Reviewer: Rebecca Flowers

Our responses are to the reviewer's comments are shown in italics.

Review of Fox et al., Origin of Great Unconformity Obscured by Thermochronometric Uncertainty, Geochronology

We thank Prof. Flowers for this detailed and helpful review. We have modified the text where suggested and this has improved the framing of the problem and also the ongoing debate. We have attempted to satisfy all reviewers with our modifications. These are listed in the general response we have provided. Due to the number of changes requested by the reviewers, we have not tracked changes.

The core of the discussion focuses on whether the thermochronometry alone can be used to constrain cooling histories through time or whether constraints are required. It is our belief that it is very important to establish what the data can and can not resolve. It is useful to show what a solution might look like without any constraints as in many cases, the constraints are controversial or complicated. Through linking samples in space using concepts like the AER, or grouping samples together, more thermochronometric data is used to infer a solution. This should perhaps be attempted before incorporating additional constraints. It is also important to highlight that incorporating data from different thermochronometric systems will also provide greater resolution.

Our goal was to simply explore the importance of uncertainty in damage parameters. To isolate this, we attempted to infer a thermal history, only using the data. Our model was too simple, however, the best fitting model does approach an acceptable GU thermal history. To ensure that we are able to produce this thermal history, we have decreased the uncertainty on the data. In this way, we have produced a thermal history that is consistent with the GU but have also been able to explore the importance of the uncertainty.

This contribution is focused on better constraining the uncertainty associated with the zircon radiation damage accumulation and annealing kinetic model (ZRDAAM), and then evaluating how this uncertainty influences the ability to resolve thermal histories associated with development of the Great Unconformity. The manuscript begins by reviewing the existing ZRDAAM calibration, presents a new calibration based on the same kinetic dataset as used in the original ZRDAAM, uses a new approach to constrain ZRDAAM uncertainties, carries out inversion modeling of a dataset to explore the influence of ZRDAAM kinetic uncertainties on data interpretations, and concludes with some implications regarding the resolution of (U-Th)/He datasets in deep time.

This is a useful and informative contribution. I agree that there's opportunity to use the current Great Unconformity debate as motivation to improve kinetic model calibrations, which in turn can improve our ability to address problems like the origin of this feature. This paper seems to approach the Great Unconformity problem from the direction of evaluating whether the thermochronologic data alone can resolve thermal histories in deep time. I completely agree that this absolutely is not possible without integration with other types of information, like that from geologic data.

I provide a variety of suggestions below that I think could be used to further strengthen the paper. Most importantly I highlight (described further below): 1) the need to more clearly and completely explain the new calibration, 2) that it's essential to include the Great Unconformity in any model that is supposed to explain the Great Unconformity, 3) that both the data being modeled and the predictions from the inversion models should be plotted, and 4) one aspect of the implications section that could be more valuable if expanded.
Abstract

Lines 13-14: "...about the origin of the Great Unconformity, a global erosional event that represents a period of almost a billion years at the end of the Precambrian."
Suggest modifying this sentence. The Great Unconformity is a geologic feature, not an event. This sentence also asserts that it represents a global erosional event that represents a billion years – but the debate is actually about the timing and duration and whether it represents a global erosional event or not.
Introduction

Corrected.

Line 33: Keller et al. (2019) isn't appropriate to cite in this sentence because there are no thermochronologic data presented or interpreted in that contribution. Also suggest adding an e.g., before the refs, or adding more refs, because there are more papers that have used thermochronologic datasets to make interpretations about the Great Unconformity than those cited here.

We have modified the introduction and this has been accounted for.

Lines 35-36. "...in the Grand Canyon, the erosional event spans from circa 1200 to 250 million years...". This is the amount of missing time. The erosional event didn't span this entire interval. Also should use an e.g., for the reference, because many have noted this missing magnitude of time in the Grand Canyon.

Corrected.

Lines 38-40. "This approach highlighted that erosion rates increased across the North American craton during Neoproterozoic glaciation, supporting the hypothesis that...". It would be more appropriate to replace the word "highlighted" with "inferred". None of the datasets used in that paper require that erosion increased during the Snowball glaciation, as shown in the comment by Flowers et al. (2022). This is true even when one does not take into account kinetic model uncertainties, which I agree further increase the uncertainty in the inferred tT paths, and are the focus of this contribution.

Corrected.

Line 46: "...concepts of geochronology". Also involves concepts of diffusion. Suggest modifying to "...geochronology and diffusion."

Corrected.

Line 49: missing a word here, should be “diffusive loss”.

Corrected.

Lines 43-51: Suggest emphasizing in this paragraph that the dates represent a time-integrated thermal history. This paragraph focuses only on simple exhumation scenarios, which differ from the deep-time studies discussed in this paper, that include multiple burial and erosion events.

Corrected.

Line 57: suggest rewording to “the transition from open-system to closed-system” given the phrasing of the previous sentences.

We have kept this sentence because it is quite general.

Line 59: Appropriate to cite Ketcham et al. (2013) here, along with Guenthner et al., 2013.

Cited.

Line 67. “Both those in favour of a glacial (McDannell et al., 2022) and tectonic (Flowers et al., 2020) origin...”

To be more accurate, suggest modifying to “Both those in favor of a globally synchronous glacial origin (McDannell et al., 2022) and regionally diachronous tectonic origins (Flowers et al., 2020)...” The text has framed the problem here and elsewhere as a debate over a single origin for this feature – but a critical aspect of the debate is whether or not there is indeed a singular origin or if there are multiple origins.

Corrected.

Lines 69-77. This is a great description, and also nicely highlights that annealing at higher temperatures after damage accumulation at low temperatures affects the retentivity and the date. It’s this element that could be useful to bring into the geological framing in lines 43-51.

Thank you! We want to keep the general structure and keep the intro to thermochron as concise as possible. With all the shifting of the intro, we have hopefully achieved this and feel like it is acceptable to focus on the problem here.

Line 79. Suggest changing RDAAM to ZRDAAM, as ZRDAAM was defined in previous paragraph. RDAAM refers to a widely used apatite radiation damage accumulation and annealing model, which isn’t discussed in this paper.

Corrected.

Lines 81-85. “Parts of this history were reported to within less than 10 degrees between 700 and 250 Ma and then again from 15-7 Ma. It is unclear whether the data really provide such tight constraints on temperatures in the past...”
- It’s clear that the data don’t provide such tight constraints on the temperatures in the past, because the model of their Figure 4 says that when the Great Unconformity developed that the basement was at 200 +/- 10 °C (so, 7-10 km deep in the crust, depending on assumptions), when we know unequivocally that the rocks were at the surface during deposition of the Cambrian sandstone on the basement to define the Great Unconformity. This model isn’t set up to require that the Great Unconformity be part of the model. See Peak et al., 2022. This doesn’t seem like a good example to use here.

We have kept this example here to highlight that sometimes thermal histories appear to be too well resovled. Because our focus is on the importance of damage model uncertainties, this is a really nice example to highlight as our damage model would increase the uncertainties.

Lines 87-88. “...known amounts of radiation damage.” Suggest being more cautious here. Instead of saying “known amounts of damage”, could say “...well-constrained amounts of radiation damage.” For the Sri Lankan zircon, which I agree was a good sample to use in the Guenthner et al. study, the assumption of rapid cooling at 440-420 Ma to estimate radiation damage seems reasonable, but this is a long timescale with some uncertainty in the history. And even if one acquires Raman data, there is currently ambiguity in how to translate that data into the type(s) of damage in the crystal that matter for He diffusion.

Lines 94-95. “We show that natural variability in radiation damage annealing parameters causes ZHe ages to be dispersed even for crystals...” Delete “annealing”? As I understand it, this paper doesn’t address variability and uncertainty in radiation damage annealing kinetics?

Corrected.

The existing calibration of the radiation damage and annealing model
Line 123. “...at a specific cooling rate.” And assumed grain size.

Corrected.

Line 139. “..between the model parameters.” Helpful to specify here that you mean zDo and zEa.

Corrected.

Lines 143-144. “However the accuracy of this model has only been assessed by looking at general trends in model predictions.” Do you mean in Guenthner et al.? Or in the variety of studies that have aimed to explain data using this kinetic model? Provide some references here?

We have only cited the Guenthner paper here as these data are the best to constrain the parameters and this is the only time the model has been compared to the data in a quantitative way.

Lines 125-146. Clear explanation of the Guenthner model.

Thank you!

It would be worth explicitly noting somewhere that this contribution does not address uncertainties in the annealing model.

We have highlighted this point at the end of the paragraph. Lines 144.

A new calibration of the zircon radiation damage and annealing model
This section is the crux of the paper and I think that more explanation is needed. I believe that I understand the Guenthner calibration, but I don't understand how the end member values for this new calibration (and thus their associated uncertainties) were obtained here based on the explanation provided. If what is presented here is indeed an improved approach as the authors are clearly arguing, then this is exciting and it should be explained with enough clarity so that those who generate the datasets used for model calibrations can understand and apply this method. If the authors would prefer not to lengthen the main text, then the appendix would be a great place for an extended description.
Lines 194-196. From the Figure 1A and 1B plots, it's not obvious that the revised calibration predicts the observed data better than the original calibration. It would be helpful to add some text here that explains what features of this plot that the reader should focus on to see this.

We have refined this plot by adding colour that reflects total alpha dose. We have also stressed that both models do not fit all the variability in the data. Our aim to assess the uncertainties, not provide a new damage model.

Figure 1. If I understand correctly, the purpose of this figure is to show how well the Arrhenius data for each sample used in the ZRDDAM calibration are fit with the original calibration and the ZRDAAM calibration of this paper. Right now, it's difficult to make this comparison across the two plots. This probably could be done more successfully by including a separate Arrhenius plot for each sample, labelling each Arrhenius plot with the sample name, and on the same plot including the diffusion kinetic data, the prediction from the original ZRDAAM calibration, and the prediction from the ZRDAAM calibration for that sample.

This is not the goal of our analysis. It would also make too many figures and complicate the simple message we wish to highlight. This would be the core of a new paper that attempts to improve the fit the to the data with a new parameterization.

Lines 196-203 and Figure 2. Could you please explain more fully and clearly how these histograms were calculated. Again, this is the crux of the paper, since this calibration and the associated uncertainties are then applied to draw the main conclusions of this contribution.

We have improved the overall description of the model and the parameters.

Propagating model uncertainties

Lines 222-223. "Note, this implementation of the model has been used previously (Tripathy-Lang et al., 2015). It sounds like this model calibration was presented seven years ago in previous work? To what extent was it described there? This should be explained in the introduction.

We have clarified that this refers to the numerical methods.

Lines 224. "...an effective U concentration ([eU = [U] +0.24[Th]; Gastil et al., 1967))." I hadn't encountered this paper before, which interestingly uses eU to refer to "equivalent uranium" (although not effective uranium). That paper doesn't include an eU equation. Cooperdock et al. (2019) should be cited here for the equation, (eU = U + 0.238Th). Flowers et al., 2022 also lays out the equations for eU.

Added references here.

Line 241. "...and the data from McDannell et al. (2022)". Miltich (2005) should be cited for the data. McDannell et al. mined the Minnesota data from the Miltich (2005) undergraduate honors thesis without publishing them.

Added the references here.

Line 242. "...for the sample "Minnesota"... These are actually multiple samples, not a single sample.

Corrected and clarified.

Lines 241-242 and Figure 5. "For this reason, we use only the ZHe data and do not incorporate additional constraints."

- This simulation is supposed to explain the Great Unconformity, but the highest probability time-temperature paths aren't at surface conditions when the Great Unconformity developed. In the final comparative statement that compares the three panels, the caption states "The overall patterns are very similar, but the apparent resolution is different, resulting in different geological conclusions." To me, the geological conclusion here should be that all of these model results are geologically meaningless because the highest probability tT paths violate the Great Unconformity, so all three models should be discarded. Could the authors either include the Great Unconformity in the models or better articulate why the Great Unconformity isn't honored in a model that is supposed to explain the Great Unconformity? This is now a repeated characteristic of many published QTQt models that are supposed to reproduce the Great Unconformity.

We have redone the analysis and the GU is reproduced to some degree now. The discussion about whether to include constraints or not is at the core of thermal history modelling and requires a separate discussion, not a paragraph in a paper on damage models.

Figure 5. Please plot the data being modeled here and show how well the tT paths fit the observed data – for example, by making date-eU plots of observed vs. modeled data. See Gallagher (2016).

The data are well predicted by the model. These plots can be seen in the original publications.

Implications
Lines 264-266. "In turn, it may be challenging to resolve cooling histories sufficiently to attribute the Grat Unconformity to Cryogenic Glaciations (McDannell et al. 2022) or geodynamic process related to the break-up of Gondwana (Flowers et al., 2020)." Yes, agreed that it's not currently possible to resolve cooling histories at this level with the "thermochronologic data alone" even when not considering uncertainties in kinetic models. This is why it is essential to integrate other types of information into models, such as geologic data. Flowers et al. (2020) didn't argue that we could resolve the cooling histories sufficiently without other information, as implied in this sentence. Perhaps you could modify to remove that implication?

Corrected.

Lines 268-281, and final sentence in this paragraph: "For example, McDannell et al. (2022)'s results for Pikes Peak highlight how models that ignore overdispersion appear to resolve a 700 Ma cooling signature, which is smoothed out when the overdispersion is effectively reduced by adding excess uncertainty on some of the data."
- There are interesting and important points made in this paragraph about how uncertainties can be accounted for in QTQt and the associated influence on the inferred tT paths. The final sentence that makes a vague reference to a figure in McDannell et al. (2022). It would be great if the authors would add a figure in this paper that helps to illustrate the valuable points being made here rather than vaguely referring to a figure in that paper. Alternatively, this paragraph could be eliminated.

We have kept this to highlight the discussion. We tried drafting a new figure but were unable to make an effective figure.

- The Pikes Peak models in McDannell et al. (2022) also violate the Great Unconformity relationship, as noted elsewhere about other published models.
- If this model is discussed, the I feel that it also is important to cite our 2022 comment on this paper. In that comment we show that entirely different tT paths, not captured in the McDannell et al. models, can explain the data.
Lines 283-314. This is a nice illustration of the dispersion expected in real datasets, and provides some of the rationale for the binning into synthetic grains as done by many who simulate (U-Th)/He data.

Code Availability. It would be appropriate to put the codes used for the calculations in this paper in a supplement and available for download so that others can reproduce these results and apply the approach to other kinetic datasets. This is now done so easily that it no longer seems appropriate to require an email to the authors to obtain the code.

QTQt is not available online. We are also attempting to do this same analysis with different diffusion datasets.

I enjoyed reading this contribution and hope that the authors find these comments helpful to further strengthen the manuscript.

Becky Flowers, CU Boulder

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