Geochron 2023-13

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

RC1: *anonymous reviewer comments, August 1, 2023*

AC2: replies by Raymond Jonckheere, August 1, 2023

*This is a very **interesting** and **important** paper, in principle suitable for publication in Geochronology. The author examined the confined fission tracks (FTs) in apatite in detail, with a particular focus on the shape and width of etched tracks. He analyzed four samples of Durango apatite annealed at different temperatures, and measured geometrical parameters (length, width, angle to **c**-axis, etc.) of horizontal confined FTs by utilizing induced FTs in pre-annealed apatite. This is an **original** investigation with **unique** geometrical analysis of confined FTs at different stages of annealing, and thus has an important **impact** on the FT thermochronology.*

However gratified I am by the reviewer's praise, it would be more convincing if he did not fall over himself to **reject** our manuscript at the end of one page of vacuous comments, without having troubled to read the actual content.

***However**, I found following important issues/pitfalls in the present manuscript that should be treated appropriately before publication:*

*(1) First of all, there is no description about the assessment of uncertainty of individual data in the geometrical analysis, particularly the width of FTs, which is likely the key parameter for reliable confined FT length analysis. I agree with the author's point of view that the assessment of track width is the key, but then, the author should explicitly describe in the text the uncertainty (i.e., **accuracy** and **precision**) of track width measurement.*

This is not a genuine comment; I cannot think of a substantial fission-track publication ever in which this has been done.

As suggested before, I am prepared to upload images of all the confined tracks with the measurements as shown in Figure 1.

*(The author merely gives in Table 1A an "Error" of **0.01 micrometre** (= 10 nm) which is amazingly small for optical microscopic observation.) Otherwise, it may result in the overinterpretation of the obtained data within the range of uncertainty, and lead to total misunderstanding of the phenomena. Note that this issue involves the **propagation** of analytical **errors** in calculating model parameters, such as track etch rate.*

In Table 1A, the first 0.01-µm error is on the mean of r_0 (mean: 0.69 µm; standard deviation: 0.23 µm). A 1-2% relative error on a mean of 629 measurements is normal. The calculation can be checked in the supplement.

Furthermore, this has nothing whatsoever to do with the precision of a single microscope measurement.

On reflection, columns 6 to 10 of Tables 1A and 1B present no useful information; I therefore propose to delete them.

The comment is disingenuous; I am not digressing on textbook error propagation for equations as trivial as (1)-(6).

*(2) The **assumption** should be more explicitly documented for calculating the etching rate (and other parameters) from the observed geometrical information (i.e., length and width of a part of confined FTs). The documentation needs to be given in the relevant part of the text, not only giving a series of equations. Otherwise, it may be difficult for readers to follow the logic of the study.*

I am confident that the logic of our manuscript is evident to someone who has read it. The equations are explained in Aslanian et al. (2021; eq. 2-6), and numerical examples are discussed in detail in Jonckheere et al. (2022).

*For example, the author gives the variation of effective etch time versus angle to **c**-axis (Fig. 6), calculated from **track etch rate** values (Fig.7; **constant etch velocity** is assumed without explicit documentation). Then later in the text, he discusses the validity of assuming constant etch velocity throughout an **entire track length** (Fig. 8).*

The effective etch time of a track is calculated from its width and the apatite etch rate, **not** the track etch rate v_T (eq. 1a).

The assumption of a constant track etch rate underlies all practical calculations of cone angles and etching efficiencies since Fleischer and Price (1963). E.g., Fleischer et al. (1975), Tagami and O'Sullivan (2005), Hurford (2019). The discussion in our manuscript, prompted by Tamer and Ketcham (2020) and Ketcham and Tamer (2021), shows that it is right. Had it not been so, our work would have come to a different conclusion.

We nowhere assume a constant etch rate over the **entire** track length, but only over the straight sections we measured.

*Such a framework of the paper is just confusing and I suggest **reorganizing** the text in a more appropriate logical flow. Concerned with this, the author should better document/discriminate between physical theory, experimental observation, model calculation, and interpretation. These appear to be confused/contaminated from each other in places in the current text. This makes it difficult to **understand** the significance of new findings in the study.*

I find that reading a manuscript helps to understand it. I am not reorganizing it based on such a vague comment.

(3) We see many typos of the experimental parameters in the text and figures that are similar to each other. This makes it further difficult to read the paper correctly.

Like all conscientious authors, I will check the manuscript for typos and consistent use of definitions and symbols.

*Because of these issues, I judge that it is not appropriate to accept the paper at its present form of data presentation and interpretation. Therefore, I regret to suggest **rejecting** the paper, with a strong encouragement for the author to resubmit the material as a more carefully **reorganized** and **rewritten** manuscript.*

It is obvious that this is not an objective review, but an attempt to inflict the greatest possible damage. On both occasions when the reviewer refers to actual data, he shows a complete lack of understanding of the subject.

Apart from that, not one comment refers to the content of the manuscript except for what can be gleaned from the abstract and a glance at Table 1. The reviewer must realize that this is not acceptable reviewing practice.

The random hand-waving about error propagation and structural reorganization are tried and tested tools in the arsenal of unscrupulous "anonymous" reviewers intent of causing maximum disruption with least effort.

Sincerely,

Raymond Jonckheere
Freiberg, august 1st 2023.