

Solid Earth Discuss., author comment AC3  
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## Reply to RC3

Elco Luijendijk et al.

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Author comment on "An analytical solution for the exhumation of an orogenic wedge and a comparison with thermochronology data" by Elco Luijendijk et al., Solid Earth Discuss., <https://doi.org/10.5194/se-2021-22-AC3>, 2021

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Note: Our replies to the comments by Reviewer 3 are shown below each comment in *italics*

Dear authors,

This article shows a new analytical solution to predict thermochronological dataset in an orogenic wedge. This simple model assumes a transport only accommodates by basal detachment. I appreciate all the details available on the different parameters implemented in this model.

The authors applied this model on a profile perpendicular to the Himalayan wedge. Results make in evidence a good correlation between predicted ages and observed ages using an uniform model without effects of individual tectonic structures. Authors concludes that the principal implication of these results is a good reproducibility of the thermochronological data with a simple model and a possible steady state evolution of the Himalayan orogenic wedge.

The manuscript is well written and we have all the details on the model and the analytical procedure, however major points have to be clarified and discussed, see my general comments.

### **General comments**

1/ The main topic of the article have to be clarified l. 154-155 " *Note that the goal here is not to provide a detailed geological case study, but to demonstrate the use of the equation to calculate deformation and exhumation rates*" but authors highlight in abstract and conclusion "The results also imply that this part of the Himalayas may be in steady-state." (l.7-8) and "This indicates that the Himalayas may be in steady-state and that, at a large scale, the exhumation of mature mountain belts may be approximated by a relatively simple model of uniform and steady-state deformation, accretion and transport" (l. 239-241). This conclusion may be true but it is not relevant for the Himalaya Mountain range using one cross section.

*Reply: This is a good point, and the reviewer is right that based on our study alone, we cannot make inferences for the entire Himalayas. Our results are however valid for the particular region. We have modified the statement accordingly.*

2/ Similar model have been developed by Batt et al. 2001 (JGR), a comparative study of the 2 models or at least a discussion on the main differences between the models must be developed. "However, to our knowledge no analytical solutions exist for the relation between deformation and exhumation of mountain belts." (l.23-24), this sentence is partially true and must be modified to show the specificity of this new model.

*Reply: This oversight has also been raised by reviewers 1 & 2. A sentence on this previous model has been included in the introduction in the new version of the manuscript.*

3/ Constant geothermal gradient is a big assumption in the models and it is not realistic according studies of Coutand et al., 2014 (JGR) and McQuarrie and Ehlers, 2015 (tectonics). This point and the impact on the model must be developed in the discussion. New kinematic models are not implemented (see Ault et al., 2019, tectonics for a synthesis) specify a closure temperature is possible and easier to implemented in the models but residence time in the Partial Annealing Zone (PAZ) and Partial Retention Zone (PRZ) need to be low to assume a closure temperature.

*Reply: Agreed, we have updated the thermal and thermochronology model in the revised manuscript. See also our replies to comments by reviewer 1 and 2.*

4/ Discussion of the results must be more detailed using differences between the predicted and observed ages and to discuss potential fault activity along the cross section. The comparison with study of McQuarrie and Ehlers, 2015 (tectonics) based on the better fit of the new model seems a bit complex. Differences between the 2 models are not specified in the text.

*Reply: Agreed, we have expanded the discussion of the comparison between our results and the previous work by McQuarrie and Ehlers (2015).*

5/ This model applied on only one cross section in orogenic wedge of the Himalayas does not allow to provide strong conclusion on the Himalayas tectonic regime.

*Reply: We agree and would like to refrain from statements about the entire Himalayas, see also the replies to the comments by reviewer 1 and 2.*

You can find detailed comments in the sections below:

### ***Specific comments***

L.5 Precise the location of the transect.

*Reply: Ok.*

L.6-7 Remove this sentence, themochronological data not explained by the model can result of individual fault activity. See my general comment

*Reply: The model fit is relatively good. We therefore feel that claiming that -at a large scale- the mode fits the trend of the data is valid here.*

L.7-8 This conclusion is not relevant if we consider ages do not fit with the model, see my previous comment.

*Reply: We are not sure what the reviewer means here. The model fit is shown and discussed. Why would the conclusion not be relevant?*

L.17 I agree, one of the big advantages of the model presented is the low computational cost compare to other thermo-kinematic models.

L.23-24 Please consider Batt et al. 2001 (JGR), see my general comment

*Reply: Agreed, see previous reply*

L.91 Vxc ?

*Reply: Fixed.*

L.104 Fig.3 to be homogeneous in the text

*Reply: Thanks for noticing, we have corrected this.*

L.106-107 It is not the good caption for the figure

*Reply: It is actually, we are not sure what is wrong with the caption here? Could you specify the issues with the caption?*

L.135-139 Develop the simplified solution is necessary to the discussion ? If not you can remove this part in appendix and just comment the among of differences between the 2.

*Reply: We have removed the simplified solution from the manuscript.*

L.139: Considering the color bar on Fig.5d error decrease in the first kilometers.

*Reply: Agreed, we have expanded the discussion of the errors here*

L.143-145 Is it possible to merge the 2 figures ? Also a common color bar for the 2 models seems more adequate.

*Reply: We feel that merging these 2 figures would produce a figure that would be too large.*

L.154-155 It seems important to clarify the goal of the paper. This sentence suggests that the main goal of the paper is to highlight numerical approach if it is the case other journals are more relevant and therefore you cannot conclude on geological implications.

*Reply: I presume the reviewer means the analytical approach not the numerical? It is true that the manuscript presents an advance in methods and not a case study or a new knowledge of a geological process. However, we feel that the new equation and its potential applications are of sufficient interest to thermochronologist and geologists to warrant publication in Solid Earth.*

L.159-160 m.m-1 ; the choice of each input parameters have to be justified by references

*Reply: The references are mentioned in the first part of the sentence (The geometry of the wedge is based on published geological cross-sections (Long et al., 2012; Coutand et al., 2014; McQuarrie and Ehlers, 2015))*

L.162-164 A table in appendix with all the thermochronological data implemented in the model can be helpful for the reader. You have not selected MAr partially resetted (Long et al., 2012, Tectonics), you have to add a sentence on that.

*Reply: Thanks for noticing. We have clarified why the non-reset MAr samples were not used. The thermochronology data are a 100% copy of data used by McQuarrie and Ehlers (2015) and as such we feel that it would be superfluous to include a table in the manuscript. However, the data are included in the GitHub repository and the zenodo publication of the model code, which we now refer to in the main manuscript.*

L.165 Uniform geothermal gradient is a big assumption on this surface. Important thermal perturbation can be observed at this scale (McQuarrie and Ehlers, 2015, tectonics).

*Reply: The thermal model was updated to include steady-state conduction and advection*

L.165-166 add °C km-1

*Reply: Ok.*

L.167-169 Use references for the different "closure temperature" used and not "resetting temperature", numerous models showed that is a quite large range of temperature in particular if exhumation rates are low (see Ault et al. 2019, Tectonics).

*Reply: We have revised the thermochronology model and have removed this part of the manuscript.*

L.169-172 I agree but it can be an important bias on the model and you can have an idea calculating time residence of each sample in PAZ for AFT and PRZ for ZHe. It can be a sentence to justify that samples do not stay long time in PAZ and PRZ, in this case proxy on closure temperature can be used.

*Reply: We have revised the thermochronology model and have removed this part of the manuscript.*

L.179 Define MAE.

*Reply: There is a definition in the same sentence (mean absolute error).*

L.186 It can be helpful for the reader to locate the different ages and samples on the cross section and having the possibility to link ages with the different tectonostratigraphic units. Scale of the Fig. 7a seems false, it is not 200 km long with this scale.

*Reply: The modeled cross-section was larger than the geological cross-section to avoid boundary effects in the model. The sample locations have been added to the cross-section*

L.198 More discussion about the data and differences between observed and predicted ages can be useful. I am very curious to see if AHe and AFT which not fit come from a particular tectonostratigraphic unit and If it is the case it can be associated to a major fault activity ?

*Reply: We feel that this is out of scope of the manuscript. The model that we propose does not resolve individual faults. Although it could be adjusted to do so, that would warrant a separate publication.*

L.200 Add °C km<sup>-1</sup>

*Reply: We have revised this section of the manuscript*

L.208 Comparison with model from McQuarrie and Ehlers (2015) seems difficult. Your new model fit a global trend but previous model includes more parameters and explain better 2<sup>nd</sup> order trend in the dataset.

*Reply: The general consensus on assessment of model performance is that all else being equal models with less parameters are preferred over more complex models.*

L.209-210: Digitizing from figure is not acceptable for publication. Ask the data.

*Reply: After a repeat request we have now gotten the data.*

L.223-229 As suggested previously, it can be relevant to discuss the differences between observed and predicted ages to discuss the reason of the differences geothermal gradient, fault activity ?

*Reply: We feel that this is out of scope for this manuscript and would require a separate more detailed study.*

L.239-240 This sentence is for a profile in the southern flank of Himalayas and cannot be in the conclusion.

*Reply: Agreed, we have revised this sentence to state that this section in the Himalayas may be in steady-state.*

### **Figures:**

Fig 2 Increase the size of the figure and the font size

*Reply: agreed, this suggestion has been implemented.*

Fig 3 and 4 Increase size of a) b) and c). It can be more lisible same comment for Figs. 3,4,5,6, 8, 9 and 10. The caption of Fig 4 do not correspond at the figure.

*Reply: agreed. We are not sure what is wrong with the caption of Fig. 4.*

Fig 5 and 6: Merge the 2 figures, use the same color bar with the same range of value. Delete minus before isochrons ages.

*Reply: We disagree with the merging of these figures. It would results in a very large figure in which the subpanels would be difficult to read and interpret.*

Fig 7 It can be helpful for the reader to locate the different ages and samples on the cross

section and having the possibility to link ages with the different tectonostratigraphic units. Scale of the Fig. 7a seems false, it is not 200 km long with this scale.

*Reply: We have added the sample positions. And its true that the cross-section is not 200 km, we now explain the reason for this in the main manuscript.*

Fig 10 thermochronometer in the caption

*Reply: ok*

Fig 11  $R^2 = 0.43$  not  $-0.43$

*Reply: The  $R^2$  is actually negative here. Note that the coefficient of determination ( $R^2$ ) can be negative if the variance of the model error exceeds the variance of the data, which is the case here.*