Geochron 2023-13

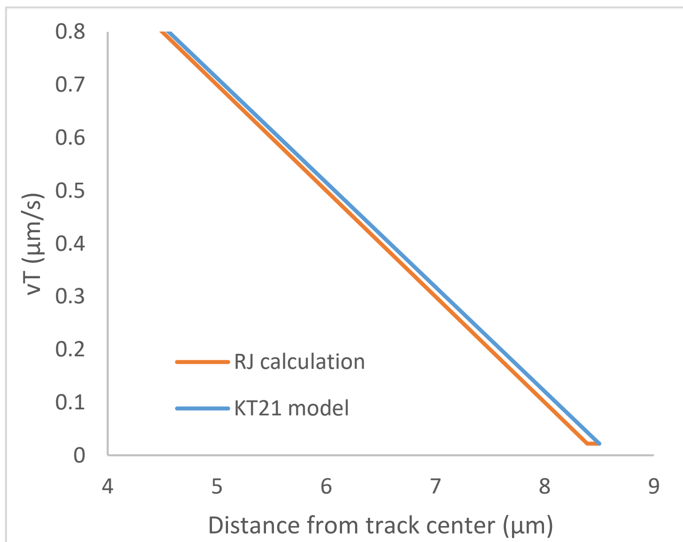
Response to R. Jonckheere (AC1)

Richard Ketcham and Murat Tamer

As with Dr. Jonckheere's judgement on our previous contribution to this journal (KT21), we recommend publication of this work, even if we have quarrels with it, though we hope more can be fixed as described below. Our preferred solution would be for a few assertions to be moderated, and for the applicability of MT's related measurements to be considered and discussed rather than outwardly ignored and implicitly contradicted, and the original authors come to mutual agreement about the content of the paper, rather than one insisting on his own way and brooking no dissent. Failing this (or either way, really), we would request that publication be conditioned on making all data available.

Following are some final responses for this discussion. Comments by the author are in red italics (except those in gold, where he was quoting our previous comments), and do not cover every point of discussion, but are presented in the original order.

"I rederived the equations because I was interested in the preferred linear vT-model, and wished not to complicate the equations with vB, as vB > vT over 0.11 µm at the ends of unannealed induced tracks. I did, however, consider vB in my calculations by setting vT = max(vT, vB)."



This accommodation is not sufficient to match the KT21 model, as it misrepresents the modeled etch rate along the entire track; the final 0.11 µm should not etch at vB, but at a higher rate. When graphed (right) the difference may seem insignificant, but in practice it makes a substantial difference in calculating length as a function of time. To use Dr. Jonckheere's shortcut, the latent length could be increased to make sure the etch rate matches vB at the same distance where the KT21 model does, in this particular case 17.22 µm rather than 17.0. In his response, RJ changes the c-value (rate slope), but it's not clear if this completely corrects the problem; Figure 4 of the response remains incorrect, because the red curve should be at vB after 8.5 µm, not at some lower velocity that needs to be corrected.

"For discussion, it is perhaps useful to explain the disagreement with TK20 and KT21. (1) The manner of our vT-measurements assumes that vT is constant along most of the length of a fission track. If it isn't, and vT varies along a track, as the linear model requires, then our data are meaningless."

We agree that constant vT is an assumption, and we disagree as to whether it is a necessary or well-advised one. All the same, it does not render the data "meaningless", but a kind of average, depending on the points selected for measurement.

"(2) In contrast to TK20 and KT21, our samples provide no evidence for an increase of the track etch rate vT following partial annealing."

Whether the samples provide no evidence is not certain – the author's analysis presented may imply this, but the analysis is not exhaustive, and is affected by the preconceptions that have gone into it. This is why we support publication, but with the requested condition that all underlying data be made available.

We note that MT also measured several hundreds of spontaneous tracks in Durango apatite as a part of this study, but for unknown reasons these data have not been included in this manuscript. Ideally, these would be made available as well, or at least to MT for his work.

"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 vT 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."

It is unclear what "can be distinguished under a microscope" means in this context; exactly what is being distinguished? We also feel that the lines drawn on top of the images does more to obscure than illuminate – they present an interpretation, but mask the actual image data. We agree that the uncertainty in the measurement is about the same as a length measurement, but as a proportion of the quantity being measured it can be substantial. For example, Carlson et al (1999) report measurement precisions of individual track lengths of ±0.15 µm, after a thorough self-examination using remeasurement. This uncertainty is negligible for a track length of 16 or 8 µm, but much more substantial for measuring track widths on the order of 1-2 µm, and even more so for utilizing the difference between widths to calculate an angle.

"Our data result from direct vT-measurements, compared to model estimates based on mean track lengths."

Well, to be precise: these are not direct vT-measurements, but measurements of widths and distances that are used to calculate vT with a derivation that assumes it is constant, and interpreted with an elegant model of etching anisotropy that nevertheless may also embed assumptions of constant etching velocity.

"The 20-30 s data of all the samples relate to the final ~1 µm length increase of the tracks. I[t] thus appears that most of the vT-model rests on the 10 and 15 s data in block ③."

This interpretation of appearance is not accurate. The 20-25 s and 25-30 s etch steps are also influential in that the non-annealed samples do not reach the bulk etch rate indicated by the annealed samples until after 20 s or even 25 s, depending on when measurement began.

"The large difference between the 10s mean lengths of the annealed and unannealed samples underlies the assumed increase of vT 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."

We are particularly troubled by the tenor of this comment, and similar ones in this response (e.g., "(wrong?)" in Fig. 4 caption), which is the main thing that necessitated this response. As pointed out by KT21, early-step length measurements in step-etch experiments are inevitably on under-etched tracks, and so it's unclear what drives the choice as to which tracks to measure and which to bypass. It is likely that for most analysts that have made such measurements, the criterion is simply the somewhat amorphous "best tracks that can be found"; this was the case for the measurements labeled MT13. However, the goal of the TK20 study was to find as many tracks as possible so their individual growth trajectories could be traced. This resulted in a much more thorough and meticulous search procedure, finding tracks at the edge of visibility only discernable by racking the microscope focus, or flipping through stacks of captured images (see video in TK20 supplemental material). It is likely that no previous studies used such a protocol. Insofar as these barely-visible tracks were shorter, they lowered the mean and increased the standard deviation compared to what other analysts have previously reported.

We also note that MT used the same preparation procedures, and attempted to maintain the same selection criteria, throughout data acquisition for TK20, although he did vary his imaging conditions depending on whether he was measuring in the evening (red/black for induced tracks) versus daytime (white/black for spontaneous tracks). Additionally, prior to commencing these measurements there was no expectation as to what they would reveal that could lead to implicit biasing. We thus maintain that these are quality data, and any accusation that they are "unsafe" would need far stronger evidentiary support.

It is this undertone of distrusting MT's related and pertinent data that make the paper, as written, unsuitable for MT to participate as an author. We still believe that this paper can be an excellent scientific contribution with MT getting the credit he deserves for performing all of the measurements, by removing these undertones and allowing for more flexibility, modesty, and completeness in how the data are interpreted and compared.

We further note that the points flagged by RJ are for experiments where some degree of constant-core etching structure was supported by KT21. Assuming RJ approves more highly of the data for the annealed-apatite experiments (in block ①), those are the ones that were just as well or better fit by the linear model as a constant-core one. However, even in this case we note that KT21 reports that a constant-core structure is permitted by the data (their Fig. 13); it's just that the data supported the simpler model, and that's what we reported.

Finally, the JB03 data plotted on Figure 2 of the response are for apatite from the Fish Canyon Tuff, which is more soluble than Durango apatite and thus not suitable for comparison in this context. Some of the other points we cannot comment on, as we're not sure where they are from (RJ18, LS20, the second circle with the same color as LS20 at 15s with a shorter mean).

"I submit that this is related to the substandard microscope images (Figure 2; Figures 9 of TK20 and KT21)."

We have always used the original images without changing the colors in our papers. Conversely, Figure 2 in RJ's reply is a modified version of Figure 9 in TK20, where the track tips are even less visible than the original image. The intention of the original figure was to show the evolution of track etching with progressing etch times, and did not use this level of magnification for the measurements. The original figure of our paper, and the animation we provided, better represent our measurement conditions.

Nevertheless, it is indeed possible that better microscope images (mount preparation, polishing, etc.) would have revealed somewhat longer detectable lengths for some 10s tracks measured for TK20, which could in turn have affected how the KT21 model fit the data. However, we don't believe that this difference would have been enough to overcome the general conclusion that track etch velocities diminish toward their tips, and in particular that unannealed spontaneous and induced tracks tend to be under-etched after 20s, as that conclusion is based on the later etching steps. We also don't believe it would have affected the observation that unannealed track populations featured a higher preponderance of short early-etch tracks than annealed ones, leading to a lower mean length and slower implied etching, given our efforts to keep our data comparable among each other.

"I also believe that the length distributions would have suited the intended purpose better than the mean lengths."

This objection is unclear; KT21 did calculate length distributions, but used the means of the calculated distributions to summarize results and optimize model parameters. Length distributions are provided in the supplemental material.

*[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.

This statement inappropriately presupposes that the answer is known; it is circular reasoning. It is also absurd in itself, as the author acknowledges that the measurement uncertainty is substantial.

*[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.*

We do not disagree, but reiterate that TK20 report data, data measured by the same person who made all of the measurements in this work. Those are facts as well.

To clarify on the scientific point, if one can measure a 70° track after 10s, and then measure the same track after 20s, 25s, and 30s (as TK20 did), then it seems highly likely that its effective etching time exceeds the ~13s allowed by RJ's model (or, "tentative interpretation of actual data") in Fig. 6a-d.

"Forgive me if I am growing impatient with the insistence that our results should be measured against the TK20 and KT21 "benchmarks"."

We don't know where this statement comes from. We never use the work "benchmark," and never imply that these results should be "measured against" prior ones. However, all scientific reasoning is always improved by considering applicable, independent work that can corroborate or contradict. Moreover, the "model" here is one that the author is already mentioned many times, so it seems fair game to propose an actual to-the-point test for it, rather than relying on assertions.

"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?"

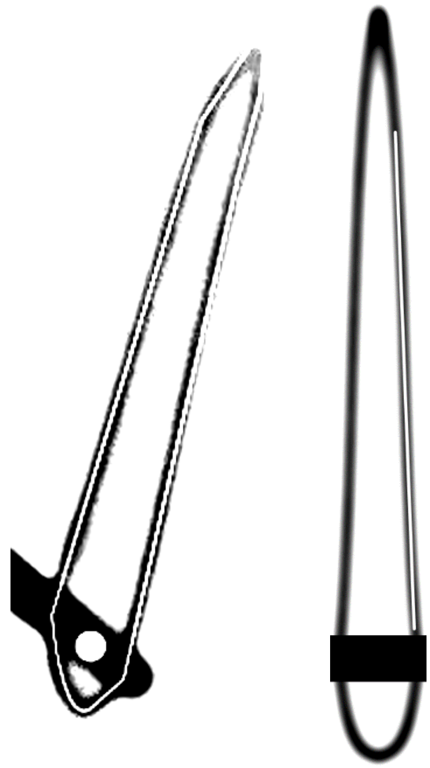
Again, the proposal is not to vet the data against the model, but to look at the data in available, more pointed ways to see whether it contradicts (and heck, possibly disproves) the model. Concerning rejecting inconvenient data, we point again to the TK20 data (2 comments ago), which may or may not be inconvenient to RJ's thesis, but deserves mention, consideration and discussion in a paper where all measurements were made by the same individual.

"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 vT model and isotropic vB (TC19; TK20; KT21)? Is this not as flagrant a contradiction between model and observation as Figure 8?"

We need this question to be much more specific to address it; does it concern the new data in TK23 (annealed tracks, with more complete experiments than in TK20 and giving an overall result consistent with his others), or the schematic diagram in its Fig. 1? If the latter, it is based on MT's own observation of thousands of confined tracks; the diagram is **schematic** in many ways, and does not take into account all applicable variables, such as mean etching rate and track length, where indeed one would expect shorter and/or faster tracks would reach polygonal tips in less time.

*[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.

Unnecessarily curt. For the sake of dialogue, we attempt to roughly illustrate the point. The first diagram to the right is Fig. 9b from the manuscript, with contrast adjusted to emphasize the blurred track edge and interpretive line. If the track were truly straight, it should be possible to draw a straight line that shows the same amount of blurring to either side, or in other words to have the blurred track edge be symmetric about the straight line. This appears hard to do; starting from the bottom and going up, the right-side line begins to the right middle of the blurred region, strays to the right edge, and ends up back toward the middle, implying a very slight curvature. Similarly, the left line also cannot stay squarely in the middle of the blurry track boundary, appearing to go center-left-center-left. On the right, we contrast this with a version of the outer modeled track in Fig. 8c, blurred to approximate the appearance of a track under a microscope. Here, after blocking out the part intersected by the etchant channel, we can also draw a straight line along the simulated track that appears to stay in the middle, and only close inspection shows that it subtly varies its position within it.



Now, these possible appearances of non-straightness in the real track image may be a manifestation of other phenomena, such as refraction, 3D etched track shape, imperfections in the microscope image, variation in track depth, etc. However, it seems clear that very little curvature is really implied by variable along-track etching rates, and within the context of the blurring and noise obtained from even high-quality optical microscopy, it could be missed, especially if one is convinced ahead of time that it is not there.

Lastly, one new question: are the “s” measurements (e.g., Fig. 1e) the distance between the centers of the circles, or along a line that is tangent to both circles? We had thought the former, but then it seems that Equation 3b should use an arctan, not an arcsin. Or, perhaps there is something else we are not understanding about the equation.

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