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Discussions

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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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 prescribedâĂŤtherefore despite their best efforts dependent on authors' interpretation. 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?

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 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. 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.âĂÍ) 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). 4. The GHC is not a trust 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 visco-elasto-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. 5. The authors analyse 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.

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

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

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