

Solid Earth Discuss., author comment AC2
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Reply to RC2

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-AC2>, 2021

Note: Our replies to the comments by Reviewer 2 are shown below each comment in *italics*

Dear authors,

This work presents a new analytical solution for the steady-state exhumation of an orogenic wedge. The authors used the prediction of the time-depth path and a simple thermal model to predict low-temperature thermochronologic ages that they compare with a cross section in eastern Himalaya. This comparison shows good fit between observations and predictions. This solution does not capture the effects of individual faults and folds, but the authors claim from this comparison that 1) in this example, a simple model is sufficient to explain the large-scale observations, and 2) that here, the Himalaya may be in steady-state.

General comments

In general, this is well written and easy to read. I like the idea to predict low-temperature thermochronologic ages from a 2D model of a critical wedge evolution, but I have important remarks that cannot be eluded:

- You claim that this is the first attempt for such a study. I disagree, at least Batt et al., 2001 did a similar 2D critical wedge model to predict low-temperature ages. This model is not presented in the present paper from Luijendijk et al., they do not explain in what their model is different, and also, there is no comparison of these two similar models. You should present the Batt et al., 2001 model, and discuss it in terms of both construction and results.

Reply: Thanks for pointing us to this paper. We have added it to the manuscript, see reply to the same comment by Reviewer 1.

- My other major concern is about the thermal model used in the present model. This is not clear to me, if the thermal model resolve the in 2D the heat equation with vertical and lateral advection. This is important because in the model, particles are advected, and thus heat also! For a similar particle path model, taking in account or not heat advection will lead to time-temperature path that can be very different, and that can impact the low-T age prediction! So, you need to precise the thermal model (and thus give the values of the complete set of heat parameters). And if the thermal model does not take in account the heat advection, I would push to update the thermal model so that it takes it in account. Or show that there is no consequence in your time temperature path, and thus that it does not impact your age prediction.

Reply: Agreed, we have improved the thermal model. See reply to the same comment by Reviewer 1.

- In the same type of comments, I am not very happy of the cooling models used to compute thermochronological ages. Here, you do not use published cooling models, but only a closure temperature. This is over simplistic, and it finally asks if your predictions can really be compared to observations! Maybe a way to prove that this works, this is to show that the cooling paths you compute (with a correct thermal model!) is similar to the cooling paths published in previous papers (i.e., Long et al., 2012?), or better, implement the published routines to compute correct thermochronologic ages from cooling histories.

Reply: Agreed. We have improved the thermochronology model. See reply to the same comment by Reviewer 1.

- In the method section, I do not understand why you present a simplified solution that does not gives very accurate results. First, this is noise in the paper, and second, it decrease the confidence we could have in your other strong simplifications (temperature model, cooling model,...). I would remove this simplified solution, or explain why this is important.

Reply: We have removed the simplified solution from the manuscript

- In the validation section, I do not understand how you set up your numerical model, i.e., what is the difference between the numerical model and the full analytical solution. You probably need to better explain (with an additional sketch?) your numerical model with the parameters used.

Reply: we have tried to better describe the numerical model in the revised manuscript. All it is is particle tracking based on velocity field calculated by eq. 8 & 9. We feel that devoting a sketch to this would probably be overkill.

- And finally, in your application to a mountain belt, you need to explain with better arguments why you choose this section, because for instance the Central Nepal section also matches the arguments you present here. Also, the comparison between the restored cross-section published and your prism model differs importantly in some places. So you need to discuss the impact on predicted ages of your approximations in

the geometry of the prism.

Reply: We did or did not add a 2nd xsection. It is true that other cross sections would also be suitable to test our model – even outside the Himalayas. We chose this section, as it has a high amount of high-quality data, and is relatively well-studied. Our point of this manuscript is not to criticize the existing model, but to present an alternative interpretation as a second endmember, if you wish. Generally it has been shown that cross sections are often only poorly constrained at depth. (In)famous examples exist e.g. from the Alpine foreland, or the Zagros mountains, where even based on high-resolution data, different geometric solutions are possible. This is also true for the chosen cross section. The internal geometry of the wedge is not that well constrained. See for instance Coutand et al. (2014) who explore various different models for this that are all consistent with the relatively sparse geophysical data.

So, because of the strong simplifications you made, I am not sure that you really can compare predictions with a real dataset, and, more, that you can use this comparison to extract strong conclusions on how to interpret low-temperature data and if the Himalayas are in steady state or not, specially without moderating your sentences.

Reply: The revised thermal model and thermochronology model provide more realistic thermochronology ages that can be compared better with the data. We did moderate the sentences concerning steady state in the revised version of the manuscript.

Specific comments

L5: precise where (approx. longitude for instance) the cross section is located in the Himalayas

Reply: this is shown in the inset map in Fig. 7b

L6-7: change “at a large scale deformation” to “deformation at a large scale” or “large scale deformation”?

Reply: Ok

Somewhere in the abstract, you need to specify that your model is only in 2D and that it does not take in account any relief (that is important for comparison with real dataset).

Reply: We have added a clarification to the abstract that the model is 2D. We have also added a brief discussion that the model does not take into account relief, which may affect the comparison with thermochronometers.

L11 & L13 & L14: add “e.g.,” before the reference(s)

Reply: Ok.

L24: and Batt et al., 2001 model? You need to say in what your approach differs from Batt et al., 2001!

Reply: See reply to the same comment by Reviewer 1.

L25-26: Expend the idea present in this sentence as the comparison is not straight forward.

Reply: We explain that this reference solved the advective transport equation. Which is the same equation that one would need to solve for rock particle transport in an orogenic wedge. Although the boundary conditions are admittedly quite different.

L91: vxc ?

Reply: Ok. See reply to the same comment by Reviewer 1.

L94: add a ref to the appendix where you explain how you defined these variables

Reply: The variables are defined in the two following equations. Which we feel would be better here than in the appendix.

L106-107: This is not what there is in the caption of Fig. 4. Correct it.

Reply: See reply to the same comment by Reviewer 1.

L137-138: Why showing the simplified solution if the full solution is a lot better? Does this solution give advantages when computing? If they are reasons to show this simplified solution, explain them, or remove the simplified solution from the paper.

Reply: See reply to the same comment by Reviewer 1.

L139: "The error increases with depth". This part is a bit confusing: on the figs. 5 and 6, the colors representing the errors are inversed, they do not show the same between figs. 5 and 6. Please, to limit the confusion, put the same color bar.

Reply: Agreed, we have used the same colorbar for the 2 figures.

Also, this sentence is true for the fig. 6d, but not for fig.5d where I see first a decrease of the error with depth, and then a rapid increase.

Reply: agreed. However, we have removed the simplified solution, so this panel is no longer in the revised manuscript.

L140-142: So, again, why showing the simplified solution if it does not permit to match well the full solution?

See previous reply

L148-151: here, you give your arguments (good thermochronologic data coverage and high convergence rates) to justify that you choose this section. But if you take the Kathmandu/Trisuli or Sutlej region, it matches the same argument, so why didn't you took these sections also? You need to give more arguments on the choice of your section.

Reply: The main focus of the manuscript is the new analytical solution. We intended to keep the comparison to field data limited to one example. Testing it with other dataset is the scope of follow up work. We also do not wish to claim that this would be the best dataset to compare it to. We simply took a high quality published study with a wide range of thermochronometers and numerical cross-section balancing in the Himalayas that we were aware of.

L159: m m-1, not m m1

Reply: Ok. Thanks for spotting this error.

L160: m m-1 not m m1

Reply: Ok. Thanks for spotting this error.

L160: 200 km? from the fig 7, it looks closer to 100 km-long than 200 km-long. So what is wrong, the text or the legend of Fig. 7?

Reply: See reply to similar comment by reviewer 1.

L160: add ref to fig. 7

Reply: Ok

L157-160: You may describe and quantify the differences of the complex geometry of the wedge build from geological and geophysical data, and your triangular simplified wedge geometry. You then need to explain what are the consequences of these differences to your model results, i.e. where your results will be accurate, and where they will not.

Reply: We would like to refrain from a detailed discussion of geological data and resulting expectations of where our model may be accurate or not, and would like to focus purely on a comparison of the model results with the data.

I ask this question because the Long et al., 2012 cross-section show an important ramp of the MHT (representing the base of the wedge), also, your model does not take in account the important frontal ramp. Though, Van der Beek et al., 2006, Robert et al. 2009, 2011 have shown that this geometry of the base of the wedge has a huge impact on the thermochronological record. So, such a simplification risks to make inaccurate predictions. You really need to reinforce this section.

Reply: We thank the reviewer for pointing us to the studies by Van der Beek et al. and Robert et al. It is true that in multiple occasions the presence or absence of a ramp impacts exhumation rates. An exciting result of our study is that the thermochronological data can be fitted without such a ramp, and at present the fit is better than in the existing model including complex geometry and localized strain. We would like to propose our model as a viable alternative.

L162-164: I think that the set of thermochronometers from the Kathmandu/Trisuli section is larger (See Herman et al., 2010 for instance). So why did you choose the Bhutan section and not the central Nepal section?

Reply: There's certainly a lot of data in the Herman et al. (2010) paper. However, we would like to refrain from testing additional cross-sections in this manuscript. The choice for the Bhutan cross-section was based on the availability of several studies and balanced cross-sections, which we felt made this an attractive target to compare with our more simple approach.

L165: 15°C km⁻¹ (also, generally, remove the space between numbers and the °C sign)

Reply: This was corrected in the revised version of the manuscript.

L165-167: I do not really understand the thermal model.

- Do you update this thermal model through time while you are advecting your particles? Do you take in account lateral thermal advection? If yes, you need to describe this model precisely in the method section. If not, I do not understand how you will be able to compute accurate thermochronological ages as in your wedge model, you have a lateral component that is important.
- Do you have heat production (i.e., radioactivity) in your model?
- What are the thermal characteristics (i.e. thermal diffusivity) of the rocks you are advecting in the model? This is crucial for the thermal model, you need to give them.

Reply: we have completely revised the thermal model used in the manuscript. See also the replies to reviewer 1.

L167-172: this is a strange choice. If you want to predict thermochronological ages, this is better to use existing cooling models. This is not so hard because you can find most of the routines in the literature or on the Web. This approach is oversimplified!

Reply: We have completely revised the thermochronology model used in the manuscript. See also the replies to reviewer 1.

L178-180: I do not understand what you do here. What do you input in the downhill simplex algorithm? You need to explicitly explain what you are doing here in details.

Also, what is the MAE of the calculated thermochronological ages?

Reply: This is a widely used optimization / model calibration algorithm that simply minimizes the error between the model and the data by adjusting the model parameters. As this approach has been used so extensively in the past, we would like to refer to the cited references for more information.

The MAE is the mean absolute error as defined earlier in the manuscript. See also reply to the same question by reviewer 1.

L181-182: where are the results/comparison of these models?

Reply: The model fit for these runs is shown in fig. 9

L186: With such an ultra-simplified thermal model that does not take in account lateral heat advection (does it?), and an ultra-simplified cooling calculation, I am not sure this is relevant to compare your predictions to the data. Before to do that, you need to refine your thermal model and cooling models. Ask any quantitative thermochronologist about what is often a critical point when submitting papers for publication: there is almost always a strong debate around the thermal model/parameters used to interpret thermochronologic data/model. This is really a critical point.

Reply: The thermochronology model was revised completely, see previous replies.

L188: the exploration of the parameter space is very rough. In the introduction, you explained that you want to propose a model that does not need a lot of computing time and resources. So if this is the case (is it the case? You do not mention it until now), it would be easy and rapid to refine your parameter search with a smaller stepping.

Reply: The exhumation model is obviously very fast since it only involves calculating two equations, which is done in a few milliseconds. However, the bottleneck in the new implementation is the time required for the numerical heatflow model, which adds significant computational expense. Nonetheless we have revised the parameter space search and have added more steps.

L208: I would not say a better fit: you fit well the general trend, but not the details, and

McQuarrie and Ehlers better explain the different local ages variations (because their model is based on numerous geological observation, and is thermally more complex), except for the farthest samples. You need to moderate your sentence, or better explain the comparison.

Reply: The better fit refers to the better model fit statistic of the analytical solution. We would like to refrain from a more qualitative discussion of local errors or not and base the assessment of model performance on statistics. In the new version of the manuscript we state more explicitly that our solution is an alternative approach.

L209-201: Ask the authors the source of their graph.

Reply: We have repeated our request and have now received the data and incorporated this in the manuscript.

L212-2013: You should change the sentence to "This suggest that, for this cross-section, at large scale, deformation of the wedge is represented by uniform deformation", or for something like that. This is because your fit does not reproduce the details of the data because 1) your thermal/cooling model is over-simplified, and 2) you do not take in account relief.

Reply: We agree that the model probably works best at large scale and have added the word at large scale to this sentence. However, in our opinion, in general the simplest model that fits the data the best should be the preferred model. Whether or not a model would come along in the future with more complexity that includes relief or the movement along single faults would fit the data better is speculative at this point. Note that the thermochronology model and thermal model have been improved for the revised manuscript.

L269, eq A5: There is a problem in this equation. A dx is missing (in place of the L?) in the left member.

Reply: True, that was a typo. Thanks for spotting this!

Figures

Figure 2:

The font is too small, increase it (it is hard to read the name of the vectors); in the caption, change the last sentence to "The symbols are listed and described in Table 1"

Reply: ok, we have implemented these suggestions

Figure 3 & 4:

Does the length of the arrows mean something? If yes, it maybe needs a scale-bar, or at

least an explanation in the caption

Reply: The length of the arrow scales with the velocity. The velocity is also visible in the colors, which is why we feel that adding a scale bar would be a bit double. However, we did add a statement to the caption that arrow size scales with velocity.

Figure 5 & 6;

See my specific comment about the colorbar for panels d). Why using a "-" in the isochrones? It will simplify the fig if you remove this "-"

Reply: ok

Figure 7:

- a) the writings inside the fig. are not readable. Please increase the size of the font.

On this fig., you may mark the MHT (which is the base of your wedge) with a thicker line, so the reader will better see what approximation you are doing in your model.

On a), you also may add the thermochronologic data (on a map-view also?) you use in the Himalayan case.

You also may write in the caption what is the red triangle...

Reply: Ok. We prefer to keep the thermochronology data in the model result figures to not overcrowd this figure. We did add the sample locations to this figure.

Figure 8:

Why the MAE is in a and ages in Ma? Cleaner if you give MAE in Ma.

Reply: Agreed, we have converted this to Ma.

Figure 10:

In caption, correct "calculated themrochronometer..." by "calculated theRmochronometer..."

Reply: ok