Geochron 2023-13

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

CC2: *comments. by Richard Ketcham and Murat Tamer, August 9, 2023*

AC4: replies by Raymond Jonckheere, August 11, 2023 and Final response

Background

Some time ago Murat Tamer told me that he had no Microscope. I sent him the images I had made for this investigation. I explained how we measure and calculate. He did this fast and well but no substantial interpretation or manuscript was forthcoming. I took over and had a manuscript towards the spring. We had an agreement that Murat Tamer would have two go's at submitting it as corresponding author, after which I would. In April I declined a request from Chemical Geology to review the manuscript "*How many vs. which* ...", by Tamer and Ketcham, despite serious misgivings about the manner in which our work was presented.

I posted the current manuscript on ResearchGate to make clear that I would not allow it to be suppressed, which I had reason to suspect. This elicited immediate criticism from Richard Ketcham. I offered to submit it to Geochronology, which had not been my initial choice, in order to allow him to comment, since he could not review it, because of his collaboration with Murat Tamer and some results that contradicted his KT21 model. I also advised Murat Tamer, who was still on board at that time, that Richard Ketcham should not be reckless in his comments. First of June, I submitted the current manuscript, which was posted on the 16th. I had made some changes to the calculation in favour of the KT21 model in order not to make it look too bad. Nevertheless, Murat Tamer still asked the associate editor not to be named as co-author. There was never, before or since the slightest scientific argument that might have prompted this decision.

Richard Ketcham posted a first critical comment on the 13th. I thereupon contacted associate editor Shigeru Sueoka to ask if he could withdraw this comment, or if I could be excused from replying. I did that because I knew that it would leave me no alternative than to demolish the KT21 model, which would cause embarrassment and friction. I was told that I had to respond "in order to stimulate discussion", which I did on the 26th.

I was informed on July 29th that the discussion was extended to August 9th because "additional" referee comments were needed. On August 1st an anonymous reviewer and Raymond Donelick posted their reviews, to which I replied on the 3rd. On the 9th, less than five hours before the deadline, Richard Ketcham and Murat Tamer posted a joint second comment in response to my original replies, to which I now have to answer. On the 11th these comments have been removed and I am asked to "finalize" before the 18th. What do I do? Is it unreasonable to ask if there has been private communication or agreement between Murat Tamer, Richard Ketcham and associate editor Shigeru Sueoka that influences the handling of this manuscript?

The extended deadline has expired; there is an insubstantial review and a favourable review by Raymond Donelick, but no "additional" review. Instead Richard Ketcham and Murat Tamer have appointed themselves reviewers instead of commentators, offering to allow my manuscript to be published against certain concessions, and the recommendation that one of them should be co-author. The associate editor is someone with whom Murat Tamer has an ongoing collaboration. Although I have no part in that, I assisted the irradiation of their samples, for which I decided to also bear the expense after Murat Tamer had withdrawn as co-author.

General replies

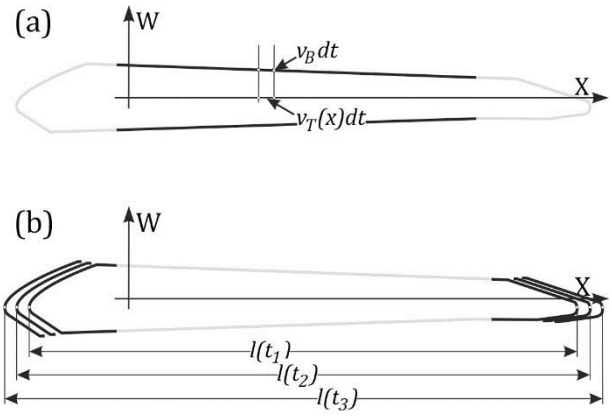
For discussion, it is perhaps useful to again explain the disagreement with TK20 and KT21. (1) The manner of our v_T-measurements assumes that v_T *is* **constant** along most of the length of a fission track. If it isn't, and v_T varies along a track, as the linear model requires, then our data are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

As far as I can tell, the latest comments are restatements of the earlier ones, further insinuations about preconceived notions, and deft use of words taken out of context, but there is nothing substantial related to any facts.

This goes to show that my essential moderate criticism, not so much of the KT21 model, as of the manner that it has been used to undermine my manuscript, has not come across as I hoped. Permit me therefore to be somewhat blunt: in the words of Wolfgang Pauli, the KT20 and KT21 model is "*not even wrong*", it is absurd.

Suppose I want to **determine** the variation of v_T along a fission track (v_T profile). I can proceed as follows (Figure 1a): digitize the track contour, which gives me a function *w(x)*, calculating the derivative |*dw/dx*| then gives me:

$$\left|\frac{dx}{dw}\right| = \left[\frac{1}{v_B}\right] v_T(x)$$



The result is an actual, continuous v_T-profile along a section of the track. Doing that for a larger number of tracks permits to investigate not just the v_T- variation along a track, but also from track to track and the correlation with their lengths, orientations, etc. That is how to do it, and how we learn that the tracks are straight.

One can also make a model, based on the assumption that all tracks have the same length and the same v_T-profile, regardless of their formation, orientation or individual annealing histories (fossil tracks); we add an isotropic v_B, and an unheard-off operator bias and roundedness selection criterion. Then we forget the entire midsection of the tracks, including the notion of width, and measure their lengths instead (Figure 1b). This construct is fitted to a handful of step-etch data; if I may be blunt, in my opinion, half are bulk-etching and the other half invisible tracks. Nevertheless it also gives a solution within the pre-defined constraints. Thus understood that is alright, and why I recommended publication of the KT21 manuscript.

But it is beyond a shadow of a doubt that such a model carries **zero weight** when it comes to real v_T-data. And I draw the line when it excuses questioning the competence and integrity of those who disagree with it. Endless tiresome tirades about unclear images, microscope resolutions, repeat measurements, assertions, assumptions, preconceptions ... indicate commentators scared out of their wits. It is undignified and presumptuous to think that one can browbeat lesser colleagues into concessions by threatening them with such a little stick.

Wherever the model contradicts even a single measurement of a single track, the model loses. **With respect to the corrections**, this entitles me to dismiss all model implications as **irrelevant**. Despite their bitterness, the commentators might consider my motivation to not have included the above comments in the manuscript.

Specific replies

p. 1: "*This accommodation is not sufficient to match the KT21 model ...*". The equations and calculations are correct, except for one adjustment, i.e., the slope *c* should be (1.7-0.022)/(17/2) = 0.197, instead of 1.7/(17/2) = 0.200.

p. 2: "*... the data be made available.*" The data are available to Murat Tamer; I will take advice from the journal about it.

Murat Tamer is welcome to the fossil track images. I leave it to his discretion to acknowledge their origin, and if relevant, that of the concept of the experiment. I suggest to integrate them with the

comprehensive fossil track data that we sent him before, and to publish the results together with Leila Sarkosh, who did the measurements. On the other hand, I do not wish to be involved in the publication.

p. 3 : There is a long section related to the independent 10 s (15 s) length data contradicting those in TK20 and KT21.

"*... finding tracks at the edge of visibility ...*": how can measurements subject to extreme selection bias form a valid basis for a v_T-profile, which is a fixed physical property of the tracks (given mineral and etchant)?

"*the JB03 data ... are for apatite from the Fish Canyon Tuff ...*": I thought this was about a v_T-model, but (apart from the etchant) it look as if it is a "*Durango-Tamer-2020*" v_T-model; what would the 2013 model be like?

p. 4 "*Figure 2 in RJ's reply is a modified version of Figure 9 in TK20*." I changed colour to greyscale to do the commentators a favour; the human eye is much more sensitive to shades of grey than to colour, in particular red.

"*... we don't believe ...*", "*... we also don't believe ...*"; those are not arguments about facts that require an answer.

"*... presuming ill intent ("political and calculating").*" I will forego several karma points here and stick to the rule: *tell the truth or at least don't lie* (Jordan Peterson). I supervised Murat's Bachelor and Master theses and spent countless hours talking to him since. I did my utmost to dissuade him from retiring as co-author. I can be mistaken, but his motivation is obvious to me, in one short sentence: "Richard Ketcham is a much more promising vehicle for future publications than Raymond Jonckheere". He is right, it is true, as this manuscript proves again. And it is a valid reason; I am therefore not suggesting "*ill intent*". It was a sensible and acceptable career choice. But there can be no suggestion at all of a scientific motivation. That said, since the danger appears less than he feared, he is welcome to come on board again, assuming he can make his mind up if he wants to sink this ship or sail in it.

p. 5 "*The author is confusing his feuds ... all scientific reasoning is always improved by considering applicable, independent* work". Yeah ... this comes from scientists who twice published a sensational v_T-increase due to annealing based on five measurements, without verification or reference to published and other accessible data, and a model that has never been – and will never be – tested against any data at all.

p. 6 "*... it seems that Equation 3b should use an arctan, not an arcsin ...*". The arcsin is correct; Murat Tamer can explain.

Conclusions

Reviewer #1 (anonymous): no corrections.

Reviewer #2 (Raymond Donelick): all suggested corrections accepted.

Comments #1 and #2 (Richard Ketcham and Murat Tamer): I can find nothing in these comments that indicates an error in the manuscript; all the data are valid and the equations and calculations are correct, except for a minor (<2%) adjustment of one of the constants used. I will of course make this correction.

As to the co-authorship of Murat Tamer, I would be pleased to have him on board again. However, I make no concessions in return regarding the content of this manuscript. I urge him to accept, as I consider the present criticism a mere token opposition to safe face. It will serve him better to be seen to abandon the TK20-KT21 models, as he started to do in TK23, and fall in line with the work of our group.

R. Jonckheere
Freiberg, August 11