We thank Chelsea Mackaman-Lofland and Kendra Murray, as well as Associate Editor Shigeru Sueoka, again for the additional comments on our manuscript. In this document, reviewer comments are replicated in black and our responses are in blue. Line numbers in blue refer to the revised version of the manuscript without tracked changes.

Associate Editor (Shigeru Sueoka):

This manuscript has been re-evaluated by two reviewers. Both reviewers highly appreciate the authors' responses to the previous review comments and acknowledge that the manuscript has been significantly improved. While a few additional minor revisions have been suggested regarding wording and parameter consistency, the manuscript is considered publishable provided that these points are appropriately addressed or responded to.

We thank the associate editor for the supportive comments and hope that all concerns have now been addressed.

Reviewer 1 (Chelsea Mackaman-Lofland)

Vasey and coauthors have made excellent revisions in response to reviewer and editorial suggestions, and the revised paper is considerably improved from their initial submission. I think the changes to the text that explain the difference between geodynamic and thermokinematic models; examine the impact of different approaches to approximating topographic evolution; clarify terminology related to surface uplift, rock uplift, erosion, and exhumation; and the authors updates to Figs. 5 and 6 are particularly beneficial. In addition, I am thrilled to learn of other efforts within the low-T thermochronology community to create Python resources in support of diffusion/annealing kinetics, and in this context, fully agree with the authors and other reviewer that the authors' choices of diffusion and annealing kinetics are appropriate for this contribution.

We thank the reviewer for the supportive comments.

Though the authors have provided a clear response to reviewer comments and text updates that help explain the intention behind the highly simplified model with static temperature structure (section 3.3; Figs. 4, 5; lines ~220–237 in the revised manuscript without track changes), I still think it worthwhile for the authors to update their simplest/benchmark scenario to incorporate a geologically realistic temperature structure, including advection of isotherms. I strongly encourage the authors to make these changes because showing how thermal structure evolves in response to a simple block uplift scenario would help illustrate how geodynamic models treat temperature evolution in the lithosphere, and may guide readers/potential GDTChron users in building understanding and intuition for how the magnitude and rate of rock uplift/exhumation rates, alongside other parameters, affect the thermal structure of the lithosphere, and ultimately thermochronology dates. A benchmark case that incorporates broadly realistic thermal parameters, rock uplift/exhumation rates, AND evolution of lithospheric thermal structure would also be most consistent (and allow comparison with) similar block uplift calculations in thermokinematic models (e.g., Braun, 2003; Braun et al., 2012; Lock & Willet, 2008).

In our revision, we have updated the simple exhumation model to have a conductive geothermal gradient through 20 km of upper crust with a density of 2.8 g/cm^3 and a thermal conductivity of 2.5 W/mK. The model domain has a surface temperature of 0°C and a basal temperature of 600°C, which corresponds to a basal heat flow of 85 mW/m^2 combined with radiogenic heat production of 1×10^{-6} W/m^3. This geotherm remains in steady state prior to exhumation, and isotherms are advected upwards during exhumation before returning to steady state after exhumation. We have added 100°C and 300°C isotherms to Figure 5 to illustrate this graphically. This change results in a slight younging of ages but no first-order change to the results or interpretation (Lines 200-233).

I also appreciate the authors' response to reviewer comments and text updates to explain their choice of model domain showing AHe ages in Fig. 6 (second row), but I'll reiterate that, in my opinion, showing forward modeled AHe ages for the same model dimensions as the geodynamic model is not particularly helpful or illustrative for readers. The X Distance (km) can stay the same, but please consider changing the vertical exaggeration and zooming in on the predicted cooling ages in the uppermost crust, where the AHe chronometer is most sensitive.

We have updated this panel in Figure 6 to have 2x vertical exaggeration, which minimizes distortion while showing the age patterns more clearly.

Specific comments

The authors have made very helpful changes to the figures to ensure consistency in their treatment of reference timing (e.g., changing references to time elapsed in Myr to time before present/model termination, in Ma). I encourage the authors to take another pass through the text to make sure they are using the same timing references in the text and figures when describing model evolution and results.

We have made several additional changes from Myr to Ma (or added Ma in parentheses) throughout the text to improve clarity/consistency. We also have specified what each of these abbreviations means (megaannums – millions of years ago vs. megayears – million years) at their first use to help with clarification (Lines 114-115).

Lines 79–81 "They [kinetic models used in GDTChron] are also sufficiently computationally efficient to evaluate the large number of thermal histories output by geodynamic models and are often appropriate for the relatively simple thermal histories captured within such models" : here, it would be more accurate to say that for relatively short-lived thermal histories involving rapid cooling events, the ages and track length parameters predicted by kinetic models currently implemented in GDTChron don't deviate significantly from age/track length parameters predicted by other, more updated kinetic models, then discuss the extent to which such thermal histories are relevant to geodynamic model interpretation.

We have revised this sentence along these lines and thank the reviewer for the suggested phrasing (Lines 78-82).

Pending these minor updates and corrections, I consider the manuscript ready for publication.

References not included in the manuscript:

Lock, J., & Willett, S. (2008). Low-temperature thermochronometric ages in fold-and-thrust belts. Tectonophysics, 456(3-4), 147-162.

Reviewer 2 (Kendra Murray)

I was delighted to receive a revised version of this manuscript. The authors have addressed my major criticisms. With a few minor revisions (see below) in my opinion it is ready for publication.

We thank the reviewer for the supportive comments.

MAJOR COMMENTS

1. Align kinetics and partial retention/annealing temperatures listed starting at line 115.

a. He systems: The Farley 2000 and Reiners 2004 kinetics have different closure temperature ranges than the modern kinetics models. However, the PRZ temperatures reported for the AHe and ZHe systems starting at line 115 appear to be for the modern kinetics (but there are no citations in the text to confirm where these PRZ temperature ranges are coming from). For internal consistency, please report the canonical temperature sensitivities that correspond to the kinetics models being used: ZHe: 160-200˚C, AHe: 55-80˚C. These can be found in Table 1 of Reiners et al., 2005 (Reiners, P.W., Ehlers, T., and Zeitler, P., 2005, Past, Present, and Future of Thermochronology: Reviews in Mineralogy and Geochemistry, v. 58, p. 1–18, doi:10.2138/rmg.2005.58.1.).

b. AFT: Where is the AFT PRZ range reported in line 116 (~60-110˚C) coming from? Given the kinetics described in section 2.2, I assume this should align with the AFT Tc range also reported in Table 1 from Reiners et al. (2005): 90-120˚C. This canonical Tc range also appears to better align with the tT history shown in Figure 1a and the narrative description of the simple model results at lines 122-123 (i.e., exiting the PAZ at 15 Ma agrees with the low-T end of the PAZ being at 90˚ not 60˚ on Figure 1a).

As suggested, we have changed the reported PRZ and PAZ temperature ranges to align with the values in Reiners et al. (2005) and have cited that paper (Lines 116-119).

2. I remain dissatisfied that the ASPECT model is not being used to solve for a more realistic but simple geodynamic scenario for Section 3.3. Presumably this software is capable of solving such a simple advection-diffusion problem — why be “deliberately unrealistic”? (Line 209). But, the limitations are now sufficiently discussed within Section 3.3.

As suggested by Reviewer 1 above, in our revision, we have updated the simple exhumation model to have a conductive geothermal gradient through 20 km of upper crust with a density of 2.8 g/cm^3 and a thermal conductivity of 2.5 W/mK. The model domain has a surface temperature of 0°C and a basal temperature of 600°C, which corresponds to a basal heat flow of 85 mW/m^2 combined with radiogenic heat production of 1 x 10^{-6} W/m^3. This geotherm remains in steady state prior to exhumation, and isotherms are perturbed and moved towards the surface during exhumation before returning to steady state after exhumation. We have added 100°C and 300°C isotherms to Figure 5 to illustrate this graphically. This change results in a slight younging of ages, which we discuss in the text, along with the new model parameters (Lines 200-233).

However, the beginning of section 4 (lines 239-240) contradicts the idea that the simple model is "deliberately unrealistic": "The exhumation model described above is deliberately simplistic in order to demonstrate expected patterns of thermochronometric ages in a highly controlled system." Deliberately simplistic is different than deliberately unrealistic, and if the goal is actually to demonstrate the "expected" age patterns, that is not what the scenario in 3.3 is doing.

Please be more clear about the goals of the model that is presented in Section 3.3 and ensure the text is internally consistent on this point.

Now that we have implemented a more realistic temperature structure and described it in Section 3.3, the description in Section 4 should now be accurate and internally consistent. We have removed prior descriptions of this model as "deliberately unrealistic."

TECHNICAL CORRECTIONS

Lines 115, 116, 120, etc: For clarity, rather than using "above" and "below" to describe temperatures relative to partial retention/annealing temperatures, use "warmer than" and "colder than". Partial annealing/retention "zones" refer to specific depths, so "above" or "below" makes sense in that context (e.g., lines 203-205). But, because the above = colder whereas below = hotter translation won't be intuitive for many readers, I suggest that throughout the paper, the authors choose either depth or temperature as the reference frame for partial retention/annealing behavior and only use the corresponding above/below or hot/cold terminology.

Here, and throughout the manuscript, we have revised to use either "warmer" vs. "cooler" or "shallower" vs. "deeper" rather than "above" vs. "below" to clarify temperature vs. depth and avoid confusion. We thank the reviewer for this very helpful suggestion.

Line 123-124: "This example indicates how ages within these systems will produce distinctly different ages from the same time-temperature history due to their variable kinetics." Unclear; ages do not produce ages. Rephrase for clarity.

We have removed "ages within" from this sentence, which was a typographical error.