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Solid Earth
Discussions

Interactive comment on "Testing the effects of topography, geometry and kinematics on modeled thermochronometer cooling ages in the eastern Bhutan Himalaya" *by* Michelle Gilmore et al.

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Gilmore et al. present a sensitivity analysis for a recently developed modelling approach in which structural restoration is combined with forward thermal-kinematic modelling to predict thermochronometer ages in fold-thrust belts, and subsequently use these ages to constrain the timing and rate of thrust-(sheet) motion in such settings. This is a promising approach, which h is being developed by several research groups separately (e.g., Almendral et al., 2015; Erdös et al., 2014; McQuarrie and Ehlers, 2015; 2017). However it still faces challenges, in particular how to take into account the topographic evolution through time and how to handle the large degree of freedom

in the models. The present manuscript explores some of these challenges, in particular the effect of material properties (heat production rates), reconstructed geometry and kinematics, and the topographic history, which all influence the predicted thermal histories significantly but are very difficult to constrain. It is therefore a useful contribution to the still small but growing number of papers on this subject, and I would recommend publishing this in Solid Earth after moderate revisions.

I have two major comments and a number of smaller, more specific comments on this manuscript. The first major comment concerns the context of this study and what is exactly new in it. When I started reading this, this was not very clear for me. Long et al. (2012) presented the structural cross-section and thermochronology data used here, as well as similar data for the parallel more westerly Kuri Chu cross-section. McQuarrie and Ehlers (2015) modelled the data for the Kuri Chu cross-section in a similar manner to what is done here. What is new in this manuscript is the modelling of the (eastern) Trashigang cross-section. This is a valuable exercise in itself, and the comparison of the outcomes of the two modelling exercises in enlightening (see below), but I think it would be useful if the authors presented this context and the relationship of this study with previous work straight up in the introduction, so that readers are not left wondering what is new or different here with respect to previous work by the same group of authors.

My second comment concerns the inferred history of shortening rates; in particular the strong variability in these rates that the analysis suggests. I have been intrigued by this outcome since the initial paper by Long et al. (2012). I reviewed that paper at the time and already queried the authors about the robustness and implications of that finding but am still struggling to understand it. Starting from what we know (and progressing toward lesser constrained inferences): the modern convergence velocity between India and Tibet is $\sim$20 mm/y; the total India-Asia convergence rate is about twice that. If we accept the results of Molnar & Stock (2009), India-Asia convergence rates have decreased since 20 Ma; from 54-83 mm/y before 11 Ma to 34-44 mm/y after that, for points in the NW and NE corner of the Indian subcontinent respectively. That total India- Asia convergence rate should be distributed between far-field deformation in the Tibetan plateau and its northern borders, shortening in the Himalaya, and underthrusting of the Indian plate beneath Tibet. It is interesting, and reassuring, to note that most of the tested models predict shortening rates in the order of 5-6 mm/y in the last ∼10 Ma, which is consistent with estimated "overthrusting" rates in simpler thermo-kinematic models used to predict thermochronology ages (e.g. Brewer and Burbank, 2006; Whipp et al., 2009; Robert et al., 2009; 2011; Herman et al., 2010; Coutand et al., 2014, and others). Any increase in shortening rates up to the total India- Tibet convergence rate of ∼20 mm/y could potentially be explained by temporally variable partitioning between "overthrusting" and "underthrusting"; since these concepts are really defined by a particular frame of reference only (which is in my view controlled by the erosional efficiency in the Himalaya), that could be plausible and possibly linked to temporal variations in erosional efficiency. If one wants to invoke further increases up to the India-Asia convergence rate, that would only be possible by temporally transferring far-field deformation to the Himalaya, but it remains in the realm of possibilities. The inferred rates of ∼70 mm/y during building of the Upper Lesser Himalayan duplex are more problematic, because – if true – they would necessarily imply north-south extension in other parts of the Himalaya-Tibet system, for which there is very little evidence. The inferred reconstruction requires significant amounts of shortening to build this duplex (at least 150 km or ∼1/3 of the total shortening since 20 Ma according to Fig. 3) and I wonder whether a more conservative structural solution would not be possible to fit the surface observations for this duplex. In any case, the preferred models with variable shortening velocities pose significant questions, which should be addressed more directly. The reader is really left wondering how well resolved these shortening histories are, given the significant number of unconstrained parameters in the models. Some of the specific comments below refer to these unknowns.

Overall, the paper is fairly well written and illustrated. On a number of occasions, phrases don't run because a verb is missing or because of singular/plural confusions.

A certain number of typos also remain. All of these can be weeded out by some careful editing. The use of some internal "modelling jargon" like "Python topography", "Split KT" etc. does not add to the general understanding of the manuscript – the authors might want to find some more eloquent terms to describe these modelling settings.

Specific comments, tied to page and line number:

p. 1 l. 7-10: the first two phrases of the abstract do not really set up the problem in a very clear manner or "draw" the reader into the problem – you may want to consider rewriting these into something more clear and specific.

p. 2 l. 13-20: this first paragraph of the "Geologic background" section looks a bit lost on its own; it is not very informative (why is the onset of motion on the MCT important here?) and could easily be combined with the following "Tectonostratigraphy" section. The Daniel et al. (2003) and Tobgay et al. (2012) references are missing in the reference list.

p. 3 l. 20-21: how were the data exactly projected into the cross-section? This is a critical step, as the ages (in particular for the low-temperature systems) will be influenced by the local topography. See further comments below.

p. 3 l. 30: why do the ZHe ages require "rapid" cooling? This inference can only be drawn by comparing them to other thermochronometer data, or by assessing age-elevation profiles for instance.

p. 3 l. 32: three ZHe cooling ages north of the MCT are shown on the cross-section (but only two on the map?). Also, the cross-section of Fig. 2 gives the impression that the samples between ∼57-65 km are from the lower Greater Himalayan sequence, while the map shows they are from the upper. Maybe you should sketch in some of the geology above the topography to make this clearer. This also brings us back to the question above of how these data were projected into the cross section. What was their imposed elevation? Simply plotting them on the topography in the cross-section puts them on a much lower structural level than where they actually are!

p. 4 l. 5-9: why do you take this approach? It is easy enough to model the individual data using the combined Move/Pecube approach . . .

p. 4 l. 17-18: the question here is obviously: “how was the new topography obtained?” this is discussed further on – you may want to refer the readers to this later discussion here.

p. 4 l. 26-27: Note that a subsequent similar model by the same authors (Hammer et al., GRL 2013) comes up with much lower estimates for the elastic thickness in Bhutan (< 25 km) than in Nepal.

p. 5 l. 2: here you could reference some of the previous studies using the same approach.

p. 5 L. 27-31: a self-consistent approach would be to use a critical-taper topography in the models – it is not clear if the “Python topography” is based on such an approach, but the link between the imposed topography and a critically tapered wedge model could be outlined here.

p. 7 l. 6-17: see general comment on variable shortening rates above. More justification and discussion of these rates is needed.

p. 7 l. 16: it seems that this is the first time the Kuru Chu section is mentioned; it hasn’t been introduced previously (but should be).

p. 7 l. 19 (and numerous other occurrences): why do you call the reconstructions “flexural models”? This is surprising and confusing, as flexure is only one component of these models; the structural reconstruction is at the heart of them. You could call them “kinematic models” or something like that.

p. 7 l. 30: the INDEPTH lines were shot in the Yadong rift, which overlies the Yadong cross-structure – a probably important lateral ramp in the Main Himalayan decollement.

Is the 4° dip you cite here relevant for the decollement west or east of the Yadong structure? In any case, this would be valid for western Bhutan and not necessarily for eastern Bhutan. It is not obvious that comparing the decollement dips with data that are not from the same region is very informative, given the probable lateral segmentation of the MHT.

p. 8 l. 1-4: this is counter-intuitive. The flexural response should be driven by the topographic loading, not by the kinematic scenario. Therefore, if the different kinematic models lead to differences in flexural loading profiles, it must be because the (imposed) topographic response to the kinematics is different between these models.

p. 8 l. 15-19: why do you not simply use the present-day topography as the final topography in the model? This is a known entity, and at least that would help in comparing kinematic and thermal histories at the right structural and topographic levels for the data points.

p. 8 l. 22-24: this phrase is hard to read and also appears counter-intuitive. In the critical-wedge model, the surface topography (α) and decollement dip (β) are linked through the critical taper angle (which itself depends, among other things, on β). Therefore, it might be more self-consistent to try to find a surface topography angle that corresponds to the critical taper for each time step (and degree of topographic loading). This would be an iterative approach, but I’m sure it can be done. See comment on p. 5 l. 27-31 above.

p. 9 l. 11: you may have modified your version of Pecube, but in the “standard” model, heat production is constant with depth, so that “surface heat production” is a bit of a confusing term in this context.

p. 9 l. 15: this seems a fairly obvious result, since the kinematics of the models do not change, only the thermal field. The samples have the same “normalized” thermal histories; the temperatures are simply somewhat higher throughout for the models with higher heat production.

p. 9 l. 19: “ages” not “rocks”, I think.

p. 10 l. 3-5: a bit of a rambling phrase that is difficult to read/understand.

p. 10 l. 24: “later” not “earlier” I think?

p. 10 l. 32-33: there are many free parameters in these models: not only an infinite number of shortening-rate histories, but also significant degrees of freedom in the imposed structure and the topographic evolution. I fully understand and appreciate the difficulties in exploring this complex parameter space, but how robust are the inferred rates really? This is not obvious, and given the important implications of the shortening-rate history, this should be discussed. An alternative approach would be to not allow shortening rates that are greater than the plate-scale convergence rates at any time (i.e. use the plate-convergence rates as a constraint) and try to find models that can explain the data using this constraint.

p. 11 l. 10-11: why is this your expectation? The erosional history would depend on the topographic history through time, rather than the final topography. In the no-topography scenario, if I understand well, there is no topographic change through time. If in the other topographic scenarios topography diminishes locally in the final timesteps, this will predict younger ages.

p. 11 l. 14-15: a list of 6 adjectives (“Python topography model fully reset Mar ages”) followed by another of 4 . . . Maybe rewrite?

p. 11 l. 20-23: this is an important point but it also seems fairly obvious. It clearly points to the need of a self-consistent treatment of topographic evolution. The best way forward may be to combine these models with simple surface-process models to erode the topography through time.

p. 12 l. 2: I think you are discussing MAr ages specifically here? May be useful to state this.

p. 12 l. 30: “older ages” seems more correct than “earlier ages” in this context.

p. 12 l. 31: you have been calling this the MHT throughout the manuscript. Better stick to this acronym so as not to confuse the readers.

p. 13 l. 1-2: another somewhat rambling phrase . . .

p. 13 l. 5: this ramp is rather located at ∼90 km in the present-day geometry (Fig. 2)?

p. 13 l. 12-15: is the cross-section of Fig. 9 still balanced? There is all of a sudden 35 km more Baxa group in this cross-section, while the rest of it has not been modified. Could these additional 35 km be found by reducing shortening in the upper LHS duplex? In that manner you might also be able to reduce the problematic shortening rates necessary to produce this (and the associated ZHe ages).

p. 13 l. 25-29: this is problematic. First of all, you change two major inputs to the model (structural geometry and heat production) at the same time here, while previously you have carefully only changed one parameter at a time. Second, you introduce spatially variable heat production here, which you did not do previously and which could have led to better fits in the previous models. This is a large change in the thermal structure and it should be justified. Although I am sympathetic to the fact that heat production could be significantly higher in the GHS than in the LHS, to really model this properly you should ascribe heat-production values to the different units, and advect these with the units.

p. 13 l. 31-32: OK, but we are left wondering how much of this improved fit can be ascribed to the new structure and how much to the increased heat production. p. 14 l. 5-6: following up on the previous comment; can the data really tell the difference between the improved structural geometry and the increased heat production? There is very little data in the “bump” region. You use a simple visual comparison of predicted and observed ages; it would be useful to provide a more objective and quantitative comparison to back up inferences such as this.

p. 16 l. 10-12: this is introducing yet another unconstrained parameter. I am not sure it is the best strategy to further complexify the models to improve the fit; this seems like a bit of a “flight forward”. A more complete sensitivity and resolution analysis might be a more productive way forward.

p. 17 l. 9-10: “the amount of exhumation in this model is just at the amount necessary to reset AFT ages” is strange and apparently incorrect. The ages record cooling through the closure temperature at a certain time in the past. The thermal structure is going to affect that time, but the total amount of exhumation is much larger than the AFT closure depth it would seem.

p. 18 l. 10-15: A bunch of hard-to-read phrases that are in need of a few commas. Also, “after 13 Ma” would be better than “longer than” and replace the colloquial “till” by “until”.

p. 18 l. 15-20: another potential issue that is not discussed concerns the diffusion kinetics of He in zircon. Recent work has shown that the effective closure temperature of the ZHe system can vary from as low as $\sim$120 °C to as high as $\sim$240 °C as a complex function of the degree of α-damage (e.g. Guenthner et al., 2013). If you have underestimated the ZHe closure temperature (I suppose you are using the “standard” ZHe diffusion parameters built into Pecube) you could significantly underestimate the duration of shortening on the upper LH duplex, and thereby overestimate the shortening rates.

p. 18 l. 25-28: the first part of this argument is somewhat circular, since the McQuarrie and Ehlers (2015) scenario was input in the models here, without extensively testing all other potential scenarios. So the fact that the model predicts these variations in rates should not come as a surprise. In contrast, the dissimilar timing between the two sections that are only $\sim$25 km apart should be worrying. How can the same structure be active at time intervals that are several million years different between two adjacent locations? Again, the reader is left wondering how much of this difference could be due to variable diffusion kinetics?

p. 19 l. 2: given the numerous unexplored degrees of freedom in the models, it appears risky to assess the validity of the data based on the modelling outcomes.

p. 20 l. 1: not sure what is meant with this phrase; what is “the spatial nature of thermochronometry”?

Figures

Fig. 1: the inset geological map of Bhutan (panel B) is very small and not very readable. You should either increase its size or decrease the amount of detail on it. Also, in the legend of the main panel (C), the Chekha Formation should be above the Greater Himalaya to keep all units in their structural order. Finally, it would help the reader if the colours used for the different thermochronometers were consistent between this figure and the following.

Figs. 5-10: much more data appears to be plotted in these figures than in Figs. 1 and 2. What do the lighter-coloured data points refer to? For clarity it would be better to take them out. In Fig. 7, why does the “template topography” model not predict AFT ages everywhere?

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