

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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The comments by different Reviewers of this ms show a high degree of consistency in highlighting a large number of issues, particularly emphasising the poor standard of presentation associated with this manuscript in terms of the lack of explanation in both the text and Figure captions. It should not be necessary for a reviewer to read a number of other papers in order to review a new paper, but in this case it is not possible to follow the work from the ms alone. I spent way too much time in reviewing this ms and I'm sure the other reviewers did too. I particularly congratulate Ed Sobel on his thorough and thoughtful comments. My overall response to the ms is to ask what

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practical consequence this work could have. I understand that any progress in understanding the fundamentals of track revelation is useful in principle, but at the end of the day, in practical application we have a grain mount containing various apatite species with different etch rates, all etched together, for one etch time and with one etchant. If analytical data in unknowns and age standards and calibration data for kinetic models are generated using the same conditions, then reliable results should be obtained. This raises a huge number of practical questions, but they are not directly relevant to the present discussion. One issue that still puzzles me is the repeated assertion, in the paper under review and in earlier papers in the chain, that VB is not anisotropic. Surely the etch figures in a prismatic surface show that the etch rate is higher along the c-axis than perpendicular to it. Can anyone explain this conundrum? I see little point in responding to most of the authors' comments on my review. The consistent opinion of all reviewers is that this is an extremely poorly presented manuscript, and requires significant revision before it could be considered acceptable for publication. Perhaps if the authors take all the comments into account the final product may eventually be easier to understand and appreciate. However I would like to respond to one comment: In their response to my Review, Ketcham and Tamer suggest that I was misdirected in my criticism of the Wauschkuhn et al. (2015) study. I was not. In my review I discussed the basic motivation of the Wauschkuhn et al. study, in terms of their ideas concerning the KTB borehole (as expressed in the title of their paper). I was not discussing the particular aspect that Ketcham and Tamer refer to in their comments, as illustrated in Figure 15 of Wauschkuhn et al., casting doubt on the principle of equivalent time. As I was reviewing the ms by Ketcham and Tamer I did not see the need to provide additional comments on any other aspect of the Wauschkuhn et al. study, apart from their basic premise. In regard to the evidence in Figure 15 of Wauschkuhn et al. (2015), the data presented there appear to fully support the validity of equivalent time. My reading of that Figure is that the induced tracks that were pre-annealed do not begin to start shortening again until heated at a temperature above that used in the initial treatment. At higher temperatures, both induced and pre-annealed induced

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populations give similar track lengths, which is just what is predicted from equivalent time. Regarding the comparison with spontaneous tracks, Wauschkuhn et al. (2015) acknowledge that they cannot be compared directly with induced tracks because “the fossil track population is not a single-length population, and contains somewhat shorter and somewhat longer tracks than a population of induced tracks pre-annealed to the same mean length”. Duddy et al. (1988) provided clear experimental evidence confirming the validity of equivalent time, based on measurements of mean track length in apatites that had undergone various variable temperature annealing treatments. In common with Wauschkuhn et al. (2015), Ketcham and Tamer in the paper under review fail to mention this and proceed to cast doubt on the concept. In their response to my review, Ketcham and Tamer state “The experiment in Wauschkuhn was designed very specifically to test the equivalent time hypothesis in a way Green (1988) did not.” This comment is either deliberately misleading or betrays a basic lack of understanding. The Green (1988) study was not designed to test equivalent time. The Duddy et al. (1988) study was designed to test equivalent time and the concept passed with flying colours. It is unacceptable to cast doubt on the validity of equivalent time without citing published evidence that clearly validates the concept. The fact that they discussed Duddy et al. (1988) in a previous paper, as noted by Ketcham and Tamer in their Response, is irrelevant to the present discussion, and only highlights the lack of full documentation in the ms under review.

Paul Green. January 2021.

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