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Interactive comment on "Testing the effects of topography, geometry and kinematics on modeled thermochronometer cooling ages in the eastern Bhutan Himalaya" *by* Michelle Gilmore et al.

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We appreciate the time and energy that the reviewer put into the evaluation of our manuscript. The comments and questions were insightful and addressing them has improved the quality and the clarity of the presented science. We have arranged our response by 1) reiterating the comments of the reviewers 2) providing our response and clarifying where the comment was addressed in the revised manuscript.

RC3 Dear Colleagues In this manuscript the authors present results of sensitivity of predicted thermochronological age distribution on several parameters: prescribed topographic evolution, geometry of the basal detachment and kinematics of the related fold-and-thrust belt and crustal heat production. The authors conclude that “this study presents a successful approach for using thermochronometer data to test the viability of a proposed cross section geometry based on forward models of the kinematic, exhumational, and thermal history of an area”. I fully agree with this statement but have several comments that could help authors improve the manuscript and help reader better evaluate the contributions. I concur with the comments by referee #1 and try not to repeat them here. I apologise for several self-citations, but my research group has been working in the area and applying similar research techniques since couple of decades. General Comments: 1. The general limitation of the kinematic models is that the geometry and kinematics is prescribed – Therefore despite their best efforts dependent on authors’ interpretation.

[reply] This is true, the geometry and kinematics are both prescribed, but they are also testable. Following this approach, a cross-section can be invalidated by not matching available cooling data –which is an important step forward. Although this approach seems limiting, it has the potential to refine the geometry of the active décollement in addition or as a compliment to inverse methods. The determination of a décollement through searching a parameter space (see response to general comment 3 below) provides low broad posterior probability density functions (PPDFs) that may have a permissible range in depth of 3-5 km (Coutand et al., 2014). Within that range we can test a specific geometry and require it to match additional known constraints such as the surface geology. See additional comments to RC1 and RC2 on cross-section solutions. Multiple sections of the manuscript reflect the ability to test different geometries and as well as different (albeit prescribed) kinematics using the approach of this study. The revised manuscript retains this emphasis.

1, continued. I agree that this is still the best approach to interpret the spatial pattern of thermochronological data, and couple of authors of this manuscript have made significant progress with their previous publications (McQuarrie and Ehlers, 2015) in reducing these limitations. Unfortunately, the additional problem with the Pecube is that it cannot generate simultaneous movement on faults with opposite sense of slip. In the Himalaya, and in particular for the GHS, the cooling and exhumation were affected by the simultaneously motion along the MHT at the base and the South Tibetan Detachment (STD) at the top. The STD in the eastern Himalaya was active as a ductile shear zone until 11 Ma, which is half of the period of the here presented experiments. Could the “tectonic denudation” affect the cooling pattern of the northern part of the section?

[reply]There are two important points here. 1) Using the modified version of Pecube as presented in this paper, we actually can generate simultaneous motion on both the MCT and the STD. This can be done in Move by first applying 10 km of motion to the MCT, then 10 km of motion to the STD, and finally accounting for the flexural load and resulting change in topography. The resulting displacement field would show pure extrusion of Greater Himalayan rock in prescribed 10-km increments (or increment value of choice). 2) Although we could, we did not include simultaneous motion of the STD. This choice was made for a variety of reasons; 1) early STD magnitude is largely unconstrained and predates the ages preserved in the thermochronometers systems used in this manuscript, 2) not including STD motion (i.e. potentially more recent activity) allows us to evaluate what component of the low-temperature (ZHe or AFT) exhumation required extensional exhumation from 10-0 Ma. Tectonic denudation could absolutely affect the cooling in the northern part of the cross section. However, based on the match between our best-fitting models and measured cooling ages, we argue that any recent (7-0 Ma) tectonic denudation is minimal. The critical dataset needed in the north would be MAr ages. These data should record the earlier ($\sim$ 11 Ma ??) cooling signal of the STD. We do suggest potential links between the periods of rapid shortening and STD activity in section 5.3 of the resubmitted manuscript.

2. The shape of isotherms and their effect on the cooling rates. Himalaya are an active contractional orogen, therefore, the isotherms are deformed and the geothermal gradient is not constant in space and time. Was this accounted for in the experiments when

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calculating the eroded material or when calculating the exhumation rates? For example the same rock uplift rate, minus same surface erosion rate will not yield the same cooling rate. Therefore because the exhumation rates are based on thermochronology, i.e., cooling rates, thermochronological data cannot be simply converted into exhumation rates based on an assumed geothermal gradient. The exhumation rates will depend on local instantaneous geothermal gradient at different times. This is not discussed in the manuscript.

[reply] Geothermal gradients and the resulting shape of isotherms in the model are dynamic and change at each incremental time-step based on 1) thermal parameters prescribed to each model in Pecube; 2) locations and magnitudes of fault displacement; 3) locations and magnitudes of erosion as dictated by structural uplift, isostatic flexure, topographic evolution, and erosion in the flexural-kinematic model; and 4) the rates of deformation and exhumation which are dictated by the absolute timing of each step which we assigned as input in Pecube. We reproduce the same inverted thermal gradients at the MCT (when active) and KT (when active) that have been both proposed and modeled for these structures. Reviewer 3 is correct in that this point should be explicitly stated in the manuscript for clarity. Revisions were made in sections 3.2 and 5.1.

3. The authors write that they have performed a sensitivity analysis. However they have performed a limited number of experiments changing one or two parameters at the time (I concur with the related comments by referee #1). However it would have been better to perform a systematic search through the parameter “space” by providing the ranges of variables and searching for the most optimal value – the lowest misfit. I agree that this is a very time consuming approach, which requires tens of thousands of experiments. However this is the only approach that can provide a statistically relevant evaluation of any of the parameters. Pecube produces posterior probability density functions (PPDFs) for each model parameter, (Braun, J., P. Van Der Beek, P. Valla, X. Robert, F. Herman, C. Glotzbach, V. Pedersen, C. Perry, T. Simon-Labric, and C.

Prigent (2012), Quantifying rates of landscape evolution and tectonic processes by thermochronology and numerical modeling of crustal heat transport using PECUBE, Tectonophysics, 524-525, 1–28, doi:10.1016/j.tecto.2011.12.035.âĔŸA′l) I admit that I do not know if this can be implemented by the technique presented here (combination of Pecube thermokinematic modeling and Move kinematic modeling).

[reply] The variables that are assigned in Pecube (in particular heat production properties) can be determined by a systematic search through parameter space. However, the much more interesting and debated properties such as cross section geometry, kinematics, velocity, and topography are all a function of the flexural-kinematic model generated in Move. For these models, designing a parameter search or graphically representing a parameter search is much more complicated. For example, supplementary data Figure 1 shows 9 different models where topography, kinematics, or geometry were varied. These 9 models all produced a foreland basin, dip of the decollement and surface geology that were all considered acceptable (within 1 km of modern thickness; +1/ -0.5° of modern dip; and 1 km of modern surface geology). Over 50 other flexural models were tested that did not match these criteria. Of the 9 models that are presented in this study, each model was run using 4-7 different velocities to 1) see predominant trends on the predicted cooling ages and 2) determine which combination of velocities resulted in predicted cooling ages that best matched the measured data. For all of the different velocities and the different kinematics and geometries we examined a range of thermal properties, specifically Ao (surface radiogenic heat production – see response to comments from supplementary document (annotated manuscript) p. 9, l. 8 below), which has a large, known range of measured values (i.e. Ray and Rao, 2000; Menon et al., 2003; England et al., 1992; Whipp et al., 2007; Herman et al., 2010 – all references in manuscript). Ao was varied in 0.25 to 0.5 μW/m3 increments. Note that we do not test the effect of basal heat production, but rather hold that fixed at 1300°C at the asthenosphere ~110 km (Table 2). While the number of variations we tested is not an infinite number (or 10's of thousands) it is respectably above 500 simulations (in Pecube). The challenge is of course visually showing that range. We understand,

based on comments by reviewer 1 and reviewer 3, that the full range of parameters tested was not clear and we have rectified this in the new version of the manuscript, particularly in section 3.

4. The GHC is not a thrust sheet-the rocks in this lithotectonic units were affected by pervasive and heterogeneous ductile deformation. Similarly the MCT is not a fault but a several kilometers thick ductile shear zone with mylonites derived both from footwall block rocks and the hanging wall block rocks. All these rocks deformed as viscoelasto-plastic thermally activated materials and ought to be modeled as such not as Mohr-Coulomb materials. I do not question the applicability of cross section balancing and thermokinematic modeling for the rocks and structures that were dominantly deformed as the latter mechanisms. Therefore the particle displacement paths were not as simple as implemented by thermal-kinematic models. In conclusion, these models are applicable for the period after the cessation of pervasive ductile deformation. This is regardless weather the lithotectonic unit was emplaced according to the channel flow tectonic mode or to the classical fold nappe mode. In either case the pervasive ductile deformation occurred before the thermochronological record used here. Finally, the thermochronological data presented here and available in general cannot constrain the tectonic processes that occurred before them.

[reply] The short answer is that we completely agree with Reviewer 3 that “the thermochronological data presented here and available in general cannot constrain the tectonic processes that occurred before them.” The available cooling age data we are evaluating are all younger (MAr ages of 11-14 Ma) than the MCT emplacement (23-16 Ma). However, what is interesting about the model process is that some of the models (such as our best fit model between 80-90 km from the MFT) predicts MAr ages that are a result of the proposed age and rate of the MCT. We do not have data in this region, so it would be interesting to see if they are in fact as old as what is predicted. With respect to the MCT as a fault or a shear zone, the boundary between uniquely Greater Himalayan rocks (by provenance) and Lesser Himalayan rocks (again by provenance)

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is actually quite discrete (« 1 km). However, we absolutely agree with Reviewer 3 that the shear imparted to the rocks above and below this zone is pervasive and heterogeneous and that the emplacement of the MCT on the LH rocks occurred while both lithotectonic packages were not behaving in a purely elastic or brittle fashion. We also agree that the AFT, ZHe, and MAr cooling ages reproduced accurately by the model all reflect cooling after predominantly ductile deformation in these rocks. It is important to note that the models used (Move and Pecube) do not attribute any mechanical behavior to the rocks. They only describe kinematics, or the motion of material. The kinematics invoked here are just as discrete as the kinematics used in Coutand et al. (2014) at 600°-700°C at 20-30 km depth or Herman et al. (2010) at 600-700 °C and 20-30km depth. The kinematics modeled in Move do not differentiate how ductile or plastically the rocks are deforming internally. The emplacement of the Greater Himalayan rocks above Lesser Himalayan rocks is critical for the heating and cooling of the Lesser Himalayan rocks and thus needs to be in the model. If the data we were evaluating were sensitive to the compressed temperature gradient (∼450-700°) across the MCT zone (∼ 1 km below and above in Bhutan; e.g. Long et al., 2016), trying to replicate the magnitude of fault-parallel shear would be more critical. As stated in response to General Comment 1, with respect to sensitivity of the cooling ages to the STD, the model predicts a cooling history and exhumation age and rate for GH rocks that can be compared to measured histories to assess how close just the simple (albeit possibly ductile) thrust emplacement model can account for the measured temperatures before attempting to incorporate a much more complex process. The manuscript was revised to clarify these points in Sections 2 and 3.

5. The authors analyze and discuss the effect of the thermophysical properties of the rocks on the spatial pattern of cooling ages. However only the values of heat production were changed (2 and 4 μW/m3). However the thermal properties control the Péclet number, which dictates how strongly are the isotherms deflected because of the thrusting. This furthermore implies that the thermal properties have to include the study of sensitivity on thermal conductivity, heat capacity and density of the rocks.

[reply] We agree with Reviewer 3 that crustal thermal fields are sensitive to thermal conductivity, heat capacity, and density. In this study (and most exhumation studies), crustal thermal properties are assumed constant because most thermal models, including Pecube, solve the advection-diffusion equation on an Eulerian grid which is not capable of tracking moving material properties. This makes implementation of variable thermal conductivity in a highly deformed thrust belt impossible. Lagrangian grids circumvent this problem, but have shortcoming for exhumation studies. They cannot accommodate large amounts of deformation, such as in the Himalaya, without becoming unstable, and require frequent re-meshing and interpolation of model parameters and properties, thereby progressively introducing numerical uncertainty into the model. Although hybrid Eulerian-Lagrangian techniques exist, these are not commonly used and difficult to implement. Given these limitations, we (like most other studies) use average upper crustal thermophysical properties and assume they remain constant through time. However, please note that the thermophysical properties we do use are based on observations (largely from Whipp et al., 2007 and references therein). To accommodate this reviewer's concern, we have modified the manuscript in the following ways: 1. We more clearly state in the model setup section 3.2.1 that we are using observed thermal physical properties for the lithologies present in this region (see Whipp et al. 2007, and Ehlers, 2005). 2. We add a caveat statement in the same section to say: "Although thermophysical properties such as thermal conductivity, heat capacity, and density vary between different lithologies within a fold and thrust belt, the implementation of variable material properties in areas of large deformation is not possible in programs such as Pecube which solve the advection diffusion equation on an Eulerian grid. Thus, we address this potential issue by using the best available average measurements of thermophysical properties for the lithologies in this region." In addition, see response to RC1 (p. 9 l. 11) for surface radiogenic heat production.

Specific Comments: 1. Valla et al. [2010] have shown that relief development must be 2–3 times faster than the background exhumation/erosion rate to be recorded and quantitatively extracted from thermochronological data. Valla, P., F. Herman, P. A. van

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der Beek, and J. Braun (2010), Inversion of thermochronological age-elevation profiles to extract independent estimates of denudation and relief history I: Theory and conceptual model, Earth Planet. Sci. Lett., 295, 511–522. Please comment in your manuscript in the relevant places.

[reply] The approach and conclusions of Valla et al. [2010] is included in Introduction and Discussion.

2. What is the evidence in the field (i.e., petrological) for the burial by Kakhtang thrust? Kakhtang thrust appears very steep therefore the burial rate might not be high. In addition the KT emplaced some of the hottest rocks in the Himalaya therefore the isotherms might have been disturbed during its activity, in other words heating and cooling does not need to imply burial and exhumation.

[reply] We agree with the reviewer's comments. However, the flexural response of motion on the steep Kakhtang Thrust is subsidence in the footwall. Modeled isostatic accommodation of this thrusting dramatically lowered topography in the footwall of the thrust and reduced erosion rates south of the thrust. In some models, enough subsidence occurred during out-of-sequence thrusting that sedimentation occurred in the immediate footwall. A potential relict of this footwall subsidence is the enigmatic low-relief surface preserved in the Bhutan Himalaya (Duncan et al., 2003; Grujic et al., 2006). This low-relief landscape contains hundreds of meters of sediment infilling of paleo-relief and is now out of equilibrium with respect to where it was formed (Adams et al., 2016). In eastern Bhutan, the infilled sediment is derived from the structurally higher GH; conglomerate is common, thus making it easy to associate the clasts with rocks carried by the KT. As published by reviewer 3, the low-relief surface is in the immediate footwall of the Kakhtang Thrust (Grujic et al., 2006). Our flexural modeling of this region and others (e.g. McQuarrie and Ehlers, 2015, 2017; Rak et al., 2017) has highlighted the ubiquitous response of footwall subsidence and the development of low relief in the footwall region of out-of-sequence faults, thrusts, etc. We do not think that the spatial relationship between the low-relief surface and the Kaktang thrust is coinci-

dental. Not only do slower erosion rates in the Kakhtang Thrust footwall alter thermal gradient, but the reduced topography limits the magnitude and rate of future erosion. The current model does take into account the deflection of isotherms as the KT moves and advects deep, hot material upward during fault motion. The amount of disturbance to isotherms in each time-step is related to the geometry of the fault and magnitude and rate of motion assigned in the time-step. Section 3.1.2 has been revised to explain these observations in the footwall of the KT and our rationale for modeling different timings of out-of-sequence thrusting. Section 4 has also been revised based on these comments.

3. Technical corrections a) Vertical uplift and vertical exhumation. Both rock and surface uplift and exhumation concern the vertical component of the particle displacement (in three different reference frames). Therefore word vertical is superfluous. However one must make difference between rock uplift and surface uplift, in particular in an article like this one where both processes are discussed. Please adhere strictly to the definitions by England and Molnar, 1990. Surface uplift, uplift of rocks, and exhumation of rocks. Geology, 18(12), pp.1173-1177. b) There is no process named “surface radiogenic heat production”. Please correct the wording accordingly in the entire document.

[reply] Language was corrected to reflect England and Molnar definitions.

RC3: All the above comments and further technical comments are in the annotated file. Please also note the supplement to this comment: https://www.solid-earth-discuss.net/se-2017-117/se-2017-117-RC3-supplement.pdf

[reply] The corrections suggested in the supplementary document were all addressed. Specific comments raised in the Supplementary PDF that are not addressed above are included below.

p.3, l. 30-31 furthermore as indicated by thermo-kinematic experiments, like this study, the cooling rates were not steady in time and space.

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[reply] We agree completely with this statement but feel the point is best addressed in the results and discussion sections. We have removed interpretations of "rapid cooling" and or processes and simply describe the age data.

p.4 line 5 Is there any effect of sample elevation? This is important since you are testing the model for the sensitivity on surface processes.

[reply] There are very limited/ modest age elevation relationships as discussed in McQuarrie and Ehlers, 2015. The relationships that are present include 1) the samples from Coutand et al. (2014) that have an age-elevation relationship, and if used to determine an exhumation rate, suggest a very modest rate of 0.4 mm/yr; and 2) the southern ZHe samples when combining the data from the Kuru Chu and Trashigang transects, located ∼30 km from the MFT. The younger ages (8.5 to 10 Ma) at lower elevations (0.5 to 1 km) in the Kuru Chu and older ages (11 to 11.6 Ma) at higher elevations (1.6 to 2.4 km) along the Trashigang transect suggests differential exhumation of 0.7 mm/yr. These are expanded on in sections 2.2 and in 5.3.

p. 4, l. 20 since it is an active convergent orogen the isotherms are deformed and the geothermal gradient is not constant in space and time. Was this accounted for in the model when calculating the eroded material?

[reply] In this section we mention that advection-diffusion thermal models are used to calculate the evolving subsurface temperatures (i.e. modified isotherms and geothermal gradient) but discuss this more explicitly in section 3.2.

p. 7, l.30 What is the argument that the geometry along distant section is applicable to the study area? How do these values compare to the estimates by Coutand et al. (2014) and by Singer, J., Obermann, A., Kissling, E., Fang, H., Hetényi, G., Grujic, D. (2017) Along-strike variations in the Himalayan orogenic wedge structure in Bhutan from ambient seismic noise tomography. Geochemistry, Geophysics, Geosystems, 18, 4, 1483-1498. DOI: 10.1002/2016GC006742.

[reply] We have removed the INDEPTH reference. We found that Singer et al. (2017, JGR Solid Earth) had the specific data on to the dip of the décollement and the dip of the Moho. The relationship between the décollement geometry of Long et al. (2011b) and Coutand et al. (2014) is no different than that described in McQuarrie and Ehlers (2015; figure 3C). However the modified décollement geometry is much closer to EB1 in Coutand et al. (2014), with more discrete ramp steps.

p. 9, l. 8 ["surface radiogenic heat production"] –There is no such physical process. Please correct the wording accordingly in the entire document.

[reply] Author Response: With due respect, yes, there is. Please see detailed response to comment: p. 9 l. 11 from Reviewer 1: Radiogenic heat production at the surface can vary spatially by large amounts (e.g., Mareschal and Jaupart, 2013) and is a function of the concentration of heat-producing elements in the crust. Systematic sampling of crustal rocks now exposed at the surface indicates that heat production diminishes with depth through the crust and that this decline is not monotonic (Ketcham, 1996; Brady et al., 2006). Thus we prescribe an exponential decrease in heat production with depth, as opposed to assuming a constant crustal heat production. An exponential decrease in heat production with depth requires definition of a surface radiogenic heat production (Ao) and an e-folding depth.

p. 13, l. 25 there is no such a process. Do you mean surface heat flow or (radiogenic) heat production

[reply] Neither, please see response to p. 9, l. 8.

p. 15, l.2 The GHC is not a thrust sheet as the MCT is not a fault but a several kilometers thick ductile shear zone. Therefore the particle displacement paths were as simple as predicted by thermal-kinematic models. Therefore these models are applicable for the period after the cessation of pervasive ductile deformation.

[reply] In the end we may need to continue to agree to disagree with Reviewer 3 on this

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point. Our definition of a thrust sheet is not quite that rigid (literally and figuratively). We have added “ductile” in front of “thrust” because we agree with Reviewer 3 that the fault that places Greater Himalayan rocks on Lesser Himalayan rocks is part of a much broader shear zone with pervasive shear both above and below the tectonostratigraphic boundary between the two units.

p. 15, l.25-29 Do you consider also that the isotherms are less deflected above this ramp than above the major ramp? This influences how closely spaced are the isotherms and therefore even with the sample particle displacement vector and surface denudation rate, the cooling rate will be different.

[reply] Yes, absolutely. The version of Pecube that we are using calculates the evolving thermal field including the deflection (or lack thereof) of isotherms with ramps.

p. 16, l. 28 Is there a justification to increase the number of significant digits (i.e. topography angle of 1.75°)

[reply] Yes, small changes to small angles (0.25° is 12% of a 2° angle) have a large effect when applied over hundreds of kilometers.

p. 18, l. 5 [The sensitivity of the model to the age of MCT] –However this depends also on how is the MCT treated. As a single fault with a displacement of a slab above it or as it is in the field as broad ductile shear zone with pervasive deformation in the hanging wall block.

[reply] While we agree that how deformation in the broad MCT zone is treated may have an effect on the temperatures within a few kilometers above and below the thrust fault (within the shear zone), this deformation is not captured by any of the available cooling age data we are evaluating. All of the thermochronometers available are younger (11-14 Ma) than the MCT emplacement (23-16 Ma). Available geochronologic data for this region support ductile shearing on the lower STD between circa 23 and 16 Ma, and shearing and associated exhumation of GH rocks in the MCT sheet after circa 23 Ma

and continuing until circa 18–16 Ma [Grujic et al., 2002; Daniel et al., 2003; Kellett et al., 2009, 2010; Chambers et al., 2011]. Thus our statement that we cannot evaluate the MCT emplacement age or rate is correct.

Figure 2 and Figure 4: [pointing to northern part of cross-section]–Something is missing here, both the topography and the geology.

[reply] The geology and topography in the northernmost portion of the cross section was never a part of the original geologic cross section (Long et al., 2011b). This is why it is blank in this figure as well.

Please also note the supplement to this comment:
https://www.solid-earth-discuss.net/se-2017-117/se-2017-117-AC3-supplement.pdf

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