Solid Earth Discussions

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Comment on se-2021-22

Anonymous Referee #3

Referee 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-RC3, 2021

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.

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.

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.

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.

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.

You can find detailed comments in the sections below:

Specific comments

L.5 Precise the location of the transect.

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

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

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

L.91 Vx**c** ?

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

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

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.

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

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

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.

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

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.

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).

L.165-166 add °C km-1

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).

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.

L.179 Define MAE.

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.

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 ?

L.200 Add °C km-1

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 2nd order trend in the dataset.

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

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

Figures:

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

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.

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

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.

Fig 10 the**r**mochronometer in the caption

Fig 11 R2= 0.43 not -0.43