MS No.: egusphere-2026-1925 - A different take on fission-track annealing in apatite - Author response to reviewer #1

General Comments	Response
This manuscript proposes a novel multi-stage empirical model for fission-track (FT) annealing in apatite. By distinguishing between initial gradual shortening, subsequent accelerated shortening (segmentation), and ambient-temperature annealing, the authors aim to build a robust approximation rather than absolute mathematical precision. The approach of explicitly separating the gradual and accelerated stages of annealing is a strong point, effectively building on earlier mechanistic insights that track shortening and track segmentation are kinetically dissimilar processes. The manuscript helpfully addresses the common failure of high-temperature models to accurately predict track shortening at geological time scales by integrating short-term, low-temperature data.	We are indebted to the reviewer for the overall positive assessment of our manuscript.
However, the resulting model introduces a quasi-fanning Arrhenius equation with three fitting parameters (plus L_0 and a unit-correcting scale parameter), relying on empirical thresholds rather than a unified kinetic theory. For model validation, the authors compare calculated FT lengths and ages with KTB borehole profiles. To fully assess the model's utility for thermal history inversion, the validation needs to be expanded. Currently, it is unclear how the model performs under variable temperatures. Calculating closure temperatures to allow for a direct comparison with existing models, as well as presenting calculated track length distributions, would significantly strengthen the manuscript.	Our model is indeed as empirical as most others. It is too difficult to construct a genuine kinetic model, involving physical constants and the kinetics of specific atomic-scale processes, because it must also deal with the connectedness of the defects and the effects of etching. The scale parameter is not dependent on the fitted data, but takes a value of 1 in appropriate units (e.g., µm). For calculating *S*, we used the L_0-values of the unannealed samples. However, Carlson et al. (1999) showed that L_0-differences between samples are due to their etch rates (*Dpar*). Thus, for etching protocols tailored to the apatite composition, as in the case of Ravenhurst et al. (2003), L_0 would be a constant, independent of the sample. Even for fixed protocols, L_0 is given by the value of *Dpar* (Carlson et al., 1999; eq. 1). We agree that validation is important. However, despite the inconclusive outcome, we believe that the KTB results are significant. The samples have different thermal histories, consisting of a complete or partial transit of the apatite partial annealing zone, followed by isothermal holding at temperatures spanning the entire annealing range (Figure 7). One could even argue that the fact that their thermal histories are related adds to their strength. At this stage, our aim is to predict the mean track lengths. There are to our knowledge no published length distributions for the KTB samples. Detailed measurements are in progress. We aim to contrast our approach, rather than its predictions, with that of others models. Following the reviewer's advice, we will include a set of geological closure temperatures for comparison.

Specific Points

1. Using absolute shortening (S) instead of the normalized length ratio (r) implicitly assumes that all apatites shorten at the same absolute rate regardless of their initial length (L_0). This appears to contradict existing experimental findings where unannealed length correlates more strongly with etching rates (Dpar) than with latent track properties. The authors should justify this choice.	That the mean track lengths in all apatites anneal at the same rate throughout the initial gradual stage, up to the onset of accelerated shortening, is an explicit assumption of our model. $S = L_0 - L$ is the constant distance between the parallel lines in Supplement Figure S1. The Ravenhurst et al. (2003) data illustrate this: their samples were etched for different times to the same "degree" (same *Dper*). The consequence is that L is independent of composition, and remains so on heating up to the point of accelerated annealing. We believe that this does not contradict the linear relationship between L_0 and *Dpar* of Carlson et al. 1999), but reinforces their conclusion that this relationship is due to etching alone.
2. The text relies too heavily on frequent references to tables and figures in the supplementary material, which hinders readability. I recommend moving the most critical data and figures into the main text, while keeping within the journal's length limits	We understand the reviewer's concern. While Supplement Figures S1 (13 pages) and S3 (7 pages) are too big, we will transfer Supplement Figures S2 and S4 to the main text. Table T1 is large but of limited interest to the end result, while Tables T3 and T4 are more for reference than of real consequence to our work. Still, we will include Table T2 in the text.
3. The presentation in Figure 1 could be improved. It would be highly beneficial to include error bars so readers can fully appreciate the degree of agreement between the experimental data and the model curves. Additionally, please clarify the origin of the parallel lines; are they directly fitted to the data?	The 1σ standard errors (~0.1-0.2 μm) are smaller than the symbols; we will add typical error bars. The fit to the unannealed mean lengths was obtained by least-squares regression. The lines for the annealed samples are parallel and to pass through the centre of the samples that are still in the stage of gradual annealing, and excluding those showing accelerated shortening.
4. Lines 103–104: The Ravenhurst et al. (2003) dataset is fundamentally a study on chemical etching, as samples were etched under varying conditions depending on their chemical compositions. Combining this dataset with those of Carlson et al. and Barbarand et al. may introduce inconsistencies and could explain the discrepancies observed in Figure 4. The authors should address this potential compounding factor.	Their constant-*Dper* approach gives initial mean lengths that are independent of composition, supporting the conclusion of Carlson et al. (1999) that initial-length differences are due to the different etch rates of different apatites, and not to latent-track properties. If initial-length differences between apatites are strictly due to etching, it is reasonable to assume that that also applies to the mean lengths of partially annealed samples. A consequence of this approach is that, strictly speaking, $r = l/l_0$ is not the ideal measure for comparing the annealing rates of different apatites. In contrast, $S = L_0 - L$ eliminates the etching-related differences between apatites as well as between protocols, as long as both the annealed and unannealed tracks are etched the same, allowing us to combine datasets. The median residuals are +0.11 μm for the Ravenhurst et al. (2003) data, -0.04 μm for the Barbarand et al. (2003) data and -0.06 μm for the Carlson et al. (1999) data, i.e., a maximum offset of~0.17 μm. This is small compared, e.g., to the >1 μm initial-length difference for unannealed tracks in Durango apatite. Thus subtraction ($S = L_0 - L$) removes most of the systematic differences related to etching. The question is then if the remaining offsets are due to the etching conditions, or not. We cannot answer that;

	there is an abundance of factors, from experimental conditions related to etching, sampling and measurement right down to the temperature control of the annealing experiments that could be responsible. We will include the above information in the manuscript.
5. Lines 115–118: The rationale behind the "zero-length" logic requires further clarification. While it is clear that S approaches A_1 as T_0 approaches zero Kelvin, it is less clear why L should be forced to be greater than L_0 if the data fit yields an A_1 value less than zero. This might suggest that the empirical formula does not fully capture the physical reality of the process.	As the reviewer mentions: as $T \rightarrow 0$, $S \rightarrow A_1$ (equation 1), and, since $S = L_0 - L$, it follows that $A_1 = L_0 - L_{T=0}$ and $L_{T=0} = L_0 - A_1$, or $L_0 + \lvert A_1 \rvert$. That $L_{T=0} > L_0$ and $A_1 < 0$ ($\lvert A_1 \rvert > 0$), is a consequence of the fact that the measurements of S are relative to the measured unannealed induced-track length (zero-length L_0) and positive S indicate shortening. It is clear that the hypothetical $L_{T=0}$ must be greater than a real, measured length, and hence corresponds to negative S, i.e., negative A_1. We will add a short description to the manuscript.
6. Lines 118–119: Please explicitly clarify if your definition of C_1 corresponds directly to that of a fanning point temperature.	As far as we can tell, there is no direct relationship between C_1 and the point of instantaneous annealing. The comment refers to the fact that, as $T \rightarrow \infty$, $1000/T \rightarrow 0$, leaving only C_1 in the denominator of equation (1). We will delete this statement, as it is indeed confusing.
7. Lines 128–130: Dimensional units are critical here. While the authors establish a connection between B_1 and C_1, a third parameter is not truly eliminated; rather, it appears to be made arbitrary. For in-stance, C_2 is effectively set to 1 micrometer. If a different unit of length had been chosen, C_2 would simply change to reflect that unit. Please clarify this parameterization.	To an extent. The occurrence of B_2 in both the numerator and denominator implies that its unit is reciprocal temperature (K^{-1}), and the fraction is unitless. As S (and A_2) are in µm, we multiplied the fraction in equation (2) with a constant (×1 µm) for purely formal reasons. We will explain that it, as well as A_2, is in the same units as S, i.e., the units of the fitted data. This is a consequence of measuring absolute length reduction $S = L_0 - L$, rather than relative shortening $r = L/L_0$. We explain above why this is best suited to our approach.
8. Equations 3 and 4 and Lines 131–141: This formulation resembles a modified fanning model. It appears that a moving fanning point will cause a significant departure from standard models when applied to long-time, low-temperature data (i.e., geological timescales). Expanding the discussion on the implications of this departure would be valuable.	Following earlier descriptions, we assumed that S varies with $(1/T)$ and $\ln(t)$, and followed the data to our equation (Figure 2). Although we imposed no specific model, the result indeed proved to be a three-parameter fanning linear model with a fanning point on the vertical axis ($T \rightarrow \infty$). This is fortunate, as a case can be made that it has to be so. Otherwise, beyond the crossover, track length would increase with annealing time. The fanning point lies at -34.34 on the $\ln(t)$ axis, corresponding to $\sim 1.2\ 10^{-15}$ s, a frequency of $\sim 10^{15}\ s^{-1}$, or ~3.3 eV. This is close to the results for a substantial subset of single-sample fits carried out by Ketcham et al. (2007; Figure 2a) These frequencies are however closer to values for electronic rather than the atomic processes involved in annealing. These values do not lend physical meaning to our model, but perhaps suggest that it is reasonable. We think that this comment refers to Figure 1 of Ketcham et al. (2007), comparing fanning curvilinear models fitted to the Durango data of Carlson et al. (1999) and Barbarand et al. (2003), which diverge in the area of geological annealing. While we do not know much of the calculations, we assume that when identical models are fitted to two similar, but not identical, lab datasets, they will agree in the data region but can diverge at long and short timescales where they are unconstrained, without causal connection

	between them. We will report fanning-point coordinates for our model, estimates of the partial annealing zones for different holding times, and of the closure temperatures for conventional cooling rates. We stress, however, that our aim is not to enter another competitor in the model competition, but to show that a reasonable model can be built on a different perspective on the data.
9. Figure 9 and age calculations: The agreement between the calculated FT ages and the KTB profile ages seems to be a direct consequence of the arbitrary choice of the bias model. Furthermore, the methodology behind the FT age calculation needs transparency. It appears a simple length correction was applied rather than calculating the cumulative reduction across multiple track populations generated at different times, as is standard in thermal history inversion software. Please clarify this procedure.	Yes and no. The bias model plots the normalized tracks densities (r_ρ) in apatite prism faces against the normalized mean lengths (r_L) of track populations at various annealing stages. One concern is that both measurements are subject to significant variation, in particular in samples in advanced stages of accelerated annealing. Another is that the data are old and scarce (Green, 1988) and that different equations have been fitted over the decades. The data moreover suggest that different apatites could define different trends (Figure S2). In that case, models fitted to mixed data may be artefacts (Green, 1988). There is thus significant risk involved in picking a specific model of the shelf. We considered that we could with equal justification construct a first-order model suggested by the data in this work. Following earlier work, we assume that the model consists of two linear sections, a section with unit slope through (r_ρ, r_L) = (1,1), and a flatter section through (r_ρ, r_L) = (0, ½), to where several models appear to converge (Wauschkuhn et al., 2015). Both lines intersect at a break in slope. Its coordinates were taken from Figure 5, i.e., corresponding to the onset of accelerated annealing in Fluorapatite ($S \approx 2.2$ µm or $r \approx 0.86$). The KTB data thus do not influence our selection of a bias model. We aimed to avoid an arbitrary choice and to adopt instead a first-order model consistent with the remainder of the data. We will include an explanation in the manuscript.
10. Lines 351–353: At the laboratory timescale, the proposed model behaves essentially like a standard fanning model. The text should explicitly highlight what unique insights or advantages this model provides at this scale compared to existing frameworks.	Our aim, at this stage, is to demonstrate that a valid model can be constructed based on a different take on the data and a somewhat different concept of the annealing process. This includes redefining the dependent variable as $S = L_0 - L$, which eliminates some consequences of compositional differences between apatites, and some differences between etch protocols. It allows constructing a general three-parameter annealing equation, without a transformation of the dependent variable. The differences between apatite varieties first come in at the stage of accelerated annealing (break-up?), and could be correlated with the latent-track diameters. While this is not proven, it offers definite possibilities for investigation. One can take this further, and consider if the anisotropic annealing rate within a single apatite is related to an anisotropic latent-track diameter, as some initial results suggest. To account for that, one would have to think of annealing in terms of a percolation process rather that in pure kinetic terms. That could establish a real link with the latent-track studies of Paul and Fitzgerald (1992). We furthermore

	take the view that the bias function is not established once and for all for all scientists and protocols. We consider that our work shows that an alternative approach to fission-track annealing, without a predetermined mathematical form, naturally results in a simple fanning linear model with a fanning point at $1/T_F$ =0 and $\ln(t_F)$ = -34, close values for actual electronic and atomic processes (compared to the fanning points of curvilinear models). At the opposite end, $\ln(t_G)$ = +35, in conjunction with the principle of equivalent time, our model makes reasonable predictions of fission-track annealing in geological samples. Maybe not the most accurate, but reasonable. We also ask how models should be evaluated, as there are too few, too limited, and too uncertain benchmarks, and no formal consensus. We will include an explanation in our manuscript.

Conclusion

Recommendation: Revisions Required (Major Revisions)

In summary, this manuscript possesses high scientific significance for the geochronology community, offering a thought-provoking and valuable discussion on fission-track annealing mechanisms. The effort to bridge laboratory data with geological timescales is highly relevant. However, the exact calculation methods require greater clarification, the validation process needs to be expanded (specifically regarding closure temperatures and length distributions), and the overall presentation needs some polish to improve readability. I recommend publication after the authors have addressed these substantial concerns.	We are grateful to the reviewer for supporting our work and will add explanations based on the reviewer's comments and the above responses. At this stage, we wish to emphasize our approach rather than the exact predictions of our model. Although we do have modelled length distributions of the KTB samples as a by-product of our calculations, we have no experimental data to compare them with. Detailed length measurements are ongoing (Trilsch et al., 2025) and we propose to report the results when they are completed.

Sincerely,
R. Jonckheere
F. Trilsch
B. Härtel
T. Nagel