Dear Reviewer,

We would like to express our gratitude for the thorough review of our work. We received many insightful comments that have helped us to significantly improve the manuscript. We revised the model input parameters and performed a sensitivity analysis to a selection of model input parameters. The temperature predictions have slightly changed in the revised models, together with the estimated amount of lithosphere extension, due to the revised input parameters we applied. Please find our detailed point-by-point responses to the comments below.

Kind regards,

Eszter Békési and Co-authors

REVIEWER #1

The manuscript by Bekesi et al. entitled 'Modelling transient thermal processes in the lithosphere: application to the NW Pannonian basin' presents a simplified modeling study on the thermal evolution of the NW part of the extensional Pannonian basin considering distinct crustal and mantle thinning factors and sedimentation. The calculated new thermal field is then used to present a 2D yield stress section of the lithosphere. Finally, the manuscript contains a brief discussion on mantle xenoliths. Given the large number of major issues of the manuscript, I suggest substantial revision before considering it for publication.

The title does not reflect the content of the manuscript. Reconstructing the thermal evolution of the lithosphere and particularly the deep lithospheric mantle is challenging, indeed, because of the large number of transient effects, i.e. partial melting, melt emplacement, phase changes, shear heating, non-uniform upper crustal, lower crustal and mantle thinning, paleo-surface temperature variations, basin inversion and related deformation, water circulation, etc. This manuscript uses the stretching factors approach of Royden and Keen (1980) to somehow consider crustal and mantle thinning in a simplified way, but none of the other transient effects are taken into account.

We thank the reviewer for highlighting also the transient processes not considered in the modelling technique. We revised the title to reflect the methodology more clearly. Still, the methodology is capable of reconstructing the most important transient effect that accompany extensional basin formation through the crustal and subcrustal stretching factors and sedimentation, as well as the detailed present-day crustal geometry. We extended the manuscript by commenting on the transient processes not taken into account in the modelling and discussing their potential effects. For more details, we refer to the revised ms.

The abstract and the manuscript claims that one of the main goals is to better quantify the thermal field in the entire lithosphere. There are too problems with this: (1) the model does not use any observational constraints from the deep basins, crust or lithosphere, and likely it is not sensitive to temperature variations at great depths; therefore, the goal cannot be reached with this method. (2) While the manuscript presents one possible model result, a sensitivity analysis, *assessing the role of different initial and boundary conditions and input parameters are missing, therefore, it is not an easy task to see how robust or reliable is the model. No model limitation section is included, despite the large number of assumptions the authors made.*

We agree that the limitations of the model have to be highlighted and the fact that the model is valid for a specific case of input parameters should be discussed. We revised the ms. with the detailed description and validation of input parameters we used. Additionally, we performed a sensitivity analysis to test the effect of variations in input parameters. We also included a model limitation description. About deep crustal and lithospheric constraints; the model considers a detailed present day crustal geometry, including the crustal thickness map based on seismological observations (Kalmár et al., 2021) and is compared with the present-day lithosphere thickness (Kalmár et al., 2023). For more details, we refer to the revised ms.

The used model parameters: Many parameters are not justified and seem to be far from reality. The source of other input data is not clarified and thus cannot be checked. A model needs to be reproduceable by the community and you need to make available the most important input data. 1. Initial crustal thickness: the authors assume a constant 35 km thickness. What is the source of this parameter? The study area includes metamorphic core complexes, their formation requires a thick and hot crust, which infers that your chosen initial values are lower than it should be. Previous reconstructions (e.g. van Hinsbergen et al. 2020) reported a much larger amounts of extension and a thicker initial crust. Geochemical studies based on xenoliths inferred a much larger initial crustal thickness (Torok et al.). Finally, the basement units of the region derived from the Alps, likely having a much thicker crust than 35 km in the Early Miocene. The initial lithospheric thickness of 120km: what is the constraint on this and how much role does it have? Lithologies: what is the source of this? For instance, the sand to shale ratio is proposed to be 1:9 for the 'Lower Pannonian' (Upper Miocene). How is this constrained? After a brief google search, well logs published by Stano et al. 2016 shows a sand to shale ratio of at least 50%. This means that your applied thermal conductivities are wrong, and this is a major issue. The timing of extension: in the model a uniform timing for rifting is assumed between 18-10. Most structures are inferred to be active only until the Middle Miocene (e.g. Majcin et al. 2015), a few small-offset normal faults would not have influenced lithospheric thinning.

We revised the ms. to better describe the input data and parameters we used. First, we revised the initial crustal and lithospheric thickness to 40 km and 135 km, respectively, to better represent the overthickened pre-extension lithosphere of the region. We also tested the effect of a range of input values as described in the previous comment.

We corrected the sand to shale ratio of lower pannonian sediments to 30:70. The ratio of sand is indeed even higher in some wells shown in Sztanó et al., 2016, but the thickness of Lower Pannonian sediments dominated by clay is relatively large in the Danube basin and Zala basin compared to the sandier turbidites (Szolnok fm.). We finally chose the Bősárkány-1 well to define a realistic ratio of 30:70, which we considered an acceptable average for the whole study area. For more details and references, please see the revised ms and further replies to the specific comments on thermal conductivities of reviewer #2.

The crustal thinning factor is simply calculated from the present-day crustal thickness, basement depth and initial crustal thickness (eq. 3.). For the initial crustal thickness, we assumed 35 km in the previous model, which was updated to a more realistic value of 40 km, but an initial thickness of 45 km was also included in the parameter test. The higher initial crustal thickness results in higher thinning factors (~ 1.3) also in the Rechnitz core complex area. The initial crustal thickness of 40 km was chosen as an input value that is realistic for the majority of the study area, however, it is important to note that the crustal thickness was possibly even larger in the western periphery of the study area. Higher initial crustal thickness in e.g. the Rechnitz core complex area would have resulted in even larger crustal thinning factors. We included this discussion in the revised ms.

We used a uniform timing for rifting for simplicity (to avoid inverting for subcrustal stretching factors for different times by introducing further parameters in the inversion complicating the models), that covers the main rifting phase for all parts of the study area. We agree that rifting was no longer intense after the Middle Miocene in parts of the study area, although active rifting e.g. in the Transdanubian Range is younger (~15-8 Ma, Fodor et al., 2021). We tried to choose a rifting period that covers the period of the intense rifting phase of all parts of the study area as described in the revised ms. We also comment on this limitation and potential effects of uniform timing of rifting on the resulting stretching factors in the ms.

The model result: this is already a mixture of discussion and describing some results. How is it possible that nearly 0 crustal thinning is calculated for the Rechnitz core complex area, that must have undergone substantial crustal thinning? This is a sign of the wrong model parameters. In Figure 6, it is not possible to read the values of the mismatch between the well and model data, but it still seems to be a significant error. About sediment blanketing: in the results of the shallow temperature field chapter you write: "Positive anomalies are the reflection of sediment blanketing, meaning the insulating effect of sediments with low thermal conductivity." – The deposition of cold sediment would lead to decreased temperature values at shallow depth and higher thermal values in the basement because of the blanketing of low conductivity sediments.

We thank the reviewer for the suggestion to separate the results and discussion sections, but we think the explanation of some basic features and processes can remain in the results part. Otherwise we would need to describe the results again in the discussion part to be able to discuss the observed temperature anomalies. Still, we moved some parts of the results to the discussion. We revised the sentence on the effect of sediment blanketing to be more clear. It is right that cold sediments would lead to decreased temperatures, but post-rift sedimentation initiated 10 Myrs ago, allowing sufficient time for the deposited Pannonian sediments to warm up and have an insulation effect on the deeper/older sediments and basement rocks. About the Rechnitz core complex, please see the previous reply. We also revised the representation of mismatch between modelled and observed temperatures. In terms of the magnitude of errors between modelled and observed temperatures, we show the largest mismatch on the maps. These values may remain relatively large due to potential measurement errors but may also be attributed to local variations in sediment geometry and composition that are not captured by the model, or can even be caused by local fluid convection e.g. in the carbonate basement as detailed in the ms.

A better fit with the overall temperature measurements at shallow depth could have been achieved with introducing variations in the thermal conductivity of sediments, accounting for local TC anomalies because of compositional differences (i.e. sand:shale ratio) compared to the average values we used, but the precise representation of the shallow (<5 km) thermal field was not the main purpose of this study. We discuss this also in the further points of reviewer #2.

How did you consider the uplift of the basin margins linked to the ongoing inversion of the basin (e.g. Bada et al. 2007)? Likely it would have a major impact.

The geometry and structure of the uplifted basin margins are taken into account by the present-day crustal geometry that is used as a model input. Further effects of the neotectonic inversion on the temperature field are considered negligible, due to the minor amount of shortening and thickening of the crust. Based on present-day shortening rates in the NW-Pannonian basin, the accumulated shortening strain over 8 Myrs would be around 0.024 (strain rates based on Porkoláb et al., 2023), which is around 5 km over a 200 km long section. This estimation shows that the thermal effects of this shortening is probably very low, and the most important effect is the geometry of the uplifted basin margins, which we do take into account in the model. For more details, please see the revised ms.

Structure of the manuscript: The results and their discussion are not separated. You should make clear which parameters and which model outputs are well constrained and what is the sensitivity of others.

We separated the results and discussion more clearly where applicable and described the model inputs and uncertainties in more detail. For more details, please see the previous comments and the revised version of the ms.

Comparison with previous studies: this manuscript doesn't even mention previous modelling efforts on the crustal and mantle thinning, surface heat flow and basin temperature evolution. In the detailed comments below, you find many useful papers that can be used to compare your results with previous inferences. Besides well data, vitrinite information is also widely available in the region that should be used to validate such models.

We thank the reviewer for the suggestions for previous studies. We revised the ms. to compare model parameters and results with further studies where relevant. About vitrinite reflectance data, we could not implement such observations in the model, but we added a short comment on their potential applicability to the ms.

Because of the many limitations listed above, the final sentences on the new stress envelope or comparison with xenoliths remain elusive and in general they don't really connect with the manuscript. Instead, you should discuss the sensitivity and reliability of the thermal model and compare it with previous inferences and with other similar regions.

We extended the discussion with a detailed "Model uncertainties and limitations" section and we also performed a sensitivity analysis. For more details, please see the revised version of the ms.

further detailed comments:

Title: it does not reflect the content of the manuscript

We revised the title.

Abstract: reliable thermal evolution is not modelled for the entire lithosphere due to the limitations of the modelling approach and lack of constraints

We added a sentence on the effect of selected initial parameters and the corresponding sensitivity analysis and model limitations to this specific case of parameter selection.

ln 19-20: not all the sedimentary basins are extensional

Corrected for the specific case of extensional basins.

ln 22: Royden and Keen 1980

Reference added.

ln 32: most thermo-mechanical models are constrained by observations, e.g. Lescoutre et al. 2019; Heckenbach et al. 2021; many others

We revised and partly excluded this part of the introduction.

ln 37: in the upper crust

Corrected.

ln 44: sometimes you include Late Miocene, in other places you write Early to Middle Miocene. Which is true?

Since extension migrated through time in the entire Pannonian basin, here we revised the text simply to Miocene.

ln 47: how is this inversion stage considered in the model?

Please see our previous response.

ln 53: i.e. compositional changes through sedimentation: what does this mean?

Here we only refer to the changes in thermal properties due to sedimentation, that corresponds to the change of composition of upper crustal structure. We revised the text to be more clear.

ln 54-56: you should reflect on the large number of previous thermal modelling efforts in the region, including, but not limited to: Lankreijer et al. 1999; Majcin et al. 2015; Bartha et al. 2018; Balasz et al. 2021; Rybar and Kotulova 2023

We extended the ms. with more comparisons where relevant and possible.

ln 59: "high precision" - can you elaborate?

We excluded this term and discussed model limitations and reliability in the ms.

ln 59: for (not to)

Corrected.

ln 73: lower plate with respect to what? Out of context.

With respect to the Alpine subduction system, as described in the text. We think that this is not out of context.

fig. 2: what is the sedimentary basin on the right side? Also indicate the orientation of the section.

We added a description to the ms. to explain the SE limit of the section and added the section orientation to the figure.

ln 87: justification?

We revised this value and discussed its effect, please see previous comments and the revised ms.

ln 91: delete -

Deleted.

ln 95: where is this thickness map presented, shown?

We uploaded the input carbonate thickness map to the corresponding data repository (https://data.mendeley.com/drafts/vp7jdp79y4), which we will make available with the publication of the paper.

ln 100-101: justification?

We revised and extended this part of the ms., please see the previous detailed comments and revised ms.

ln 118-120: rephrase

We rephased the text.

table 1: what about paleogene rocks?

The extent and thickness of Paleogene rocks at the study area is limited and were therefore not separated from the pre-Pannonian Neogene sediments in the models. Please see the revised ms.

table 1: what is the source of information behind this data?

We added a detailed description on how we constrained the composition and properties of the layers, including several references. Please see the revised ms.

ln 128: is it available or the most important data now made available with this manuscript?

The references for temperature measurements from which we selected the calibration dataset are described in the text. The original datasets are available in the appendix of Dövényi and Horváth, 1988 and in Dövényi et al., 2002 and on the website of the Geothermal Information System (Ogre, 2020). We uploaded the dataset of selected measurements to https://data.mendeley.com/drafts/vp7jdp79y4, which we will make available with the publication of the paper.

ln 135: meters?

Corrected.

ln 136: in fact I cannot see too many wells in the deep basins. Elaborate

We specified this sentence to the vicinity of the Zala basin.

Figure 3: scale of the basement depth map?

We added the scale to the map.

ln 161: what about different amounts of upper and lower crustal thinning, likely affecting the Rechnitz region?

The methodology cannot separate the upper and lower crustal stretching factors, but the resulting thermal effect due to the difference between lower and upper crustal stretching is considered by the present-day crustal geometry and composition. Please see also the previous detailed comments on the basin inversion.

ln173: sensitivity of this assumption? What if the initial lithopsheric thickness was lower or higher?

We included a sensitivity analysis in appendix A to demonstrate the effect of initial crustal and lithospheric thickness as well as the subcrustal stretching factor. For more details, please see the ms.

ln 174-175: In this model, when the lithosphere was thinned to ca. 60 km, you had a 60 km depth domain of constant temperature beneath? How reliable is this? Why dont you use a constant heat flow lower boundary condition?

We thank the reviewer for pointing out the need for the explanation of the model reliability below the present-day LAB depth. For all models, only lithospheric thermal properties were considered, and the post-stretching models extend until the depth of the initial LAB because of the pre-defined model geometry. Model temperatures can only be considered reliable approximately only until the present-day LAB depth. We also added this to the ms. and we describe in the methodology that the post-stretching models have the same geometry and resolution as the starting model.

Ln 180: so your model is only accurate until 5-10 km depth?

This statement is not referred to the overall resulting model, only to the steady-state part, and it does not mean the models are not accurate deeper.

Ln 190: instead of this, it would be more useful to write about the thinning factors of the study area

We added more discussion on the stretching factors to the ms., but we did not delete this introductory part to help the readers understand the terms used in the modelling.

Ln 196: grammar

Revised.

Ln 198-200: what is the limitation of this?

We commented on this in the newly added part of the discussion.

Table 2: Lab: 120 meters?

Revised.

Ln 204: There are many other studies calculating different crustal and mantle thinning, e.g.: Lankreijer et al. 1995; 1999; Majcin et al. 2015; Bartha et al. 2018; Balasz et al. 2021; Rybar and Kotulova 2023

Revised, please see previous comments.

Ln 205: Primary?

We deleted this term.

Ln 206: what does past-extension mean?

We meant the thermal state after extension, most importantly at present-day. We revised the text.

Ln 209: why 35 km?

Revised and explained in previous comments.

Fig. 5: how would you discuss these patterns?

We added more explanation to the discussion.

Ln 259-263: this is discussion, not results

We agree that this is discussion but it belongs to the interpretation of basic features described and therefore we left the revised version of this sentence in the results part.

Ln 275: I would respectfully challenge this statement. How can you be sure that the deposition of cold sediments would increase the temperature in such shallow depth? It would *increase at larger depth. Of course, you have higher temperature values, where the crust is thinner and therefore the mantle is more elevated.*

Please see our previous comment on the thermal effect of sedimentation. The fact that elevated temperatures in shallow depth are also a result of higher crustal and lithospheric stretching is fully valid and added to the ms.

Figure 7: which wells are these, what is the source of information? Is it open-source? At least the used and presented well data should be better documented and shared with this manuscript. It is also a warning sign how the errors increase with depth which questions the reliability of the models.

We added references to the datasets from which we constructed the calibration dataset. We uploaded the dataset used for calibration to https://data.mendeley.com/drafts/vp7jdp79y4, which we will make available with the publication of the paper.

Ln 294: ref

Corrected.

Fig. 10: on the well data the basin was much shallower, which is right? Furthermore, it is not likely that the crust would be laterally homogenous, therefore it is difficult to understand the value of this cross-section. The rheological section would of course be different if heterogeneities were included, but we have no detailed information on these.

The well is not located along the trace of the cross-section (please see Fig. 3b), therefore the difference between basin depth. We selected a cross-section trace to also include the deepest part of the Danube basin. The section is included to represent the difference between basins and basin margins and to provide an average approximate estimate on yield stresses.