worse than useless for Durango apatite (Figure 2).

I beg not to refer to our work as "assertions" based on a "model", which I find abhorrent; the constraints are tentative interpretations of actual data that make sense to me, and are here presented to the reader for discussion.

*[line 432] It seems worth asking whether track rates really vary by a full **order of magnitude**, or the uncertainty of the rate estimation has something to do with it, or possibly because track etch rate varies and they are trying to measure etch rates along different sections of tracks. For example, a prediction of the Ketcham and Tamer (2021) model is that the etch rate for the s_1 track sections should be faster than for the s_2's; this seems to be the case in Fig. 1g at least. Plotting these against each other seems like an easy test to try.*

Probably not. As the manuscript explains (lines 433-437), a large part of the variation is likely due to the statistical error on the cone angle measurement, which appears in the denominator of the v_T-equation. Hence the overdispersion and the right-skewness of the distribution, and why we recommend the harmonic mean.

Forgive me if I am growing impatient with the insistence that our results should be measured against the TK20 and KT21 "benchmarks". The model for unannealed induced tracks is based on eight mean track lengths plotted against immersion times; one measurement is questionable and another has been excluded. The data and model predict (1) an isotropic apatite etch rate, (2) tracks that have no intrinsic lengths, (3) length distributions that are the exclusive outcome of etching, (4) accelerated etching after annealing, (5) a linear v_T-model dependent on an *ad hoc* observer bias and an invalid tip roundedness criterion, all on the merit of χ^2-values that can barely distinguish between a constant-core and a linear model.

Is it not established procedure to **evaluate a model against the data** instead of vetting the data based on a model? When there is disagreement, should we uphold the model and reject inconvenient data? Really?

*[Figure 8] This figure demonstrates a **mathematical error** in how the author has tried to reproduce the model of Ketcham and Tamer (2021). The graph at the top of the figure has the track end at vT=0, rather than v_T=v_B. The result of this error is seen most obviously in **Fig. 8c**; the contours indicate that the along-track rate is less than the bulk rate, which contradicts the Ketcham and Tamer model.*

True; Figure 4 shows the corrected model in red and the original one in Figure 8 in black. It also indicates the data range on which the model is based except for the 10 s measurement, which is unconvincing in my opinion. There is indeed something amiss with the left tip of the track in Figure 8c, which I will of course correct.

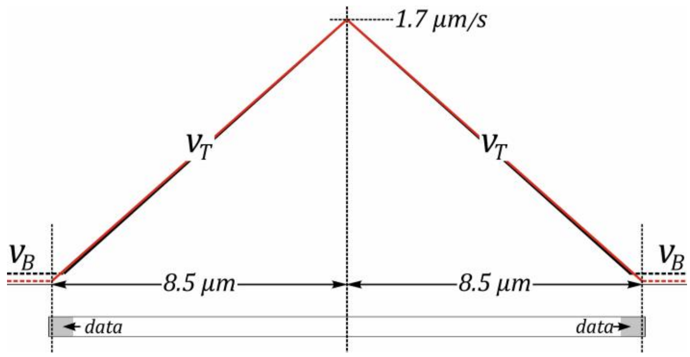

Figure 4. Original (black) and corrected (red) linear v_T-model used for calculating the progress of the etchant along an unannealed confined track in Durango apatite (5.5 M HNO_3 at 21°C). The shaded areas indicate the database for the vT- model, excluding the 10 s (wrong?) and 15 s (not used) mean track lengths (TK20 Table 3).

This error should however not distract from the significance of Figure 8, which is undiminished. That is to illustrate that a linear v_T-model produces tracks that are never observed, either as confined tracks or as surface tracks. The confined tracks can of course be disappeared by a nifty selection criterion; that is the advantage of models. The point is however that it must also agree with the experience of trackers **at the microscopes.**

*[line 450-451] "... except for perhaps 1 μm at either end" seems to admit that **track etch rate does vary**, as the thinning of the track toward its tip is impossible to miss. Furthermore, although Fig. 8 has mathematical problems, it's notable that the only place where the variation in etch rate is similarly impossible to miss is in the last μm or so before each tip.*

Indeed, "v_T" decreases, or the track would be infinite. How this comes about is not clear to me. I propose that there is a first phase of staggered etching (JT17) which causes narrowing and rounding, and a later phase when the tips are terminated by faces with the lowest etching apatite etch rates (v_R; AS21). Depending on whether one considers individual tracks or the average of a population: this can be interpreted as a decreasing v_T (TC19, TK20, KT21), a transitional rate v_L (Laslett et al., 1984; AS21, JA22) or, based on single-track step-etch data (JT17), erratic length increments of individual tracks due to closely spaced "gaps". This seems to have some support from electron microscopic observations (Paul and Fitzgerald, 1992; Paul, 1993). One can discuss forever whether real tracks look anything like their predicted shapes in Figure 8.

I have a related question: in TK23 polygonal track tips appear shortly after ~10 s "effective etch time". How were the effective etch times determined and how does this square with a linear v_T model and isotropic v_B (TC19; TK20; KT21)? Is this not as flagrant a contradiction between model and observation as Figure 8?

*[line 468-469] Is such curvature unobserved, or just **unobservable**? What curvature there is predicted by the model along the midsection of the track is extremely subtle, and arguably beyond the resolution of optical microscopy, with its diffuse track edges…*

"Unobserved", in the sense that in five decades of confined track measurements, no one has reported a shape like in Figure 8. I have several thousand images of confined tracks (AS21; JS22) but not one like in Figure 8, not even when the apatite etch rate perpendicular to the track is more than twice the assumed isotropic value (AS21).

How am I supposed to respond to an assertion that "there is something there, but you cannot see it"?

*[line 478-480] … for example, the white outlines in Fig 9a,b are sometimes on the inside edge of the blurred region, sometimes on the outside edge. One could draw a curved line on the left side of Fig 9b that is a **scaled version of 8c**.*

No.

*[line 480] **Excess** compared to what? I note that in Fig 9e there are several shorter, under-etched tracks at the edge of visibility, some marked with white arrows and some without. What baseline is the author comparing to?*

This refers to: "*A linear v_T model creates an excess of underetched tracks, e.g., whenever etching starts at the end of a track or its effective etch time is less than the immersion time.*". "Excess" refers to the >80% of **modelled** tracks that are judged to be underetched, compared to those deemed acceptable for measurement. I gather from the v_B/v_T-condition that they are "observable", but excluded; have these tracks been "observed"?

*[line 490-492] A bizarre but clarifying assertion. In the Ketcham and Tamer (2021) model, **v_T approaches v_B** at the ends, not zero. Only in the author's attempt to reproduce it does v_T approach 0.*

I admit the mathematical error and will correct it (Figure 3). I must however take back two silent concessions that I made: (1) the access time to α is 6.7 s (instead of 6.0 s); (2) the cross-over from α to β takes 1.3 s (instead of 0 s). The length of β in Figure A3 should be 15.4 µm; the correct value was used in the calculations.

*[line 554] **Accelerated length reduction** was posed as (and still is) an intentionally generic, non-interpretive term that encompasses gap formation but leaves open the option for other possibilities.*

I accept that that is the intended meaning. However I think it is time to come down from the fence and let go of "**other possibilities**". "*I am sure there are still plenty of mistakes in the theory I will offer here, and I hope they are bold ones, for then they will provoke better answers by others*", Daniel Dennet (1983). Prophetic words!

*[line 591-592] And yet such an increase is very clearly present in Tamer's data (10s experiments in Tamer and Ketcham 2020b, Fig. 1). It's not terribly scientific to simply **ignore the data** the contradicts one's conclusion. Can the author provide at least a hypothesis for the incompatibility between the step etch data and those presented here? Which data are more reliable, confined length or inferences from circles on blurry outlines?*

I am confused. I thought I was the one with the **data** (2000+ single-track lengths, widths, angles, ...), and TK20 and KT21 were the ones with the **model**, suspended on four to seven mean track lengths (not even length distributions, let alone single track lengths; forget the angles), mostly related to the final 1 µm length increase.

I repeat that the 10 s mean fossil track length (9.11±0.3 µm; TK20; KT21) is inconsistent with an earlier measurement by Murat Tamer (12.5±0.2 µm), as it is with other measurements, including mine (Figure 2). This also applies to the induced tracks where the one measurement that has been confirmed (CA20) is discarded.

[line 593-595] It's not clear what an "excess" of confined tracks has to do with the projected lengths of ***surface-intersecting tracks****, the vast majority of which start etching immediately.*

A linear v_T model must produce a massive excess of tracks with lengths between zero and maximum, that are never seen. As far as the confined tracks are concerned, they can be "disappeared" using an *ad hoc* operator bias or tip criterion (KT21 Figure 8). Who can protest? But that does not work for surface tracks, such as ones between Figure 8c and d, which are short and pointed, even after a full immersion time. These should produce an excess of tracks with short projected and etchable lengths, but instead there is a deficit.

[Figure 10, Figure A3, line 510] These figures also show the author's ***mathematical error****. Calculated track etch times are much too long for the linear model, because the author assumes a v_T of zero at the track tip, rather than v_B. There is no explanation for how 6s was determined or estimated to be the time required to start etching the first track. And, I have to say, the track segment next to the alpha sure looks like it has curved walls to me, and not just in the last micrometer…*

I corrected the mathematical error and did a quick calculation. I am afraid that the news is not good for the KT21 model. But leaving the numbers aside: how serious is a v_T model that needs to be "saved" (or not) by appealing to a bulk etch rate v_B (0.022 µm/s) which exceeds v_T over 0.11 µm at each end of the track. The 6.0 s access time is calculated from the maximum width of α and the apatite etch rate perpendicular to it. This gives its effective etch time, which, subtracted from the immersion time, gives the access time (6.7 s).

The track next to α is a dipping surface track; I leave it to the experts to evaluate the significance of its "curvature".

It's very worth noting that this is a beautiful ***track-in-track-in-track*** *(TINTINT) image, the first I'm aware of in the literature, and poses an excellent test for the Ketcham and Tamer (2021) variable along-track etching model – can all that etching occur in the time given? I've attached a spreadsheet that demonstrates that it can, though not if it really took 6 seconds to start the first etch. If one drops that time to* ***2 seconds****, the model predicts the position of all track tips to* ***within 1 s*** *of the end of etching at 20s. One can also increase the start time a bit by making some other assumptions that are within the measurements. It also bears mentioning that Tamer and Ketcham (2020) may not have measured a track such as beta, because the top tip is very indistinct. In any event, it would be good to know the basis of that 6s determination, short of which I'll assume for now that my model passed this test.*

What in heaven does it prove that one can (almost) obtain the desired result by changing the input data? What difference does it make if TK20 would have measured β or not? It is there and it is etched; that is what matters.

I have a more serious suggestion: let's for the sake of discussion assume that the maximum etchable length (L_{lat}) of the fully etched track β (experts can examine the image stack) is not the default 17 µm, but 16 µm. This alone raises the time to reach its furthest endpoint by almost 10 s, showing how unstable the model is.

[Appendix A] Here the author has attempted to derive a simpler set of equations than those listed by Ketcham and Tamer (2021) for their model. There could well be a more elegant formulation than mine (to paraphrase Dutch grandmaster J. van der Weil, "I am a butcher, not an artist"), but these are not yet it owing to assuming that ***v_T = 0 at the tip****. One obvious place things are wrong is the case marked (1) va = c a , which should be va = c a + v_B, and similarly (2) should be va = c(l/2 – a) + v_B. How the author calculated c, though not spelled out, was probably also a bit off (should be (vmax – v_B)/(l/2)).*

I changed the *c*-value from 0.200 to 0.197 and calculated, as before, with v = max(v_T, v_B), although the latter should have no effect on Figure A3, as $v_B > v_T$ over the last 0.11 µm of the 17 µm latent tracks (Figure

4)? Appendix A would then correctly describe the v_T model, as such, rather than an inapplicable (v_B, v_T) model?

[Figure A2] The distances between the dots are not consistent with each other; if one measures them in pixels, there seems to be about a vertical ***distortion*** *of about 5%, in both Figures A2 and 10, assuming the markers correspond. The measurements are provided on another tab in the attached sheet. It's not clear if this was because the picture was subtly and inadvertently scaled unevenly at some point before or when it was pasted into this document; since both images are distorted by a similar amount, I'd guess it was at some earlier stage in the process.*

I have no knowledge of a distortion of Figure A2 or 10. I captured the images with the same microscope and camera as the other tracks in this work and measured them with the same technique as Murat Tamer used for the others.

I have measured confined tracks for more than 35 years, without, I hope, producing too much nonsense. I believe therefore that I may appeal to colleagues familiar with my work to vouch for me that I can measure tracks.

R. Jonckheere
Freiberg, 26.07.23