

Author's Response to Reviews

Geothermal implications of the lithosphere's thermal structure in northern Pakistan

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Covering letter

Dear Editor,

We thank you for the opportunity to submit a revised version of our manuscript, and we are grateful to Referee #2, Referee #3 and Dr. Sebastián Oriolo for their careful and constructive assessments. All comments have been addressed, and the manuscript has been substantially revised as a result.

The principal changes are as follows. The mathematical formulation in Sections 3 to 5 has been rewritten in a single consistent notation; the transient governing equation now includes the material advection term with an explicit sign convention, the volumetric heat capacity is retained throughout the discretisation, and the numerical scheme is described accurately as linearly implicit with lagged coefficients. A Péclet-number analysis has been added to Sections 4 and 5 to quantify the relative importance of advective and conductive heat transport. The crustal differentiation index has been moved from the Discussion into its own Results section. Throughout the Abstract, Discussion and Conclusions, statements concerning geothermal resources have been reframed at the level of thermal favourability, with permeability, fracture connectivity, in-situ stress and hydrogeological conditions explicitly identified as unassessed. Model outputs and measured quantities are now consistently distinguished, and the calibration basis of the models is stated explicitly. A new panel has been added to Figure 6 showing the kinematic block subdivision and the imposed exhumation velocities.

Four points require your attention directly:

- We have not cited Ding et al. (2025), although our response to the community comment indicated that we would. Our reasoning is given in full under point 6 of that response.
- The Code availability statement currently indicates that the repository for the 1D transient exhumation code will be provided after acceptance. The archive is in preparation and the DOI will be supplied before final acceptance.
- The differentiation index material has been relocated from the former Section 6.2 into a new Results section. In the marked-up manuscript this appears as a large deletion followed by a corresponding insertion; no content has been removed.

We believe the manuscript is considerably stronger for this review process and hope it is now suitable for publication in Solid Earth.

On behalf of all co-authors,

Muhammad Anees

Response to Referee #2

We thank the reviewer for the careful reading of the manuscript and for raising constructive questions. Below we respond to each point and identify the corresponding changes.

Point 1 — Classification of geothermal environments

Reviewer comment

In Pakistan there are three types of geothermal energy environments, Volcanic in Balochistan, Tectonic in Northern area and Geopressed in Southern Indus Basin. You mentioned here radiogenic means source of radioactive particles present in northern area. Why not you classified area on the basis of geothermal gradient variation.

Authors' response

Our manuscript focuses on the tectonic (collisional) geothermal environment of northern Pakistan, comprising the Himalaya–Kohistan–Karakoram terranes. In this region no reliable regional-scale borehole temperature data exist from which geothermal gradients could be derived. Most gradient information available in Pakistan comes from sedimentary basins such as the Indus Basin, which are tectonically and thermally very different from the high-relief orogenic domains investigated here. Because of this limitation, we classify the study area by tectono-lithological domain and its radiogenic heat production, both of which can be constrained directly from petrophysical and geochemical measurements, and then predict geothermal gradients and heat flow using 1D and 2D thermal models.

No change to the manuscript was required for this point.

Point 2 — Calculation of surface heat flow

Reviewer comment

How you calculate surface heat flow through calculation of surface temperature or used geothermometers.

Authors' response

Surface heat flow was not measured from surface temperatures or geothermometers; it is an output of the thermal models. In the 1D steady-state models (Section 3) it is computed from the temperature gradient at the uppermost model node using Fourier's law, $q = -k \, dT/dz$, where k is the thermal conductivity of the surface layer. In the 1D transient models (Section 4) it additionally includes the advective component arising from upward material transport during exhumation. In the 2D model (Section 5) it is computed from the spatial temperature gradient at the model surface boundary.

Geothermometers were not employed because they estimate reservoir temperatures from hot spring fluid chemistry and are best suited to characterising individual hydrothermal systems, whereas this study targets the lithospheric-scale thermal structure.

No change to the manuscript was required for this point.

Point 3 — Hot-dry-rock potential and granitoid availability

Reviewer comment

What is the HDR potential in your three selected areas, because there is no such granite rocks are present such rocks present in Nagar Parker Pakistan.

Authors' response

Granitoid rocks are in fact abundant and well documented in northern Pakistan. The Nanga Parbat Massif exposes Proterozoic Indian basement gneisses and leucogranites with radiogenic heat production of $4\text{--}5 \, \mu\text{W m}^{-3}$; the Karakoram Batholith is a 600 km long, up to 30 km wide granitoid

intrusion comprising granodiorites, leucogranites and syenites with heat production of about $2.5 \mu\text{W m}^{-3}$; and the Kohistan arc, although predominantly mafic, contains a felsic batholithic component with lower but non-negligible heat production of about $1 \mu\text{W m}^{-3}$. These lithologies are described in our earlier studies (Anees et al., 2023, 2024). On thermal grounds the Nanga Parbat Massif and the Karakoram Batholith are the most favourable, with the additional advantage of access through the deeply incised Indus and Hunza valleys.

No change to the manuscript was required for this point.

Point 4 — Comparison with Tibet and the Puga Valley

Reviewer comment

Are you discussing about Himalayan Geothermal Belt in introduction, have you gone through the recent development of geothermal energy exploitation activity in Tibet in China and Puga valley India. What is subsurface temperature there and at what depth geothermal anomaly present and also production potential of these fields and correlate it with geothermal areas of Pakistan.

Authors' response

We thank the reviewer for this suggestion, which we have adopted. The Himalayan Geothermal Belt extends along the whole Himalaya through Tibet and into Nepal and hosts numerous hot springs whose physical and chemical characteristics vary considerably with local hydrogeological conditions. Yangbajing in Tibet and the Puga Valley in Ladakh are among the high-temperature hydrothermal systems of the belt, in which heat is convected by deep circulation of meteoric water.

Change made: A new passage has been added to Section 7.2 comparing reservoir depths and temperatures at Yangbajing (150–165 °C at 180–280 m; 250–329 °C at 950–2000 m) and Puga (approximately 200–250 °C at 1–2 km) with our modelled values, and noting explicitly that our values are scenario-based model outputs rather than drilled or geophysically constrained measurements. Four references have been added: Fan (2002), Xue et al. (2023), Shanker et al. (2000) and Harinarayana et al. (2006).

Point 5 — Sentence on the origin of the anomalous heat

Reviewer comment

Correct the sentence despite numerous Cenozoic intrusions, the absence of active volcanism and low ^3He concentrations suggest a primarily crustal origin of anomalous heat.

Authors' response

We agree that the original sentence was imprecise, in particular in implying that the Cenozoic intrusions were unexpected given the absence of volcanism.

Change made: The sentence in the Introduction has been replaced with: “Although the region hosts numerous Cenozoic crustal intrusions — the products of collision-induced anatexis — the absence of active arc or rift volcanism and the low $^3\text{He}/^4\text{He}$ ratios in hot spring gases collectively indicate that the anomalous heat is of crustal origin, rather than reflecting a direct mantle contribution.”

Point 6 — Geothermometers and oil well data

Reviewer comment

Why not you use geothermometers for actual calculation of subsurface temperature, also use oil wells data for geothermal gradient data.

Authors' response

Chemical geothermometers applied to hot spring waters and temperature logs from hydrocarbon wells are valuable for site-specific resource assessment but are not ideal constraints on the regional background lithospheric thermal structure that we model. Most deep wells with reliable temperature logs in Pakistan are located in the Indus Basin and other southern sedimentary basins, which are tectonically distinct from the high-relief collision belt studied here; combining those gradients with ours would mix fundamentally different thermal regimes. Hot spring geothermometers in the Himalaya–Karakoram commonly reflect mixing, boiling and re-equilibration along deep flow paths, and are strongly influenced by local hydrology and permeability. We therefore based the models on measured radiogenic heat production and thermophysical properties of the main lithologies together with geophysical constraints on crustal and lithospheric thickness, and predicted geotherms and surface heat flow self-consistently.

Change made: The limitations section (7.3) now states explicitly that the models are constrained by surface radiogenic heat production, geological interpretation, thermochronologically derived exhumation rates and literature-derived crustal structure, and are not calibrated against any direct thermal observation.

Point 7 — Temperatures of 1200–1300 °C at 150–250 km

Reviewer comment

Heat flow models showing 1200 Centigrade versus 250 km is it possible for economic usage of geothermal energy.

Authors' response

The temperatures of approximately 1300 °C in our figures refer to the assumed basal temperature at the lithosphere–asthenosphere boundary, which is placed at 150–250 km depth to control the deep boundary condition of the conductive models. These values define the deep thermal state of the lithosphere in line with standard continental geotherm modelling and are not intended to represent exploitable resources. The depths relevant to utilisation are far shallower: the models indicate temperatures above 100 °C at approximately 1200–2500 m in the valleys of the Nanga Parbat Massif, and above 200 °C within about 3 km under scenarios of high radiogenic heat production or high exhumation.

Change made: Section 3.2.1 has been expanded to justify the choice of a Dirichlet condition at the lithosphere–asthenosphere boundary, to state which assumptions were and were not tested, and to note that the boundary temperature carries no implication for accessible geothermal depths. Degen et al. (2025) is cited for the broader discussion of boundary-condition influence on model reliability.

Point 8 — Magmatism versus radiogenic and topographic effects

Reviewer comment

According to your conclusion heat energy not produce due to magmatism but due to overburden and topographic effect, but I think main agent of heat in collision part is heat produce due to magmatism originated due to collision effect and plate boundaries. Overburden heat generated by geopressed effect of overburden mostly in south Indus Basin. Please explain it.

Authors' response

We acknowledge that magmatism has played an important role in the thermal evolution of the Himalaya–Karakoram system, and the manuscript notes the presence of numerous Cenozoic intrusions. Our point is that the present-day elevated heat flow can be explained without requiring a currently active magmatic body in the upper crust or shallow mantle. Crustal thickening and partial melting have redistributed heat-producing elements into the upper and middle crust, raising crustal temperatures over tens of millions of years; rapid exhumation then advects this radiogenically heated

crust upward; and topography together with focused fluid flow localises the heat in valleys and along fault zones. The “overburden” or geopressured effects of the southern Indus Basin relate to thick, low-permeability sedimentary successions and represent a different geothermal play type from the crystalline-basement system studied here.

Change made: The Discussion has been revised so that the manuscript no longer attributes magma generation specifically to crustal anatexis, and the Conclusions distinguish the mechanism identified here from the magmatic systems of Balochistan and the geopressured aquifers of the southern Indus Basin. The role of topography is now described as modulating the depth at which an already elevated geotherm becomes accessible, rather than as a heat source.

Point 9 — Spatial variation of the heat flow anomaly

Reviewer comment

Please explain heat flow anomalous variation in near collision point and rest of the surrounding areas.

Authors' response

The modelled heat flow varies systematically across the transect and is controlled by three spatially variable factors: radiogenic heat production, which is highest in the Nanga Parbat Massif and the Karakoram Batholith and lowest in the mafic-dominated Kohistan arc; exhumation rate, which is fastest in the Nanga Parbat Massif and Karakoram and slowest in Kohistan; and topography, which locally focuses geothermal gradients in deep valleys. The Nanga Parbat Massif, where the highest exhumation rates coincide with the most enriched crust, shows modelled surface heat flow of 120–180 mW m⁻², two to three times the global continental average. Kohistan forms a relative thermal corridor at 50–85 mW m⁻², and the Karakoram is intermediate at 65–120 mW m⁻².

This spatial pattern is presented in Section 5 and in Figure 7; no further change was required.

Response to the Community Comment (Dr. Sebastián Oriolo)

We thank Dr. Oriolo for a thorough and constructive review. Each point is addressed below.

Point 1 — Xenolith constraints on subsurface radiogenic heat production

Reviewer comment

You assume radiogenic heat production based on surface geology and use some assumptions to evaluate their spatial continuity. Don't you have any xenolith in exposed intrusions that may be useful to evaluate the subsurface geology?

Authors' response

This point addresses one of the key uncertainties in our approach, and we agree that the vertical extrapolation of surface-derived values is a significant source of uncertainty. To the best of our knowledge, no systematic xenolith studies focused on radiogenic heat production have been conducted in the Nanga Parbat, Kohistan or Karakoram intrusions, and existing xenolith populations have not been characterised in a way that would constrain crustal heat production at depth. The Kohistan arc is an exception in exposing a near-complete crustal section, from which heat production values can be assigned to depth-equivalent layers (Mukai et al., 1999). For the Nanga Parbat Massif and the Karakoram we rely on global analogues for the continental crust (Hasterok and Chapman, 2011; Jaupart et al., 2016).

Change made: Statements have been added to Section 3.2.2 and to the limitations section (7.3) noting that no systematic xenolith studies of the exposed intrusions have characterised deep-crustal radiogenic heat production, that this makes the vertical partitioning the dominant model uncertainty, and that such studies represent an important target for future work.

Point 2 — Criteria for the preferred models

Reviewer comment

Some parts of the text need further details on criteria to define model parameters. For instance, when you refer to 'preferred models', you have to state clearly why are they considered as such.

Authors' response

We agree that the selection criteria were not sufficiently explained. The preferred models were selected on three criteria: consistency with the observed surface geology and crustal structure reported in the literature, such that the layer-wise heat production values agree with measured surface heat production from our field gamma spectrometry data (Anees et al., 2023) and with published values for the corresponding lithologies; Moho and mid-crustal temperatures falling within ranges compatible with regional metamorphic constraints; and median parameter selection, so that Model 3 in each set represents median values of the explored range and avoids end-member scenarios.

Change made: These criteria are now stated explicitly in Section 3.2.3 where the preferred model is first introduced, and the corresponding statements in Sections 3.2.4 and 3.2.5 identify Model 3 as the median, geologically consistent solution within the explored parameter ranges.

Point 3 — Use of 'uplift' and 'exhumation'

Reviewer comment

Please revise the use of the terms 'uplift' and 'exhumation', since in some cases they may be mixed up.

Authors' response

We thank the reviewer for this terminological point. In the context of the thermal models the physically relevant parameter is exhumation, and the rates cited from thermochronological data are exhumation rates derived from cooling age gradients rather than rock uplift rates.

Change made: A manuscript-wide review of both terms has been carried out. ‘Exhumation’ is now used consistently wherever the thermal-model process is described, including in the Abstract, Section 4, Table 3 and the figure captions.

Point 4 — Behaviour of Th and U (former lines 337–339)

Reviewer comment

These statements are not totally correct. Both Th and U can be concentrated in magmas, as in the case of A-type magmatism. In fact, your own geochemical data show a general positive correlation between U and Th, except for some gneisses and granites that show high U with low Th. On the other hand, U may be more relevant for radiogenic heat production than Th, and can also be concentrated in zircon, allanite, etc. In migmatites, leucosomes may have the ‘magmatic’ fingerprint, so they may not be necessarily different to granitoids in general.

Authors’ response

We appreciate this detailed comment and agree that there is a broadly positive correlation between U and Th across the sampled lithologies, and that the two elements should not be presented as systematically decoupled during anatexis. Our intention was to highlight the difficulty of assigning representative heat production values at mid-crustal levels in orogenic settings where high-temperature metamorphism and sub-solidus to partial melting prevail, as illustrated by the high Th/U ratios of migmatitic gneisses relative to associated leucogranites in the Nanga Parbat Massif (Anees et al., 2024).

Change made: The passage has been rewritten in Section 7.1 to state that U and Th are commonly co-enriched in evolved granitic melts and broadly positively correlated in our dataset, that both contribute significantly to radiogenic heat production and may be retained in resilient accessory phases, and that leucosomes carrying a magmatic trace-element fingerprint are not necessarily distinguishable from granitoids in this respect.

Point 5 — Placement of the crustal differentiation index

Reviewer comment

The introduction of crustal differentiation first in Section 6.2 of the Discussion should be revised. Some parts should be perhaps included in the Results.

Authors’ response

We agree that the differentiation index is partly methodological and is better introduced earlier.

Change made: The definition of the differentiation index, the associated equations, the computed values for the Nanga Parbat Massif, Kohistan and Karakoram, and the corresponding figure have been moved from the Discussion into a new Results section. The Discussion now focuses on the interpretation of these values in the context of crustal evolution and their geothermal implications. We also note explicitly that, because the 1D models are steady-state and conductive, the mean crustal heat production derived in this way is equivalent to the thickness-weighted mean of the assumed values, so the differentiation index does not constitute an independent test of the assumed vertical structure.

Point 6 — Mantle source as an alternative magma generation mechanism

Reviewer comment

The authors generally consider crustal anatexis as the main mechanism for magma generation and transfer, but keep in mind that a mantle source with subsequent differentiation may also be possible (e.g., Ding et al. 2025 PNAS).

Authors' response

We fully agree that mantle-derived magmas with subsequent differentiation have contributed to the magmatic history of the India–Asia collision zone. The mid-Cretaceous subduction-related granodiorites and diorites of the Hunza plutonic unit attest to at least a partial mantle contribution, and the post-collisional Miocene syenites of the Baltoro and Kande complexes may have more complex source signatures. For the purposes of our thermal models, however, the relevant quantity is the present-day distribution of heat-producing elements in the crust, irrespective of the petrogenetic pathway by which they arrived there.

Change made: In revising the Discussion we removed the passages that attributed magma generation specifically to crustal anatexis, so the manuscript no longer favours one petrogenetic mechanism over another. The Discussion now describes partial melting and melt extraction without taking a position on the magma source.

We have not cited Ding et al. (2025). That study addresses the genetic mechanism of Himalayan leucogranite formation, a topic not discussed anywhere in our manuscript, and we judged that citing it would introduce a subject outside the scope of the present thermal modelling. We hope the reviewer will agree that the underlying concern has been resolved through the revision of the text itself.

Point 7 — Evidence linking hot springs to radiogenic lithologies

Reviewer comment

Do you have any information to robustly demonstrate the relationship of hot springs with rocks related to RHP? E.g., high radon flow?

Authors' response

Elevated radon in spring waters can indicate interaction of hydrothermal fluid with uranium-bearing rock. Two studies undertaken for health risk assessment (Ullah et al., 2022; Muhammad and Haq, 2023) report spatially variable radon concentrations in the region, with the highest value of 304 Bq/L at the Tattapani spring at Raikot Bridge on the western flank of the Nanga Parbat Massif. We agree, however, that radon alone does not demonstrate that the heat source is radiogenic.

Change made: Section 7.2 now presents these measurements as a possible indication of fluid interaction with uranium-bearing lithologies rather than as independent support, and notes that a dedicated radon or noble-gas survey would be required to establish the relationship.

Response to Referee #3

We thank the reviewer for a thorough, technically detailed and constructive review, and for taking the earlier interactive discussion into account. The comments on the mathematical formulation, the treatment of advection and the framing of the geothermal implications have substantially improved the manuscript.

Major comment 1 — Geothermal implications versus geothermal resources

Reviewer comment

The manuscript discusses “geothermal potential”, “geothermal resources”, and geothermal exploration targets. However, the presented modelling framework only evaluates the temperature field and associated conductive heat flow. The authors should consistently distinguish between thermal favourability, geothermal resource estimation, and viable geothermal exploration targets.

Authors' response

We acknowledge that our models evaluate only the thermal field and do not address permeability, reservoir quality, fluid availability, fracture connectivity or stress state, all of which are essential for resource estimation. The study therefore demonstrates thermal favourability as a first-order prerequisite rather than a geothermal resource assessment.

Change made: Terminology has been revised throughout the Abstract, Section 7.2 and the Conclusions. Statements such as “prime targets for geothermal exploration” and “significant geothermal potential” have been reworded in terms of thermal favourability; the sentence identifying deep valleys as “optimal sites for accessing high-temperature reservoirs” has been replaced with wording identifying them as priority areas for further investigation; and an explicit statement has been added to Section 7.2 that the transition from thermal favourability to a resource estimate requires characterisation of permeability, fracture connectivity, in-situ stress and hydrogeology, none of which are addressed by our models.

Major comment 2 — Quantitative confidence versus available constraints

Reviewer comment

The degree of quantitative confidence sometimes exceeds what is justified by the available data. The manuscript would benefit from a more explicit discussion of the limited calibration basis.

Authors' response

We agree. The models are constrained by surface heat production measurements, geological interpretation, thermochronologically derived exhumation rates and literature-derived crustal structure, and no direct heat-flow or borehole temperature data exist for calibration.

Change made: A dedicated paragraph has been added to the limitations section stating the calibration basis explicitly, noting that the models are not validated in the formal sense since validation requires comparison against independent observations of the modelled quantity, and identifying the requirement that Moho and mid-crustal temperatures remain petrologically and geophysically plausible as the principal external check. The Abstract and Conclusions have been revised to present the results as bracketed scenarios rather than deterministic predictions, and assertive verbs applied to model outputs have been softened throughout.

Major comment 3 — Constraints on the subsurface distribution of heat production

Reviewer comment

The largest uncertainty in the modelling framework concerns the vertical distribution of radiogenic heat production. This limitation should be emphasized more strongly throughout the manuscript.

Authors' response

We agree, and appreciate the reviewer's recognition that the approach is reasonable given the data situation. For the Nanga Parbat Massif and the Karakoram the vertical partitioning is necessarily assumption-driven.

Change made: The point has been strengthened in three places: Section 3.2.2 now states that the vertical partitioning of heat production is the dominant model uncertainty; Section 7.1 reframes the depth-dependence conclusion as emerging from bracketing plausible stratifications rather than from a calibrated inversion; and the limitations section and Conclusions state that the range spanned by the seven scenarios per terrane expresses structural uncertainty rather than statistical confidence bounds.

Major comment 4 — High predicted temperatures and heat-flow values, and Péclet analysis

Reviewer comment

Several model scenarios predict surface heat flow approaching 180–250 mW m⁻², temperatures exceeding 600 °C at 10 km depth, and even inverted geotherms. Please discuss whether these values are physically realistic, whether they represent transient end-member scenarios, and whether they should be interpreted as localized anomalies. Also interesting could be a Pe-number analysis to compare diffusion vs advection.

Authors' response

We agree that the distinction between exploratory end-members and plausible present-day states required clarification. The highest values arise only in the combination of sustained exhumation at 3 mm y⁻¹ over 10 Ma with high heat production, and could apply spatially only to the localised, rapidly exhuming core of the Nanga Parbat syntaxis.

Change made: Sections 4.2 and 7.2 now state explicitly that these values are exploratory upper bounds rather than expected regional conditions, that they are spatially confined to the Nanga Parbat Massif, and that the moderate scenarios (≤ 120 mW m⁻²) represent our preferred description of the regional thermal state. The Abstract has been revised accordingly. We also note that the inverted geotherms are a joint consequence of the fixed basal boundary and the imposed heat production and mark the limit of validity of the fixed-heat-production formulation.

Change made: A Péclet-number analysis has been added. In Section 4, for a 25 km column and a thermal diffusivity of approximately 10^{-6} m² s⁻¹, exhumation rates of 1, 2 and 3 mm y⁻¹ give $Pe \approx 0.8$, 1.6 and 2.4. In Section 5 the analysis is repeated for the shallower 2D domain, where the corresponding length scale and Péclet numbers are lower. This provides a non-dimensional basis for comparison with other orogens.

Major comment 5 — Limitations of the two-dimensional model

Reviewer comment

The 2D model extends only to 10 km depth, inherits its lower boundary conditions from the 1D models, neglects hydrothermal circulation, and represents exhumation using simplified translational motion. As a result, the 2D simulations are not an independent validation of the 1D results. This dependency should be discussed more explicitly.

Authors' response

We agree on both points. Because the basal temperature boundary is derived from the median 1D transient models, the 2D simulations inherit their assumptions and do not constitute independent validation. Their value lies in resolving the lateral interaction between adjacent blocks with contrasting heat production and exhumation rate, and the topographic modulation of shallow isotherms, neither of which the 1D framework can capture.

Change made: The opening of Section 5 and the limitations section now state this dependency explicitly and reposition the 2D model as a lateral and topographic extension of the 1D framework rather than an independent check. The 10 km depth limit is described as a deliberate modelling choice reflecting the geothermally relevant upper crust. The limitations section has been strengthened to clarify that the conductive temperature field provides a background thermal state, demonstrating that sufficient heat is available at depth to sustain hydrothermal systems but not the specific flow pathways, so the link to the observed springs is indirect.

Section 3 — comments on the 1D steady-state model

Reviewer comment

Clarify why a numerical finite-difference solution is required; discuss the physical validity of assuming steady state; use standard differential notation; justify the Dirichlet boundary condition at the base of the lithosphere and discuss sensitivity (see Degen et al., 2025).

Authors' response

The motivation for the finite-difference solution is not solely the non-linear temperature dependence of conductivity. The geological model contains abrupt lithological contrasts in conductivity and heat production, so we discretise the conservative operator directly using coefficients evaluated at half stations, which avoids the product-rule expansion that would require a differentiable conductivity field.

Change made: A statement of this rationale has been added to Section 3.1, together with a new equation defining the discrete conservative conduction operator, which is subsequently reused in Section 4.

Change made: The purposes of the steady-state models are now stated explicitly at the start of Section 3, together with a clarification that the assumption is violated in the rapidly exhuming Nanga Parbat Massif, which is precisely why the transient and two-dimensional models are introduced.

Change made: All equations in Sections 3 to 5 have been rewritten in standard partial differential notation with harmonised symbols.

Change made: Section 3.2.1 now justifies the Dirichlet condition, notes that a Neumann formulation is often preferable but cannot be anchored here, states which of the three associated assumptions were and were not tested, and cites Degen et al. (2025) for the broader discussion of boundary-condition influence on model reliability.

Section 4 — comments on the transient model

Reviewer comment

No advection term appears in the governing equation although exhumation is the central process. The governing equation should explicitly include the material advection term and define the sign convention. The units of thermal conductivity appear incorrect, the transition from Eq. (6) to Eq. (7) omits the ρc term, and the scheme appears semi-implicit rather than fully implicit.

Authors' response

The reviewer is correct on all points, and we are grateful for the careful reading.

Change made: The governing equation now includes the material advection term explicitly, and the sign convention is stated: depth is positive downward and radiogenic heat production is defined as a positive volumetric heat source, so that upward exhumation corresponds to a negative vertical material velocity. The numerical treatment is described in full: advection and conduction are handled by operator splitting on a fixed grid, with the temperature field and associated material properties translated upward by one node whenever the accumulated exhumation displacement reaches one

grid interval. A sentence has been added clarifying that the governing equation states the physical conservation law while the remapping procedure specifies how its material-transport component is represented numerically.

Change made: The units of thermal conductivity have been corrected to $\text{W m}^{-1} \text{K}^{-1}$. The volumetric heat capacity is now retained explicitly throughout the discretisation, which is written in terms of a volumetric heat-capacity matrix appearing on both sides of the matrix equation. The scheme is now described accurately as backward Euler, linearly implicit with lagged coefficients, rather than fully implicit.

Section 5 — comments on the two-dimensional model

Reviewer comment

The statement that a “translational term” is added to the heat conduction equation requires clarification. I suggest including a Peclet-number analysis. In addition, Figure 7 would benefit from an additional panel showing the imposed exhumation field and the geometrical subdivision into tectonic blocks.

Authors’ response

We have clarified that exhumation is implemented using the Solid with Translational Motion formulation in COMSOL Multiphysics®, which is solved in the spatial frame and introduces heat transport by moving solid material, so that the governing equation is an advective–conductive heat transport equation with internal radiogenic heat production. No fluid-flow advection is included.

Change made: Section 5 has been rewritten accordingly, and a Péclet-number analysis for the 2D domain has been added, with the thermal diffusivity evaluated from the assigned petrophysical properties.

Change made: An additional panel has been added to Figure 6 showing the subdivision of the model domain into kinematic blocks together with the imposed translational velocities used in Models 1 to 3. We placed this panel in Figure 6 rather than Figure 7 because Figure 6 already presents the model setup, which keeps Figure 7 confined to results.

Discussion and conclusions

Reviewer comment

The result that surface RHP values cannot simply be extrapolated to the middle and lower crust is particularly important and can be highlighted. Statements regarding petrothermal resources, geothermal targets, and exploration opportunities should be revised to reflect the fact that reservoir properties, permeability, and hydrogeological conditions have not been investigated.

Authors’ response

Change made: All statements on petrothermal resources, geothermal targets and exploration opportunities in Section 7.2 and the Conclusions have been revised to the level of thermal favourability, with an explicit acknowledgement that reservoir properties, permeability and hydrogeological conditions were not investigated and are prerequisites for any resource classification. The finding that surface heat production values cannot be extrapolated to the mid- and lower crust has been given greater prominence in both the Abstract and the Conclusions, and is now presented as bounded to the studied collisional system, with the transferable element identified as the diagnostic approach rather than the specific result.

Minor comments

Reviewer comment

Clarify the basis for the 'preferred models'; Section 3.2.3 thermal conductivity values appear to be missing for one unit; explain the origin of the value $5.33 \mu\text{W m}^{-3}$; ensure consistent usage of 'uplift' and 'exhumation'; consider adopting more standard differential notation.

Authors' response

Change made: The preferred-model criteria are now stated explicitly (see also our response to the community comment, point 2). The missing thermal conductivity value of $2.6 \text{ W m}^{-1} \text{ K}^{-1}$ has been inserted. The origin of the $5.33 \mu\text{W m}^{-3}$ value is now explained as the mean heat production of the high heat-producing gneisses and leucogranites in our in-situ gamma spectrometry dataset (Anees et al., 2023), as distinct from the area-weighted average of $4 \mu\text{W m}^{-3}$. A manuscript-wide terminology review of 'uplift' and 'exhumation' has been completed, and all equations have been rewritten in standard differential notation.

List of changes made to the manuscript

The following summarises all substantive changes. Minor corrections of grammar, punctuation and reference formatting are not listed individually.

Title / front matter	A Data availability statement has been added. The Code availability statement now distinguishes the steady-state and transient codes.
Abstract	Reframed in terms of thermal favourability; scenario-based character of the models stated; extreme heat-flow values identified as exploratory end-members; the finding that surface heat production cannot be extrapolated to depth given greater prominence; 'uplift' replaced by 'exhumation'.
Introduction	Sentence on Cenozoic intrusions and $^3\text{He}/^4\text{He}$ ratios rewritten.
Section 3	Purposes of the steady-state models stated at the start of the section; rationale for the conservative finite-difference scheme added, with a new equation defining the discrete conduction operator; Dirichlet boundary condition justified, assumptions tested and untested distinguished, Degen et al. (2025) cited; xenolith limitation and the dominant role of vertical heat-production partitioning added to Section 3.2.2; missing thermal conductivity value inserted; origin of the $5.33 \mu\text{W m}^{-3}$ value explained; preferred-model criteria stated in Sections 3.2.3 to 3.2.5.
Section 4	Governing equation rewritten to include the material advection term, with the sign convention stated; operator-splitting implementation described in full, including the properties assigned to material entering at the base; conductivity units corrected; volumetric heat capacity retained explicitly through the discretisation; numerical scheme described as linearly implicit with lagged coefficients; Péclet-number analysis added; end-member scenarios explicitly identified as exploratory upper bounds; restriction of the domain to 25 km and the basis of the 800 °C basal condition explained.
Section 5	Governing equation and COMSOL implementation clarified; dependency of the 2D model on the 1D boundary condition stated explicitly; 10 km depth limit described as a modelling choice; Péclet-number analysis for the 2D domain added.
New Results section	Definition of the crustal differentiation index, associated equations, computed values and the corresponding figure relocated from the Discussion, with a note that the index is not an independent test of the assumed vertical structure.
Section 7 (Discussion)	Passage on U and Th behaviour rewritten; magma-source attribution removed; comparison with Yangbajing and Puga added, with modelled and measured quantities explicitly distinguished; radon evidence reframed as a possible indication rather than independent support; hot spring distribution no longer presented as establishing topographic modulation of the conductive field; resource statements reframed as thermal favourability; calibration basis, formal validation status, xenolith limitation, structural uncertainty and hydrothermal circulation caveat added to the limitations section.
Section 8 (Conclusions)	Resource language replaced with thermal favourability and screening-level framing; the non-extrapolability result bounded to the studied system with the transferable element identified as the method; uncertainty statement added; distinction drawn between the collisional, magmatic and geopressured geothermal settings of Pakistan.
Figures and tables	New panel added to Figure 6 showing kinematic block subdivision and imposed exhumation velocities; Figure 8 caption notes that values for this study derive from modelled heat flow whereas comparison provinces derive from measured heat flow; stray annotation text removed from the body near Figure 1.
References	New entries added for Degen et al. (2025), Ullah et al. (2022), Fan (2002), Xue et al. (2023), Shanker et al. (2000), Harinarayana et al. (2006), Mukai et al. (1999) and

	COMSOL AB; previously uncited works cross-checked; formatting harmonised to Copernicus style.
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