

Solid Earth Discuss., author comment AC1
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Reply to RC1

Elco Luijendijk et al.

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-AC1>, 2021

Note: Our replies to the comments by Reviewer 1 are shown below each comment in *italics*.

Dear Authors,

The authors do present an analytical approach to model the particle motion in an compressional orogen and estimate related thermochronological ages. The model does allow to efficiently change the boundary condition and therefore adjust to fit observed thermochronological data and make inferences about the importance of e.g. compression, detachment velocities and frontal and basal accretion.

Although I personally like the approach and acknowledge the effort the authors did to implement this model, I do not like the focus of the manuscript given the simplicity of the their model.

For further information see my scientific comments:

Scientific comments

- There is a quite similar approach as the one presented here for accretionary wedges (not including 'compression') (Batt et 2001), but this should be mentioned in the introduction and the similarities and differences should be discussed in detail. I would also like to see both approaches applied and visually compared, e.g. for an example without compression.

Reply: We thank the reviewer for pointing us to this paper. The approach referred to by the reviewer is equation 5 in Batt et al. (2001, <https://doi.org/10.1029/2001JB000288>), with a correction published later (<https://doi.org/10.1029/2003JB002897>). This equation provides vertical velocity inside a wedge as a function of a predefined erosion rate, accretion rate, and slope of the base of the wedge. This is quite different from our approach in that Batt et al. (2001) use erosion rate at the surface as an input, whereas our equation predicts erosion rates. The Batt et al. (2001) equation therefore cannot used

to predict exhumation rates or thermochronometer ages without prior knowledge of the erosion rates. And of course erosion rates are themselves usually calculated using thermochronometer data. For this reason we cannot include a quantitative comparison between our model and the Batt et al. (2001) approach in the manuscript. However, we did add a brief discussion of this model to the introduction section of the revised version of our manuscript.

- I am not very happy how the authors simplify the thermal model, using a constant geothermal gradient (lateral and temporally constant), especially because they apply their model to the Himalayas, which is characterised by significant amount of horizontal and vertical rock motion. This does strongly perturb the thermal field, both laterally and also temporally. Although the authors show that they can fit the general age trend, that does not mean the model is correct.

Reply: We agree that the thermal model that we used is highly simplified. We have changed the thermal model and have updated the model code to include a numerical solution of the steady-state heat advection & conduction equation. The code uses the calculated velocity field as an input (eqs. 8, 9 in the manuscript), along with published thermal parameters and boundary conditions by Coutand et al. (2014) and McQuarrie and Ehlers (2015). The calculated steady-state thermal field is then used in combination with particle tracks calculated using our new equation to calculate the thermal history of particles. The new thermal model, in combination with the new thermochronology model / equations results in a similar fit to the AFT and ZFT data. However, the new thermochronology model results in higher resetting temperatures for the MAR thermochronometer. The steady-state model cannot fit both datasets well at the same time. Potential reasons for this are discussed in the results section of the revised manuscript.

- Similar to the thermal model, the calculation of thermochronological ages is oversimplified and should not be used for the purpose stated. There are numerous of recent studies demonstrating the strong impact of different cooling rates and radiation damage on thermochronological ages. In this regard, exhumation of the Himalayas does include movement above ramps and flats that should result in cooling rates to vary temporally and different amount of radiation damage to accumulate. I do not see any reason why not using available annealing and diffusion models and include them in this modelling approach (even if the thermal field is set constant, which I do not like).

Reply: We agree that use of a fixed closure temperatures was a bit overly simplistic and have upgraded the model code to calculate cooling ages using an approach by Fox et al. (2014, <https://doi.org/10.5194/esurf-2-47-2014>), which is based on the Dodson (1973, <https://doi.org/10.1007/BF00373790>) equation. We acknowledge that this approach, at least for the AFT method is still a simplification. However, the rapid cooling experienced by the samples makes linear cooling models such as used by Fox et al. (2014) relatively accurate in this case because the samples spend relatively little time in the partial annealing zone where differences in annealing rates are important. In addition, we would like to reserve the use of more sophisticated thermochronology models for follow up manuscript because this would require significantly more work on the model code and much more space in the manuscript for additional discussions.

- The oversimplified thermal model and calculation of thermochronological ages does not

allow to use this model to be applied to a real dataset. Instead it might be used to study the general trend in thermochronological ages in active orogens, and study the different age trends related to the importance of compression, frontal and basal accretion. If this model, however, should be applied to real datasets, it is mandatory to include a more exact treatment of the thermal field and state-of-the-art calculation of thermochronological ages.

Reply: This point was addressed by using a more realistic thermal model and thermochronometer model.

- The authors state that they fit the thermochron data along the Kuru Chu cross section better than McQuarrie and Ehlers (2015). The general trend is fitted by the authors, but it seems that McQuarrie and Ehlers (2015) do a better job in fitting the details of data between 0 to 80 km better. I would also like to ask the authors to contact McQuarrie and Ehlers to get the raw data instead of taken the values from a figure. Anyway getting a better fit with a simple model that does neglect part of the complexity of the geological setting, thermal structure and analytical method does not necessarily mean their model is correct. Indeed it is usually easier to fit data with a simple model, since it can be adjusted easily to optimize fitting. This is often time consuming if all available geological constraints and sophisticated thermal-kinematic models are used. However, even complex models do become computational more efficient and can be adjusted with efficient search algorithms and running models in parallel. Please add a little paragraph and explain the applicability of the different model setups and limitation associated with it.

Reply: We did request the original data from McQuarrie. We did not receive a reply previously, but have repeated the request and McQuarrie has now kindly supplied the data.

Following Ockham's razor we feel that given two model codes the simplest code that fits the data should be preferred. We are not saying that it would not be possible to fit the data better with a more complicated model. However, at present our simple model performs better in this particular cross-section than the published models. This may or may not be the case in other parts of the Himalayas or other mountain belts, which we feel is something that would be worth exploring in the future.

- The authors state that because they can reproduce the general trend in ages in the studied profile with a steady-state assumption, the Himalaya might be in steady-state. Showing that one model is fitting the data does not mean that others model do not fit the data as well or even better. Since the presented model is only working in steady-state the authors cannot prove that steady-state is the best model to fit the data. What their model however can be used for, is to study the relative importance of e.g. frontal and basal accretion, detachment transport and compression, which the authors correctly discussed.

Reply: We agree that we cannot test transient models with our approach. However, previous attempts with transient models do not show a better fit than our steady-state approach. We would therefore like to still state that there is a strong possibility that this part of the Himalayas is in steady-state. We did change the wording of this claim in the manuscript to state that "this cross-section in the Himalayas may be in steady-state".

In summary, I would suggest the authors to change the focus of their manuscript and state the drawbacks and benefits of such an approach. I do see the real application of such an approach in testing large-scale boundary conditions that can afterwards be used in more sophisticated model setups. I do not see that this approach can 'compete' or 'replace' approaches like that of McQuarrie and Ehlers (2015). I hope you find my comments and suggestions helpful and find more details in the technical corrections.

Reply: We do not intend to state that this approach should replace numerical model approaches, and hope we have made this more clear in the revised version of the manuscript. We felt that a simple model like the one we presented was missing from the literature and could be a useful additional tool to study the dynamics of mountain belts. We did add a paragraph to the discussion section that discusses the benefits and drawbacks of the approach.

Technical corrections:

Line 3: State what you mean with exhumation, amount or rate?

Reply: We mean exhumation rate here, now explicitly stated in the manuscript.

Line 20: Change to 'fault blocks'.

Reply: Ok

Line 23-24: That is not correct a similar approach for accretionary wedges (frontal and basal accretion) has been published already in 2001 by Batt et al. Please cite their work and discuss the similarities and differences with your model.

Reply: We did now add a brief discussion of this study, see reply to point 1.

Line 91: I guess it should be $v_{xc} = \dots$

Reply: correct, thanks for noticing this.

Line 99: State that you also use Eq. 7 and use the normalized compression velocity.

Reply: Ok

Line 101 and 103: It is not clear why you are defining the horizontal and vertical components in Eq. 1-4, but not all equations are used in Eq. 8 and 9.

Reply: This is because v_{yc} was replaced with the expression for v_{yc} from eq. 4. We have rewritten the equation to correct this.

Line 106-107: Should be '...internal deformation and basal and frontal accretion...', this is what the figure captions says. In this case, shouldn't the velocity vectors decrease towards the interior of the model since the accretion is only occurring along the tip of the wedge and vectors should show divergence!? The integral of the vectors along x should be similar along the wedge, or not?

Reply: The horizontal and vertical compression vectors decrease towards the tip of the wedge and are directed to the left (Fig. 4a). The accretion velocity is constant along the wedge and directed to the right (fig 4b). The combination of the two shown in 4c results in horizontal velocities that cancel out at a distance of 75 km from the tip of the wedge and a total velocity that first decreases with distance to the tip and then increases again as it passes the point where the two components cancel out. Velocity integrated over over x varies in our approach.

Line 137-139: It is not clear why you do provide the 'simplified solution', please explain and if there are no good reasons, just do not show. Can you provide the difference between the analytical and the numerical solutions as figures, that would help the reader to check what you have wrote here (instead of showing the difference between the two provided analytical solutions). Maybe you only show the difference between the simplified analytical (if you want to keep showing it) against numerical since the other one are nearly identical.

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

Line 165: A uniform geothermal gradient is really not what can be used in such setting. Depending on the model setup, and especially the horizontal and vertical rates the thermal field will be strongly perturbed. For instance do have a look on the cited manuscript from McQuarrie and Ehlers (2015) Fig. 1a).

Reply: We agree and have revised the thermal model. See previous replies.

Line 168: This approach is also much too simplistic especially since you do have significant horizontal particle motion and cooling rates of samples might vary significantly throughout their exhumation. This and the knowledge of the importance of the amount of radiation damage on He diffusion kinetics in apatite and zircon (e.g. Flowers et al. 2009; Guenther et al. 2013) has to be taken into account for the transformation of particle trajectories, cooling histories and final thermochron ages.

Reply: We agree and have revised the thermochronometer models, see previous replies.

Line 208-209: Please provide details and justification how you calculate the misfit, what is the MAE? Your model indeed fits the general trend of the data, however, the details are better fitted by McQuarrie and Ehlers (2015), with the exception of data >100 km away from the frontal tip. You may want to mention this.

Reply: We used two metrics to calculate the model fit, R2 and MAE. R2 is the coefficient of

determination, which we now clarified better in the main text and the caption. MAE is the mean absolute error, which is defined in line 179, and is now also mentioned in the caption. We have added a more extensive discussion of the model fit of our and McQuarrie and Ehlers (2015) model to the revised manuscript.

Line 209-210: Please ask the authors to provide the data, fortunately we do not need to digitize data from figures anymore!!!

Reply: We did request the data and had not received a reply before submitted the previous version of the manuscript. However, we have sought contact again and have now received the data.

Line 212: Please not that a model that fits data better is not automatically correct. The model of McQuarrie and Ehlers (2015) is based on numerous independent geological information that has been incorporated in a much more realistic model, but complex model. It is always easier to fit a simple model to data compared to complex model, but does that mean the simple model is more realistic, I would say not! There is a trade-off between complexity and fit to the data, your model and the one from McQuarrie and Ehlers (2015) are endmembers in this relation. From both model setups we can learn and they do have their eligibility. Please add a bit of this in your discussion and not just say your model is better...

Reply: We do not claim that our model is more correct. We did not use the term correct in the manuscript, and feel that correct is not a good term to describe models. If one follows Ockhams razor then a simple model that fits the data better should be preferred. We do agree that the model that we use may be overly simplistic in assuming uniform compression and transport. However, until a model comes along with a better fit to the data, we feel that the simple model should be preferred. We do think it is important to present an alternative model that provides a better fit to the thermochronological data, and let the reader judge, which is preferable.

Line 225: That orogens are in steady-state has been described already by others (e.g. Willett and Brandon 2003, Bernet et al. 2001) and that was highly discussed and even often this has been disproved by additional data and more in-depth interpretation (e.g. Michel et al. 2019). Since the thermal field of the crust is slow in responding to changes in the boundary conditions the resulting thermochronological data are often 'smooth' and the real/complex exhumation history is difficult to constrain. Fitting a steady-state pattern through data is therefore often easier compared to finding the real/complex exhumation history.

Reply: In our humble opinion, whether or not the real exhumation would be more complex should be judged by the data. And for the time being, if more complex models in this particular cross-section result in poorer fit to data then these models should not be preferred. We do however agree with the reviewer that thermochronological data alone may not be able to resolve all complexities within the system. In the case of the Himalayas steady-state has also been suggested based on a compilation of cosmogenic nuclides and sediment yield data. Our analysis corroborates these findings.

Line 239: What kind of steady-state do you mean, flux, exhumation, topography?

Reply: We mean steady-state exhumation, and have changed the text accordingly.

Figures:

Fig. 1: Can you draw a few more particle path and continue them to the surface. From the figure it is also not clear what is above the wedge, water, air? Also add 'erosion' below the precipitation on the surface.

Reply: Thanks for this suggestion, we have modified the figure accordingly

Fig. 3: In caption change to 'Panel shows...'.

Reply: Corrected, thanks for spotting this.

Fig. 4: The heading of the panels are wrong, please correct.

Reply: We have corrected this.

Fig. 5: Use '5 Ma' instead of '-5 Ma'. Change panel d to Error between numerical and analytical solution.

Reply: Ok

Fig. 6: Change panel d to Error between numerical and analytical solution.

Reply: See previous comment.

Fig. 7: Is the scale correct, I thought you speak somewhere from 200 km profile length. Looks shorter?

Reply: That is correct. The modelled cross-section is 200 km to avoid boundary effects on the results. However, the balanced cross-section shown here only covers the first part of the cross-section. We have added an explanation of this to the revised manuscript.

Fig. 8: What does MAE mean?

Reply: We have added an explanation of the mean absolute error to the caption.