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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.

Anonymous Referee #2

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Review of the manuscript entitled "Testing the effects of topography, geometry and kinematics on modeled thermochronometer cooling ages in the eastern Bhutan Himalaya" by Gilmore et al.

This manuscript analyzes the impact of variable radiogenic heat production, convergence rate, topographic estimates and out-of-sequence thrusting in determining the pattern of previously published thermochronologic ages along a transect across the Bhutan Himalaya. The authors utilize their results to validate a revised cross-section geometry of the study region.

The manuscript is generally well written. The topic is of potential interest for a broad

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international audience. However, it would benefit from a more comprehensive discussion of the whole range of geologic processes that may have an impact on the thermochronologic record of the study area.

The modelling approach utilized in this work is based on flexural and thermal-kinematic models. The authors sequentially deform the study cross section, and apply flexural loading and erosional unloading at each step to develop a high-resolution evolution of deformation, erosion, and burial over time. In other words, their approach only considers relatively shallow geologic processes. Deeper tectonic processes (e.g., channel flow exhumation and slab breakoff) that may also affect the thermochronologic record, especially higher temperature systems such as Ar-Ar on mica, are not discussed. This may puzzle part of the potential readership. I suggest to improve on the discussion, and possibly the modelling, in order to include these issues.

The dataset of previously published thermochronologic ages, which is utilized as a benchmark for modelling, is not homogeneous. AFT and ZHe data are available in most of the transect, but Ar-Ar data are not. This would suggest more caution in the conclusions based on modelling results. Moreover, these ages are invariably interpreted as cooling ages during exhumation across the closure temperature of the Ar-Ar system. Petrologic studies demonstrate that micas in metamorphic rocks often preserve disequilibrium textures, and their Ar-Ar age may thus record fluid-induced recrystallization below the closure temperature, rather than monotonic cooling (e.g., Villa 1998 - Terra Nova). Why mica Ar-Ar ages are so different in samples that are so close each other? What is the potential role of recrystallization during deformation? These issues should be discussed in the revised main text.

Some of the findings of the authors are not surprising for an active orogenic belt such as the Himalaya, notably the minor effect of radiogenic heat production and topography compared to tectonics. Nevertheless, the authors' conclusion should be supported by more robust thermochronologic data. The addition of a new ramp under the Greater Himalaya does better explain available thermochronologic ages. However, this is just

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one of the possibilities, given the degree of freedom of the models.

Is the stratigraphy predicted by modelling consistent with the geologic record? This may provide independent constraints to the reconstructions illustrated in this work, that are prone to remain otherwise speculative. I suggest to describe in more detail the stratigraphic evolution of the foreland basin, as well as all of the other geologic evidence that may be useful to support the authors' conclusions.

The abstract should be improved. The first two sentences are not relevant to introduce the focus of the manuscript. The Introduction and section 2.1 are biased by excessive self-referencing.

I will be happy to read a revised version of this potentially interesting manuscript.

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