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Interactive comment on "Confined fission track revelation in apatite: how it works and why it matters" *by* Richard A. Ketcham and Murat T. Tamer

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Review of gchron-2020-31

Confined fission track revelation in apatite: how it works and why it matters Richard A. Ketcham and Murat T. Tamer

General Comments

This paper reports on new variable VT fission track etching models for apatite and considers in detail its implications for confined fission track length measurements and their application in thermochronology. The models involve a number of clearly acknowledged simplifying assumptions, such as omitting the effects of anisotropic etching and annealing, but nonetheless provides a number of important insights into track etching behaviour and explanations for a range of previously observed phenomena. The title is an apt description of the content. The models are then applied to an existing empirical track length data set and achieve considerable success in explaining their characteristics. In my opinion the paper represents an important advance in our understanding of fission track etching behaviour with significant implications for practical applications in fission track thermochronology.

In essence, the paper explores the consequences of varying the track etching rate, VT, along a fission track rather than being constant along its entire length. It does not purport to be a fully comprehensive model of the detailed form and distribution of etched tracks in apatite, along the lines of the models being developed by Raymond Jonckheere and his colleagues, but rather studies whether variable VT produces first-order predictions that can be usefully compared with observations. In this, I think it has been remarkably successful and has made a strong case that such variability is both real and has a significant influence on measurements. The particular form of the simplified models considered are somewhat arbitrary but conform broadly to what is known about track structure in apatite and other minerals. In fact, because the models do not include the specific crystallographic characteristics of apatite, the implications could probably also be generalised to track etching in other minerals.

In order to understand the revelation of sub-surface confined tracks, the paper also considers the etching characteristics of surface semi-tracks from which they are progressively intersected in TINT measurements. The study represents the first quantitative exploration of the implications of this two-stage etching process, which has previously been understood only in the vaguest terms. This semi-track modelling component of the paper leads to its own conclusions, independently of their role in revealing confined tracks, such as the estimates made in 6.7 of the track counting efficiency for apatite. Doubtless these estimates could be refined by including crystallographic anisotropy in more complex models but the first-order agreement with independent estimates is very encouraging.

Another outcome of modelling the joint development of semi-tracks and confined tracks is to show that the great majority of confined tracks are intersected at shallow depths (section 4.3, Fig 4), with the consequence that most confined tracks will necessarily lie at low dip angles in order to avoid intersecting the surface. This explains why the usual restriction to measuring only 'horizontal' confined tracks, usually taken to mean dips of $<10^{\circ}$, actually includes a high proportion of all the available tracks. It is noted in line 158 that a confined track dip of 25° is close to the maximum observed in the Tamer and Ketcham 2020b data set and a similar observation is a significant feature of our own dip measurements on confined tracks in a range of samples (Li et al, 2018 Amer. Mineral. 103, 430) which show that $\sim$70% of confined tracks in a range of apatites have dips $<10^{\circ}$ and virtually all are $<30^{\circ}$. The model also explains the observation of Li et al. that confined tracks with the greatest dips have slightly shorter mean lengths, probably due to a greater average intersection depth and later etching start times.

The first major conclusion of the paper that VT is indeed variable along the length of fission tracks is, I think, well-made and unlikely to be controversial, indeed such variability has long been accepted by many, but without any clarity as to the implications. The second group of conclusions about laboratory annealed tracks etching faster than either fossil tracks or freshly induced tracks is more surprising and unlikely to be readily accepted at face value. Given that these are based solely on the modelling of a very limited data set, and the simplifying assumptions involved, I would suggest at least a more cautious and tentative statement of these conclusions acknowledging such uncertainties. The result is certainly interesting and points to the need for further study to be certain that this is a general phenomenon and not an experimental or modelling artefact.

One very important conclusion is that the very poor reproducibility of track length measurements in previous interlaboratory comparisons is probably centred on just one factor – the criteria used by analysts to determine which tracks are acceptably etched for measurement. Previously, the observed discrepancies could be attributed to a range of possible analytical, calibration and training factors, compounded by the difficulty of standardisation in track length measurements. The new model results, however, suggest that attention should be focused primarily on standardising track selection criteria in order to reach a much-needed concordance in length measurements across different laboratories.

Having said all this, I actually found the paper quite complex and surprisingly difficult to read in many places. I spent considerable time trying to understand some sections and some of the diagrams, despite considerable familiarity with the subject material. I think the overall readability needs to be improved if the paper is to be accessible to a wider audience. I make some specific suggestions below, but I think there are some general principles that could help.

First, a number of unfamiliar terms are used based on the geometry of the tracks which would be greatly clarified by an additional diagram where these are labelled. These terms include 'Impingement point' in Fig 2 (maybe 'Intersection point' would be clearer), the 'Relative semi-track' penetration and 'Confined track penetration' in Fig 3 and 'Intersection depth' (Fig 4). While these are defined at various places in the text, a diagram that showed them all in one place would greatly assist the reader in understanding the following diagrams. Perhaps this new key diagram might include a latent and etched semi-track and one or more confined tracks intersected at different stages of development, annotated with all the parameters used in the subsequent discussion.

Second, some basic information is simply missing such as the etchant used – presumably this was 5.5M HNO3, but this is not actually mentioned anywhere in the paper. It is not reasonable for the reader to have to look up the original source paper to know this, especially when etching conditions are actually part of the discussion and the etching times are central to the text. There is also no indication of how the track densities might have differed between the different experiments and which ones were Cf-irradiated,

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which must exert a major control on some of the parameters, such as the number of intersections per track.

Third, the paper also assumes that the experiment codes (SE1, SE2 etc) are sufficient to alert the reader to the particular track characteristics involved. These are summarised in Table 1, but even there they do not appear to be complete. For example, only one of the experiments (SE3) in Table 1 is indicated to have involved Cf-irradiation, but it is apparent from the text that several others at least also involved this procedure. It is very hard to keep all of the different experimental details in mind when looking at the results, making them more difficult to comprehend. A good example is Figure 12, where histograms of the intersection depths are given for each experiment. There are clear differences between these, but it requires considerable work of detection to figure out how these relate to the track characteristics. Perhaps these histograms could be grouped under sub-headings or annotated with ‘spontaneous’, ‘unannealed induced’, ‘annealed induced’ so that commonalities in the different groups would be obvious. More explanatory and clearer figure captions would also help.

Another example is Figure 13 B and D, which show the etching start time for the modelled confined tracks, but the discussion arising from this diagram is about the track etching time thereby requiring the reader to make a mental inversion to make sense of the diagram. Why not simply plot the inverted track etching time (20 sec, minus the start time).

In summary, I believe that this paper is an important contribution and provides a number of significant insights into track etching behaviour that have important implication for the practice of fission track thermochronology. I recommend acceptance for publication after significant revision.

Specific Comments:

Line 105: ‘Only simple modes are justifiable at . . .’

Line 125: '. . .shows the development of total confined track length. . .' (is this the intention?)

Line 139: '. . .of lengths, dips, and. . .'

Line 201: Is this 'power-law increase' arbitrary, based on the model, or derived from the actual measurements?

Line 206: '. . .corresponding case for standard track selection'. Also, what does 'standard track selection' mean here?

Line 207: I see how a selection criterion of VT/VB<=12 can be applied to the modelling results, but how can such a criterion be applied in practice in anisotropic crystals, where the form of the track tips are determined largely by fast and slow etching facets (Jonckheere et al 2019), rather than the idealised tip shapes in Fig 5.

Line 210: '. . .also predicts the intersection depth and etching. . .'

Line 211: '. . .distribution of the modelled track lengths. . .', also 'It is clear that. . .'

Line 218: '. . .first a semi-track length distribution. . .'

Line 219: '. . .then a distribution of confined track lengths. . .'

Line 221: '. . .function is the reduced chi-squared. . .' (or 'a' – still a clumsy sentence)

Line 241-2: '. . .were exerting a disproportionate control. . .'

Line 242: '. . .for experiment SE2 feature a very similar mean. . .'

Line 243: The expression of this whole line is clumsy and difficult to understand.

Line 278-285: I am really not sure what this paragraph is trying to say.

Line 299: '. . .impingement happened first for each track, where more than one is present.'

Line 344: 'both revealed using Cf semi-tracks' – this is not indicated in Table 1.

Line 454-5: I have no problem in considering a change in etching protocol and I think achieving standardisation in this area should be a community-wide objective, but the aim of ‘allowing tracks at all levels of annealing to etch more completely’ is probably impossible to achieve in practice and the concept of ‘fully etched tracks’ likely to remain a mirage. The disparity in compositionally controlled etching rates is so great between grains in many samples that it is essentially impossible to achieve an identical degree of etching in each. I think standardised procedures and calibration of the degree of etching is a more achievable goal.

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University of Melbourne, 14 January 2021

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