Author Response to SE-2017-117-RC2 (Anonymous Referee #2, 2018)

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 (black text) 2) providing our response (dark red, indented text) and clarifying where the comment was addressed in the revised manuscript.

RC2

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 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.

> A discussion of more ductile processes on the higher temperature thermochronometer systems was raised by Reviewer 2 and Reviewer 3. The flexural and thermokinematic model looks at the evolution of rocks from 30 km depth and ~ 600-700 °C (peak temperature produced in Greater Himalayan rocks in the thermokinematic model, Pecube). As mentioned in the reply to Reviewer 3, the kinematic model will not capture all of the deformation processes, but it can evaluate if the cooling through the closure temperature of the MAr system was simply a function of shallower fold-thrusts belt processes — or if deeper processes (such as channel flow or slab break off) are needed to explain the data. Also, channel flow (if active) is interpreted to be reflected in the much higher temperature monazite data, which is not modeled in this study. What is key to note is that the kinematics described here can reproduce the peak temperatures and cooling history recorded in the rocks.
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> We have made minor revisions in multiple sections of the manuscript to incorporate this discussion raised in RC2 and RC3: 1) *2.1 Tectonostratigraphy* states that the Greater Himalaya

> was deformed through ductile processes, and that MCT shear is pervasive above and below the fault, 2) *3.2 Thermal Model* includes clarification on the depth and temperature range of the model as well as how isotherms are advected by motion along faults, 3) The discussion section clarifies permissible processes to reproduce the measured ages (including MAr).

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.

> The available MAr data for this transect are very limited and were previously published by Stüwe and Foster (2001). The ^{40}Ar-^{39}Ar age spectra show relatively flat but slightly discordant age spectra that were interpreted to represent cooling ages for all 4 samples. The two sets of 11 Ma and 14 Ma ages were interpreted to record the same cooling signal that had been repeated by a fault. Our interpretation is broader and proposed that the 11-14 Ma ages represents a permissible age range in which rocks have passed through their closure temperature due to the short spatial scales between samples. Recent work from Sikkim Himalaya across the same Lesser Himalaya to Greater Himalaya transition highlights natural variability in MAr ages due to both the thermal conditions experienced by micas and the residence time at those temperatures. They measured both single grain ages (for 5-11 grains) as well as more traditional plateau age (Mottram et al., 2015) across a transect that spanned a temperature gradient over ~ 5 km. They found a significant spread in the single grain ages (2-5 Ma not including errors) and that the spread decreased (to 1.5-2 Ma) with higher temperatures and longer predicted residence times at those temperatures, suggesting that the duration of metamorphism and the temperatures reached affected the loss of Ar from mica. In each case the MAr plateau ages spanned over a much narrower age range (13- 13.4 M) with significantly more precise error bars (0.05-0.2 Ma) than the single grain ages. The ~ 5 km transect crossed temperatures that ranged from 580°c to 650°C, while the maximum temperature range for the MAr samples presented here were between 600° and 700°C (Daniels et al., 2003). Their study also showed that a dispersion of +/- 2 Ma would be expected due to diffusive differences caused by grain size variations. We do not have access to the samples to go back and examine the textures of the mica that produced the cooling ages. However we have looked at many similar rocks from almost the exact same area and have found no textures indicative of fluid flow or alteration. While this does not rule out an age spread from post-cooling fluid flow or recrystallization during deformation, we are confident that the 11-14 Ma age range encompasses the actual cooling age of these rocks because of strong similarities in age to data available directly to the east near the Kuru Chu section (~12 Ma,

Long et a., 2012; Figure 9 in this manuscript), as well as the range in ages measured by Mottram et al. (2015 in Sikkim (12-16 Ma). These ages are all younger than the youngest age for south-directed shear in GH rocks, 16-18 Ma (Grujic et al., 2002; Daniel et al., 2003; Kellett et al., 2009). In our model, the age and rate of deformation in the northern duplex of lower Lesser Himalaya most prominently control the predicted MAr ages modeled in this area of the Greater Himalaya.

Text was revised to address this point in sections 2.1, 2.2, 3.2, and 5.3. New citations are also included, i.e.:

Mottram, C. M., Warren, C. J., Halton, A. M., Kelley, S. P., and Harris, N. B. W.: Argon behaviour in an inverted Barrovian sequence, Sikkim Himalaya: The consequences of temperature and timescale on 40Ar/39Ar mica geochronology, Lithos, 238, 37–51, doi: 10.1016/j.lithos.2015.08.01, 2015.

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

Compared to other regions, even in the Himalaya, the dataset shown in this paper is rich, especially when including the data immediately east along the Kuru Chu transect as shown in Figures 9-11. MAr and AFT data are more limited than ZHe data due to cost and appropriate samples respectively. The reviewer raises an important point and that is, the models highlight regions where the predicted thermochronologic ages are very sensitive to the geometry or radiogenic heat production or velocity. Knowing these areas prior to collecting thermochronology samples would strongly influence where sampling would be the most useful for delineating geometry. Regrettably many of the gaps in the AFT data are a function of the apatite-poor lithology. Resampling and additional analyses are beyond the scope of this paper. However, the model process we present is useful for directing future thermochronologic work in the Himalaya and other mountain ranges. Although many geoscientists model data following the collection of samples, this work suggests that initial thermokinematic modeling of an area prior to collecting data can direct and inform sampling strategies.

We are not sure what other possibilities the reviewer envisioned for changes to the cross-section to also explain the published dataset. We chose to highlight an obvious additional structural solution that was proposed to the east in Arunachal Pradesh: an out-of-sequence fault at the trace of the MCT (Adlakha, V. A., Lang, K. A., Patel, R. C., Lal, N., and Huntington, K. W.: Rapid long-term erosion in the rain shadow of the Shillong Plateau, Eastern Himalaya, Tectonophysics, 582, 76–83, doi: 10.1016/j.tecto.2012.09.022, 2013.). As expanded on in section 5.2, *Using Thermochronology to Evaluate Structural Geometry*, we evaluate whether an out-of-sequence fault can explain all of the observations. While it may be able to address the younger cooling ages, having a second, more southern out-of-sequence fault that post-dates the Kakhtang Thrust would have a pronounced effect on the topography (as highlighted in our response to reviewer 3, specific comment 2), that is not seen in the model topography or geomorphic metrics of active/ recent uplift. In addition, see response to RC1 for further comments on systematic approach to structural and thermal modeling.

We have revised this manuscript to clarify these points in sections 5 (*Discussion*) and 6 (*Conclusion*s).

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.

One of the key parameters that we match through this process is the depth of the foreland basin. The modeling process also makes strong predictions regarding the detrital sedimentary signal recorded in the basin and the potential detrital thermochronologic record. Most of this research was accomplished as another research group was examining the details of the detrital climate, provenance, and sediment accumulation signal in the Siwaliks of Bhutan (e.g. Coutand, I., Barrier, L., Govin, G., Grujic, D., Dupont-Nivet, G., Najman, Y., and Hoorn, C.: Late Miocene-Pleistocene evolution of India-Eurasia convergence partitioning between the Bhutan Himalaya and the Shillong plateau: New evidences from foreland basin deposits along the Dungsam Chu section, Eastern Bhutan, Tectonics, 35, 2963–2994, doi:10.1002/2016TC004258, 2016. and, Govin, G., Najman, Y., Copely, A., Millar, I., van der Beek, P., Huyghe, P., Grujic., D., and Davenport, J.: Timing and mechanism of the rise of the Shillong Plateau in the Himalayan foreland, Geology, doi:10.1130/G39864.1, 2018). As with any provenance or stratigraphy study, most information is gained when there is a unique signal that enters the foreland basins, and these papers highlight that much of that signal is associated with the rise of the Shillong Plateau or ages that have a Tibetan origin.

The paper by Govin et al. (2018) highlights that at 6.35 Ma, there is significant input of Lower LH detritus into the foreland basin. Our models show both the age (6.35 Ma) and the signal (lower LH detritus), and the depth of the basin at this time (2.75 km), are all consistent. We agree with Reviewer 2 that matching the predicted foreland basin with the measured foreland basin is a powerful tool for evaluating the flexural-kinematic modeling and rates of deformation. We are currently working on a fully-integrated detrital provenance and thermochronologic cooling set for the Siwalik basin, but a detailed description of the stratigraphic evolution of the foreland with respect to detrital provenance cooling signals and rates is well beyond the scope of this paper to do it properly.

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.

Abstract issues were raised by multiple referees and have been addressed.

Introduction and section 2.1 have been revised to include more references to other research groups as available. In general, 26 new references (not self-citing) have been added to the manuscript.

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