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REPLY

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Key Points:

- The Comment misinterprets process-oriented geodynamic models as re-constructions of present-day structures
- Non-unique interpretations of geophysical data are assumed a priori and used to reject alternative models, that is, circular reasoning
- The critiques neither offer alternative explanations nor engage with the limitation of the classical model that motivates our study

Correspondence to:

P. Sternai,
pietro.sternai@unimib.it

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Reply to Comment by Hetényi and Cattin on: "Raising the Roof of the World: Intra-Crustal Asian Mantle Supports the Himalayan-Tibetan Orogen"

P. Sternai[1], S. Pilia[2], S. Ghelichkhan[3], P. Bouilhol[4], A. Menant[5], L. Ostorero[1], B. Vaes[1], R. Esposito[1], E. Garzanti[1], S. Cloetingh[6], D. R. Davies[3], and T. Gerya[7]

[1]Department of Earth and Environmental Sciences, University of Milano-Bicocca, Milano, Italy, [2]College of Petroleum Engineering and Geosciences, King Fahd University of Petroleum and Minerals, Dhahran, Saudi Arabia, [3]Research School of Earth Sciences, Australian National University, Acton, ACT, Australia, [4]CNRS, CRPG, University of Lorraine, Nancy, France, [5]Observatoire de la Côte d'Azur, Géoazur, Université Côte d'Azur, IRD, CNRS, Valbonne, France, [6]Department of Earth Sciences, Utrecht University, Utrecht, The Netherlands, [7]Department of Earth Sciences, ETH-Zurich, Zurich, Switzerland

Abstract We welcome the critique of Hetényi and Cattin (2026, https://doi.org/10.1029/2025tc009214) on Sternai et al. (2025, https://doi.org/10.1029/2025tc009057), as it brings to light evaluative issues that require clarification and offers an opportunity to articulate more clearly how our work advances understanding of Himalayan-Tibetan geodynamics. In this Reply, we clarify that most of the criticism arises from evaluating our process-oriented geodynamic models as if they were intended to reproduce present-day lithospheric structures, rather than to test the mechanical plausibility of Paleogene crustal-doubling mechanisms. Moreover, non-unique interpretations of present-day geophysical data are treated in the Comment as definitive constraints and used as benchmarks to dismiss alternative physically viable scenarios, leading to circular reasoning. When the content and objectives of our study are correctly considered, the critiques do not invalidate our results or conclusions. More broadly, this Reply underscores the importance of reassessing established models when fundamental limitations are identified, as a constructive pathway toward progress in Earth sciences.

1. Introduction

Progress in Earth sciences commonly proceeds along two complementary paths. One consists in addressing open questions within the framework of established models, provided those models are demonstrably robust. The other involves refining or extending an established model when fundamental limitations are recognized by developing and testing new hypotheses that broaden its applicability. Our contribution (Sternai et al., 2025) is intended to take this second approach. The Comment greatly exemplifies the inherent difficulty of reassessing deeply ingrained though uncertain assumptions that this approach faces. We therefore thank Hetényi and Cattin (2026) for the opportunity to contribute to a constructive discussion on how established models should be revisited when fundamental limitations are identified in order to allow for scientific progress.

2. On the Scope and Purpose of Our Modeling: Reply to Comment Sections 1 and 2

In a context where the outstanding elevation of the Himalayan-Tibetan orogen is explained through tectonic thickening of buoyant crust to thicknesses of 70–80 km, we outline a limitation of such model in Section 1 of Sternai et al. (2025): "*Because ductile crustal flow, which limits the lithospheric strength […], is activated at* ~35–40 km *depth* (e.g., Burov & Watts, 2006)*, the formation and persistence of the Earth's highest and largest orogen […] remains enigmatic*" (see also Figure 1). To address this limitation, we propose and substantiate that interlayered rigid mantle within the thickened crust contributes to sustaining high elevations. Hetényi and Cattin (2026) reject this hypothesis without offering an alternative explanation or engaging with the mechanical paradox that motivates our study. As we discuss hereafter, critiques evaluate our results against expectations derived from the very framework whose limitations we explicitly set out to test. However, rejecting a new hypothesis because it departs from prevailing, though non-unique interpretations does not constitute an independent scientific test.

The premise of our geodynamic modeling is stated explicitly in Sections 3 and 4.2 of our paper. Our simulations are process-oriented experiments designed to test the mechanical plausibility of crustal doubling mechanisms during the Paleogene, whose resulting structures may persist to the present. The Comment incorrectly attributes to

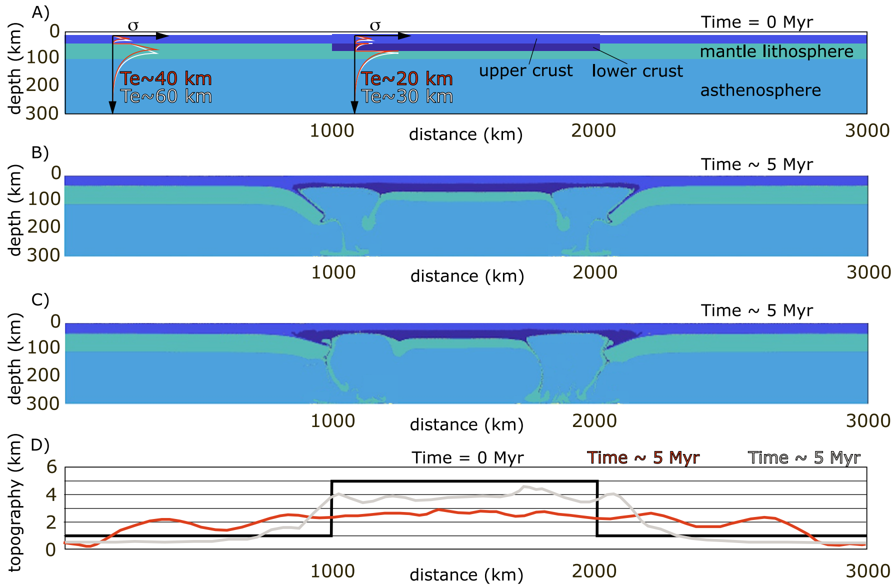

Figure 1. Stability test for a topographic plateau above thickened continental crust (similar to the configuration presented by Burov & Watts, 2006). The numerical technique is the same as in Sternai et al. (2025). (a) Numerical setup consisting of a continental plate with crust thickened to 70 km within the central 1,000 km of the model domain. The model domain is 3,000 km wide and 300 km deep and is resolved by an irregular rectangular grid with 401 × 41 nodes. Advection is tracked using up to 2 million Lagrangian particles. Free-slip boundary conditions are imposed on all boundaries of the model domain, with no tectonic convergence or extension. The uppermost 20 km of the model consists of sticky air (density 1 kg m^{-3} and viscosity 10^{18} Pa s) to allow investigation of the topographic evolution and avoid numerical instabilities. The initial isostatically supported topography includes a plateau with an elevation of 5 km. All lithologies follow a visco-elasto-plastic rheology with the same parameters as in Sternai et al. (2025). The initial temperature increases linearly from 0°C at the surface to ~1,330°C at depths of either 90 km (b) or 100 km (c). The resulting vertical rheological envelopes inside and outside the plateau (shown in the figure) correspond to lithospheric effective elastic thickness (Te; sensu Burov & Watts, 2006) of ~20 km (red envelope) or ~30 km (white envelope) within the plateau and ~40 km (red envelope) or ~60 km (white envelope) outside the plateau. Below this level, a vertical temperature gradient of 0.5°C km^{-1} is prescribed in the asthenospheric mantle. Thermal boundary conditions are 0°C at the upper boundary, an infinite-like condition at the lower boundary, and no heat flux across the lateral boundaries. (b, c) Crustal and mantle structures after ~5 Myr of model time, corresponding to cases where the initial ~1,330°C isotherm lies at ~90 km (b) and ~100 km (c) depth, respectively. In both scenarios, mantle convective instability (i.e., delamination; as in Burov & Watts, 2006) leads to the replacement of lithospheric mantle by hot asthenosphere, resulting in thermal erosion of the base of the crust, Moho flattening and reduced topographic support. Convective removal of the mantle lithosphere is more effective when Te is smaller (b), but it occurs regardless of mantle rigidity when a single thickened crustal layer extending from 0 to 70 km depth is imposed. Burov and Watts (2006) presented similar experiments with weaker and stronger lithosphere (Te ≈ 20 km and ≈65 km), in which convective mantle removal begins after ~1 and ~10 Myr, respectively. (d) Topographic profiles at model times of 0 Myr (black profile, initial topography, panel a) and ~5 Myr for the experiments shown in panels (b) (red profile) and (c) (gray profile). After only ~5 Myr, the average elevation of the plateau is reduced by ~60% in the weaker lithosphere case (b) and by ~30% in the stronger lithosphere case (c). For the investigated parameters, this process continues until the plateau is completely dismantled within ~15 Myr at most. Note that, if imposed convergence and/or erosion were accounted for, lithospheric instabilities would develop and sink even faster and topographic collapse would be accelerated.

our models the aim of providing unique reconstructions with one-to-one match with present-day structures. This is an expectation we neither claim nor pursue, and one that geodynamic modeling cannot fulfill by design. Hetényi and Cattin (2026) express surprise that Paleogene-focused models are discussed in relation to present-day geophysical data, yet they repeatedly evaluate our simulations against modern lithospheric configurations inferred from present-day geophysical observations. Because present-day geophysical data sets constitute the only available geophysical constraints, their use to discuss mechanical implications and data reliability is unavoidable (e.g., Figure 9 and Section 5.2 of our paper). However, using such data as interpretative context differs fundamentally from treating them as targets that models must reproduce exactly.

Concerns are also raised regarding our initial and boundary conditions, particularly the representation of Eurasian crustal thickness. We perform experiments that account for thickened Eurasian crustal thickness (i.e., 60 km, e.g.,

Table 2) to account explicitly for pre-Cenozoic thickening of the southern Eurasian margin, as indicated by independent geological constraints reviewed in Section 2 of our paper and pointed out in the Comment. The resulting asymmetry with thinned Indian crust (i.e., 20–40 km, e.g., Table 2, Figures 4–7 of Sternai et al., 2025) naturally leads to tectonic convergence being accommodated primarily by subduction of thinned Indian crust and, to a lesser extent, by upper plate shortening. This is an emergent outcome, not a prescribed one. The same crustal asymmetry is adopted in the illustrative model presented in Figure 1 of the Comment. In those sketches, however, strain distribution and evolution are illustrated ad hoc, without any thermo-mechanical coupling or physical constraints. While the depicted scenario is not excluded, no rationale is provided for preferring it over results derived from fully coupled thermo-mechanical models that remain internally consistent with physical constitutive laws and state-of-the-art knowledge of rock mechanics.

3. On the Use and Interpretation of Geophysical Data: Reply to Comment Section 3

Presenting a "*current view*"of the Himalayan-Tibetan system and then comparing our models against it, Hetényi and Cattin (2026) pre-frame their criticism in a methodologically problematic way. A currently dominant view cannot serve as a validation criterion when the purpose of a study is to test whether prevailing interpretations are mechanically plausible or required (see Sections 1, 2, and 5.1 of our paper). Treating a preferred interpretation as a baseline and rejecting alternatives because they do not conform to it represents circular reasoning.

Hetényi and Cattin (2026) argue that our data selection is "*highly selective, partial and incomplete*," with the specific assertion that gravity data were not considered. This is inaccurate because we explicitly discuss the spatial association between the receiver-function doublet and a positive gravity anomaly beneath southern Tibet (e.g., in Section 5.1, with reference to Shi et al., 2020). More importantly, our conclusions do not hinge on any single data set. We explicitly discuss receiver functions, gravity anomalies, seismic velocities, impedances, and densities in our paper (e.g., Section 2 and 5.1), emphasizing that none of these can uniquely discriminate between eclogitized lower crust and mantle peridotite at relevant depths and temperatures, particularly in a region subject to a long and complex tectonic history. The suggestion that ambiguity could be resolved by adding or substituting a single data set is overly optimistic.

The Comment elevates specific receiver-function interpretations, particularly the attribution of the so-called "seismic doublet" to eclogitization of underthrust Indian lower crust, to established facts. While this interpretation is possible, it is not uniquely required by the data. Receiver functions are solutions to inverse problems and depend on processing choices and assumptions. The purpose of Figure 1c in Sternai et al. (2025), is to discuss the diversity of interpretations proposed in the literature (e.g., Sections 2 and 5.1 of our paper), not to advocate a single one. The allegation that our figure selection is "*partial*" is therefore irrelevant to the scope of its use in our paper. Our goal is not to refute the underthrusting interpretation, but to assess whether alternative or complementary crustal- or crustal/mantle-doubling mechanisms are mechanically plausible.

The Comment notes that the geological data we discuss are not uniquely diagnostic. We fully agree, and this is precisely why thermo-mechanical testing is required. Our aim is not to claim exclusive interpretations, but to physically explore whether alternative or complementary crustal- or crustal/mantle-doubling mechanisms are mechanically viable within the bounds of inherently non-unique geological and geophysical evidence. It arises from our study that underthrusting of Indian continental crust directly below the Asian continental crust with full Asian lithospheric mantle removal is not physically supported within the range of tested parameters.

Miocene volcanism in the Lhasa block or hypothetical slab breakoff events mentioned in the Comment postdate the Paleogene processes addressed by our models and thus do not invalidate our results. Instead, K-rich magmatism, mantle metasomatism, and ultramafic xenoliths discussed in Sections 2 and 5.2 of our paper are compatible with mantle wedge interlayered between continental crusts. Similarly, He isotope systematics are used in our study to assess plausibility of crustal doubling mechanisms, not to reproduce their exact present-day surface distribution. Invoking lithospheric tears and localized asthenospheric upwelling, as proposed in the Comment, introduces additional assumptions that are not strictly required nor recommended by Occam's principle *entia non sunt multiplicanda*. Finally, the comparison with the Andes is inappropriate as it juxtaposes a non-collisional, ocean-continent subduction system with an evolved continental collision zone.

Thermal critiques by Hetényi and Cattin (2026) assume that local and recent constraints apply uniformly across the collision zone and throughout its geological history. Our study instead tests whether a range of thermo-mechanical conditions can plausibly explain long-term orogenic strength. Similarly, their critique based on elevation data conflates reproducing absolute topography with explaining mechanical support to topography. Our model explicitly addresses elevation change, which is the relevant metric for assessing lithospheric load-bearing capacity. In summary, the same evaluative issues (i.e., mischaracterization of our work and circular reasoning) pervade the Comment; addressing every instance (e.g., partial melting, velocity model, tomographic constraints, seismicity, etc.) would lengthen this Reply without adding insight.

4. Conclusions: Reply to Comment Sections 4 and 5

We agree that confronting models with multiple observations is desirable, and this principle undoubtedly applies to our study. However, indiscriminately attempting to fit all available data sets, particularly those that are temporally heterogeneous or subject to interpretative uncertainty, can introduce false rigor through overfitting. In other words, models can be tuned to fit many observables at once, but such fits may reflect parameter adjustment rather than physical insight. Similarly, increasing model complexity (e.g., adding a third dimension) without a clear gain in process understanding can exacerbate non-uniqueness. Our deliberate use of a 2D framework reflects the need to isolate and test a specific mechanical process. Progress in geodynamics comes not from maximizing data fits or dimensionality, but from rigorously testing physical viability of proposed mechanisms. Our study does precisely this, and none of the critiques raised in the Comment demonstrate otherwise or invalidate our results.

The assertion that no data set can support mantle material interlayered within thickened crust overstates what geophysical data can resolve. While Indian underthrusting is undisputed and Argand's (1924) insight remains foundational, no existing data set can uniquely confirm or exclude interlayered mantle or lower crustal eclogites. Invoking classical underthrusting models as definitive does not address the mechanical paradox that motivates our study: how long-lived, high topography can be sustained once crustal thickness exceeds the depth at which ductile flow weakens the lithosphere. The core disagreement does not concern whether classical underthrusting occurred, but rather whether it can sustain long-lived high topography alone. The Comment asserts that it can without demonstrations; our study tests this assumption physically and quantitatively. By rejecting this test appealing to prevailing interpretations, the Comment illustrates how challenging it can be to reassess long-held but unsupported assumptions, even when doing so may help improve established models.

Conflict of Interest

The authors declare no conflicts of interest relevant to this study.

Availability Statement

All data needed to evaluate the content of this reply are present in the original paper (Sternai et al., 2025) and cited references.

Acknowledgments
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References

Argand, E. (1924). La tectonique del'Asie. In *conférence faite á Bruxelles, le 10 août 1922, devant le Congrès géologique international (XIIIe session)* (Vol. 13). Imp. Vaillant-Carmanne.

Burov, E., & Watts, A. B. (2006). The long-term strength of continental lithosphere: "Jelly sandwich" or "crème brûlée"? *Geological Society of America Today*, *16*(1), 4. https://doi.org/10.1130/1052-5173(2006)016<4:tltsoc>2.0.co;2

Hetényi, G., & Cattin, R. (2026). Comment on "Raising the roof of the world: Intra-crustal Asian mantle supports the Himalayan-Tibetan orogen" by Sternai et al. *Tectonics*, *45*(4), e2025TC009214. https://doi.org/10.1029/2025TC009214

Shi, D., Klemperer, S. L., Shi, J., Wu, Z., & Zhao, W. (2020). Localized foundering of Indian lower crust in the India–Tibet collision zone. *Proceedings of the National Academy of Sciences*, *117*(40), 24742–24747. https://doi.org/10.1073/pnas.2000015117

Sternai, P., Pilia, S., Ghelichkhan, S., Bouilhol, P., Menant, A., Davies, D. R., et al. (2025). Raising the roof of the world: Intra-crustal Asian mantle supports the Himalayan-Tibetan orogen. *Tectonics*, *44*(9), e2025TC009057. https://doi.org/10.1029/2025TC009057

Erratum

The originally published version of this article omitted a reference. The following has been added to the References: Hetényi, G., & Cattin, R. (2026). Comment on "Raising the roof of the world: Intra-crustal Asian mantle supports the Himalayan-Tibetan orogen" by Sternai et al. *Tectonics*, *45*(4), e2025TC009214. https://doi.org/10.1029/2025TC009214. In the first sentence of the Abstract, the cross-reference for Hetényi and Cattin has been corrected to Hetényi and Cattin (2026, https://doi.org/10.1029/2025tc009214), and the citation for Hetényi and Cattin has been corrected to Hetényi and Cattin (2026) throughout the article. This may be considered the authoritative version of record.