

Mantle-driven plate convergence due to slab detachment

Corresponding Author: Dr Yangming Wu

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Version 0:

Decision Letter:

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Dear Dr Wu,

Your manuscript titled "Geological configuration controls the dynamics of plate convergence during slab detachment" has now been seen by 3 reviewers, and we include their comments at the end of this message. They find your work of interest, but some important points are raised. We are interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. Please highlight all changes in the manuscript text file.

In particular, please ensure that the revised manuscript meets the following editorial thresholds:

- Provide full justification for your methods. Specifically, validate the use of the global mantle flow model you chose and demonstrate the presence of slab pull forces in this model to support your main conclusions; alternatively, make use of an updated mantle flow model as recommended by Reviewer #3.
- Expand your discussion section and correlate your results with additional data or previously published data as appropriate.

In addition, I would ask you to rewrite the abstract, highlighting the main point of your work that seems sort of hidden.

Reviewer #1 ask you to add a Method section between Introduction and Results; however, the Method section at the end of the text is the standard format for CommsEnv and you do not need to fulfil this specific request.

Please submit your point-by-point responses as a separate file, distinct from your cover letter where you can add responses to the Editors' comments that you do not want to be made available to the reviewers. Word files are preferred. We recommend that any figures, tables or graphs that are included in the response to reviewers are also included in the main article or Supplementary Information.

We are committed to providing a fair and constructive peer-review process. Please don't hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the referees' comments (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and the completed checklist:

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Please do not hesitate to contact us if you have any questions or would like to discuss these revisions further. We look

forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

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REVIEWER COMMENTS:

Reviewer#1

In the manuscript titled: "Geological configuration controls the dynamics of plate convergence during slab detachment," Wu et al. aim to understand better the plate kinematic and plate deformation consequences of the cessation of the subduction zone when the slab is detached from the trailing plate. This work is very well needed as there are very few geodynamic numerical modeling studies that focus on that topic. This study takes a major step forward by calculating a range of numerical models for various tectonic scenarios occurring at slab detachments. Therefore, I think that this work is timely and, overall (I am not a modeler), seems robust. As such, I think that it is well deserves to be published. I have several comments that should be clarified before the manuscript gets ready to be published (these points aim to clarify the presentation and discussion).

Major points:

The Introduction section is followed by the Results section. This is a big jump that needs to be resolved. My suggestion is to either delete the title "Results" and treat each subsection ("Vigorous trench-ward..." and "Plate convergence through...") as text that presents both the methodological side and the results. Or alternatively, add a Method section where both approaches (analysis of mantle flow and modeling) are shortly presented.

The analysis of mantle flow and comparison to plate velocities: 1) it is not clear why the certain global mantle flow was chosen (ref. 40) – would it be different if another model was considered? If so, how much? – please add a justification for this decision is needed, and an assessment of how different the outcome could be if one had chosen a different model 2) how was this model constructed? If the model was constructed by using the existing global plate kinematic model (as the boundary condition for the mantle flow model) and, from that, deduced the mantle flow, then it means that the coupling between mantle and plates was already considered. These details should be cleared out. 3) If the magnitude of plate surface velocities is lower than that of the asthenosphere velocities (Lines 92-93), it does not necessarily indicate that the plate is dragged by the interior. For instance, it may happen when a recent change in plate motion occurs, and the mantle has not reached equilibrium with the new state. 4) There is no indication of the possible influence of mantle plumes on plate velocities. Did the authors make sure that these regions are not influenced by this process? This needs to be clarified in the text. 5) Maybe the most important issue in that regard is that it seems from Figure 1 and text that this analysis is rather qualitative – the details of the analysis are not laid out (where exactly the authors compare mantle flow with respect to plate motion?) and there is no quantitative comparison that will make the case strong. As of now, Figure 1 does not make this case strong, I think. If the authors could quantify the difference in velocities (sum up the misfits within the different regions) and show the outcome in a plot (misfit vs. age of detachment, for instance), that would be great. As it reads now, the subsection "Vigorous trench-ward horizontal mantle flow triggered by slab detachment" is not very convincing.

Modeling: The authors define reference models and control models (Figure 2). I did not understand why these four modes are treated differently and labeled into two separate "groups". These four models describe four different geodynamical situations, all of which could and are happening on Earth. So why two of them are regarded as references and other controls? This is confusing. I suggest that the authors re-write and explain better the modeling approach. Also, it would be good to link, at the stage of presentation of the models, the models to the locations where they are appropriate.

Minor comments:

Line 14: replace "plate" with "lithosphere"

The last sentence of the abstract (Lines 25-26) comes as a surprise and it is not clear how the previous sentences justify this claim. Some re-writing is needed (or delete the last sentence).

Line 35: "For regional tectonic plates" – is there a place where tectonic plates are not "regional"? please re-write this sentence to make it clear and logical.

Line 36-38: I would replace this motivation sentence with a text that says up-front: the influence of slab detachment on plate kinematics and deformation under different tectonic settings is poorly understood and was mostly overlooked, yet it could provide important...

Line 71: "In this study, we construct the global..." but in Line 86, the authors say that they analyzed a pre-existing global mantle flow model and estimated from that the horizontal flow. So, clearly, this text is not accurate and needs to be rewritten.

Lines 240-242: This claim that relates the size of the plate with plate motion does not come out of the numerical models shown in the figures in the sense that there is no analysis of how the velocities are behaving with respect to the size of the plate (at least I did not realize that from the figures). Some explanation or addition of a plot that shows that would be helpful.

Line 258: add to the end of the sentence this text: ... toward the trench in the same direction shortly after the cessation of subduction (i.e., slab detachment).

Lines 245-266: An important point in that discussion is how long it takes the system to "relax" and to end the influence of slab

detachment. It would be good to mention this number when analyzing the models, and use it here.

Reviewer #2 (Remarks to the Author):

Dear Authors,

I would like to extend my appreciation for your well-structured and insightful manuscript. Your work demonstrates a significant advancement, contributing valuable knowledge to the field.

Congratulations on an excellent piece of research!

Best regards,

Reviewer #3 (Remarks to the Author):

The manuscript investigates the dynamics of plate convergence during slab detachment and emphasizes the influence of geological configurations, particularly in ridge-trench and continent-trench collision scenarios. Using thermo-mechanical modeling and global mantle flow data, the study compares and contrasts the behavior of the trailing and overriding plates, including plate motion and uplift, under these different geological configurations. They find fast trailing plate motion and minor uplift in the overriding plate after slab detachment for ridge-trench collision scenarios, but slow trailing plate motion and significant uplift in the overriding plate for continent-trench collision scenarios.

I find such comparison is interesting, and relevant to the long-lasting debates on the significance of slab suction. The manuscript is well written and well organized. However, I think there are also some important issues that need to be addressed or clarified so that the main findings of this manuscript is more compelling and the significance of this study can be improved. I list my concerns below.

1) This study has utilized a global mantle flow model from a study that was done nearly 20 years ago (Conrad et al., 2007). If I understand it correctly, the mantle flow is a combination of plate-driven flow and mantle-density-driven flow. From their definition of mantle-density-driven flow, there are no slabs at depths above 325 km. This means there is no connection between surface plates and deep slabs below 325 km. In other words, there is no slab pull force in this model, which is a major defect that should be considered by this study because this study is highly relevant for the relative importance of slab pull and slab suction. There are other problems in the mantle flow model that reduce the credibility. For example, it is not a coupled plate-mantle system. The plate motion is not generated itself, but rather imposed. The mantle flow is compared against anisotropy, but in a crude way using ISA. Such comparison won't give a tight constrain on mantle flow. The rheology is Newtonian in the asthenosphere. This kind of study can provide insights 20 years ago, but unlikely now.

There have been tremendous efforts made to improve global mantle convection modeling, especially from the Caltech group led by Michael Gurnis. The subduction zone structures are now properly modeled, including a mechanically coherent slabs defined using the Slab2 model, a well-defined narrow plate interface to decouple the subducting and overriding plates, more realistic rheology including plasticity, diffusion and dislocation creep. It is a coupled plate-mantle system, with the plate motion generated self-consistently. See the series of publications, including Stadler et al. (2010, Science), Hu et al. (2022, Nature Geoscience), Hu et al. (2024, PNAS), etc. These improvement are essential for a better understanding of slab pull and slab suction, and further the mantle flow. This study have neglected this branch of studies, but used a "flawed" mantle flow to support their findings, which I think is inappropriate. Ideally, this study should use the mantle flow from these recent updates, instead of the outdated mantle flow model. I'd be happy to provide the flow field. At least, a discussion should be included in the manuscript to compare these studies and reveal the limitations of the current study.

2) While I find the ridge-trench and continent-trench collision scenarios (mainly the reference models) are interesting and representative, other control experiments do not provide much insights, from my perspective. They can be generally classified as whether the subducting plate or the overriding plate is fixed, weakly coupled to the model boundary, or free from the model boundary. The results are expected. Those fixed show restricted motion and minor uplift at the convergent boundary, while those weakly coupled show faster convergent motion towards the trench and more uplift, and those free plates exhibit the fastest convergence and strongest uplift. They are hardly representative, because I don't think there is any plate that is absolutely fixed, and for the weakly coupled scenario which is assumed to induce some resistance for plate motion, the result is not quantitative enough to provide insights for the understanding on the importance of the slab suction force. Nearby plates exert various kinds of resistive force to the target plate. How do these forces compare to the resistive force within the imposed weak zone in your model? How do these forces compare to the slab suction force? I believe the answer to these questions depends on a lot of variables, including mantle viscosity, the size of plates, etc. This means that carrying out a thorough investigation is very difficult. I would prefer to test some variable that can be better quantified, such as the size of the plate. I think it would be interesting to see how large a plate can be dragged by the slab suction force, when the subducting plate is free.

3) How to verify your model? What the manuscript presents are just model results. They are case-specific, because the chosen parameters are largely uncertain and the model results are not compared to observation quantitatively. It is a bit out of my expectation that a detached slab can induce a plate motion reaching 10 cm/yr in the ridge-trench collision scenario (reference model 1). Is there a case that can support this finding? This takes me to the next point.

4) I don't think the fast plate motion would last long in the reference model 1. The fast plate motion can be just a transient phase. It might be misleading if the fast plate motion lasts for only 0.2 Ma, for example. A short duration of fast plate motion won't be recorded by paleomagnetism due to the low resolution. Therefore, it is necessary to show the evolution of plate motion with time before and after the slab detachment.

5) About model setup. The depth of the model is 1100 km. It is not mentioned whether a lower mantle is included in the model. I think the inclusion of lower mantle is important in this case, because the mantle circulation pattern would be very different if there is not a lower mantle. And this affects the strength of slab suction. Additionally, if there is not a lower mantle, there is more space for slab to sink in the low-viscosity upper mantle. The strong slab suction force should last artificially longer.

I notice that this work used a value of 238 kJ/mol for the activation energy of dry olivine. This value is much lower than laboratory experiments (see for example, Korenaga & Karato, JGR, 2008) and numerical inversion using adjoint method (Hu et al., PNAS, 2024). This can have important influence on mantle viscosity and further the slab suction force. It is necessary to state why such a low value is used.

6) The application of model results to the formation of Hawaiian-Emperor Bend is not straightforward to me. There had been long-lasting paleo-Pacific subduction since the Mesozoic beneath East Asia, which should have generated a lot of slabs in the deep mantle. Slab suction should have existed before and after the HEB. If they are suggesting that the slab suction was stronger after the detachment of the Izanagi slab, they need to consider the duration of this stronger slab suction. Can it remain strong (relative to the background slab suction) for ~5 Ma from the subduction of the ridge to the initiation of IBM? It is not clear to me why slab suction can explain the delay between Izanagi-Pacific Ridge subduction and the HEB, either. According to the model results, fast plate motion is induced immediately.

In fact, Hu et al. (2022, NG) has investigated the mechanisms that have caused the change of Pacific Plate motion. They find the northward motion of Pacific before 47 Ma was caused by the Kronotsky subduction. The slab detachment at 47 Ma resulted in the westward shift of Plate motion. This means that slab suction is much weaker than slab pull. Additionally, the real question for HEB is why the Pacific could move northward before 47 Ma, rather than why the Pacific Plate moved westward after 47 Ma, because no mechanism was revealed for the former before Hu et al. (2022, NG). This highly relevant study, in fact the only numerical model that can predict a significant change in Pacific Plate motion, is also omitted by this study. Note Faccenna et al. (2012) is not a numerical model. It is a force balance analysis based on a lot of assumptions.

7) The application of model results to the uplift of the Himalaya needs more discussion. I agree that the uplift is related to slab break-off. However, it is also generally believed that the break-off could have caused a flatter slab which increased the coupling between Tibet and India, causing the uplift of Himalaya. In terms of the driving force, there have been a lot of mechanisms. Even for slab suction, some studies suggest the slabs beneath East Asia is more important considering the volume. It is not clear whether the detached Indian slab play a significant role or not.

Line 50: The concept of Poiseuille flow can also been discussed in Peng et al. (GRL, 2021) for East Asia.

Lines 260-262: I don't understand this sentence. More explanation is needed.

Lines 272-274: Recent studies suggest that the initial collision took place at ~60 Ma. See Ding et al. (Sci.China.Earth.Sci., 2017), Hu X.M. et al. (ESR, 2016), Zhou et al. (Nature, 2024), just for your reference.

Given the issues aforementioned, a major revision is unavoidable. However, I believe solving these issues should significantly improve the manuscript.

Jiashun Hu

Ding L, Maksatbek S, Cai F L, Wang H Q, Song P P, Ji W Q, Xu Q, Zhang L Y, Muhammad Q, Upendra B. 2017. Processes of initial collision and suturing between India and Asia. *Science China Earth Sciences*.

Hu, J., Rudi, J., Gurnis, M., & Stadler, G. (2024). Constraining Earth's nonlinear mantle viscosity using plate-boundary resolving global inversions. *Proceedings of the National Academy of Sciences*, 121(28), e2318706121.

Hu, J., Gurnis, M., Rudi, J., Stadler, G., & Müller, R. D. (2022). Dynamics of the abrupt change in Pacific Plate motion around 50 million years ago. *Nature Geoscience*, 15(1), 74-78.

Hu, X., Wang, J., An, W., Garzanti, E., & Li, J. (2017). Constraining the timing of the India-Asia continental collision by the sedimentary record. *Science China Earth Sciences*, 60, 603-625.

Korenaga, J., & Karato, S. I. (2008). A new analysis of experimental data on olivine rheology. *Journal of Geophysical Research: Solid Earth*, 113(B2).

Peng, D., Liu, L., Hu, J., Li, S., & Liu, Y. (2021). Formation of East Asian Stagnant Slabs Due To a Pressure-Driven Cenozoic Mantle Wind Following Mesozoic Subduction. *Geophysical Research Letters*, 48(18), e2021GL094638.

Stadler, G., Gurnis, M., Burstedde, C., Wilcox, L. C., Alisic, L., & Ghattas, O. (2010). The dynamics of plate tectonics and mantle flow: From local to global scales. *science*, 329(5995), 1033-1038.

Zhou, H., Hu, J., Dal Zilio, L., Tang, M., Li, K., & Hu, X. (2024). India–Eurasia convergence speed-up by passive-margin sediment subduction. *Nature*, 635(8037), 114-120.

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Decision Letter:

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Dear Dr Wu,

Your revised manuscript titled "Mantle-driven plate convergence due to slab detachment" has now been seen by our reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment.

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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Best regards,

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REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

It is the second time I've reviewed the manuscript of Wu et al. The revised work has improved significantly, and it is clear that the authors have made a great effort to revise their work. After taking care of the following issues (simple to address), I believe that the quality of the work would reach the level appropriate for publication. I congratulate the authors on an interesting and important work.

1) Once a down-going slab is detached, the entire system responds, the trailing plate (may) change its convergence velocity, and induce topographic uplift. This response does not happen instantaneously, as it takes several Myr until it reaches the new equilibrium (if equilibrium is achieved). Although important for the tectonic environments where detachments have occurred over the past few Myrs, this transitional period is ignored in the paper, and the situation is treated as a binary one (prior- and post-detachment). Therefore, I recommend that the authors add a simple plot showing how the velocity and topography evolve after slab detachment. Showing that for the ridge-trench and continental-trench scenarios should work (a single figure with four lines would do). I would also add a short text describing this transitional behavior as it might be useful for future works concentrated on the impact of slab detachments.

Along the same line, it is unclear why the authors have picked these specific time snapshots in the model results shown in Figures 2-4. For instance, in Figure 2, 0.08, 0.69, 4.7, and 9.14 Myr snapshots are shown. There is no explanation why these specific time intervals are shown, why they are different (e.g., 0.69 vs 9.14 Myr) for the various scenarios, etc. The new plot showing the evolution of velocity/topography may help us understand that. Also, it would be helpful to know when detachment occurs in these plots (do the upper panels show the moment when detachments arise?). I would recommend showing time as time after detachment (so, the upper plots should show the zero age – the time of the detachment, and the other ages should correspond to the time passed since the detachment occurred).

2) The figure captions are not informative enough to easily understand the plots. A few examples: In Figure 2, there is no description of the different panels; it is left to the reader to understand how the figure is structured and that it shows two snapshots for each model. In Figure 5, it is unclear which color pallets are relevant to which of the maps. All of this is frustrating and should be avoided.

The same goes for each one of the figures. I urge the authors to rewrite all of the figure captions and revise the figures so that they will be very clear and easy to follow.

Looking forward to seeing this work published!

Roi Granot

Reviewer #2 (Remarks to the Author):

i reviewed the manuscript after revision and its seems better than the first draft that i have reviewed. so i Recommend it for publishing in its present form.

Reviewer #3 (Remarks to the Author):

The manuscript by Wu et al. is much improved and better organized. My comments are addressed well. The language needs to be polished. I spotted quite some grammar issues, and some awkward terminology as well.

Line 21: "lithosphere-mantle-ridge" interaction. I believe this term is rarely used, because one can not capture its actual meaning from the words.

Line 49: "showing a being pronounced".

Line 55: "physical conditions"? What kinds of physical conditions do they refer to?

Line 73: the sentence does not read smooth. "Introduction" seems not the proper word.

Line 132: should be "consistent with the case with the mid-ocean ridge prescribed on the subducting plate".

Line 151-152: The sentence beginning with "The result shows a nearly stationary ..." is confusing. Maybe "in the case of ..." is not needed.

Line 213: "younger of".

Line 221: I would say "a longer plate" without "size".

Line 237: I would say "plastic deformation of the mantle".

Line 249: method of ref. 41.

Line 260: Why do they use "whereas"? From the sentence, both Figs. 5e-f and Figs. 5g-h show strong mantle-driven drag in Mediterranean-Carpathian region. I don't see the difference.

Line 266: what kind of geophysical observation? Better to spell it out.

Line 305: What does "rule out" mean?

Line 307: "continental"?

Line 328: "short-duration" rapid plate motion is hard to be recorded.

Fig. 3f: there is a label "Fig. 4c" which I think is a wrong label.

From the models, there is no trench-ward motion of the subducting plate for the continent-trench collision scenario. But when discussing the slab detachment in Tibet at 20 Ma, the authors have highlighted the speed up of convergence, for example, in Fig. 6d. It's better to make them consistent.

I don't have to see the manuscript again, but I hope the authors can proofread the manuscript a few more times.

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Reviewer comments are presented in black.
Our responses to each comment are provided in blue.
The revised contents are highlighted in red.

Responses to reviewers' comments and suggestions

Reviewer #1

(1) The Introduction section is followed by the Results section. This is a big jump that needs to be resolved. My suggestion is to either delete the title "Results" and treat each subsection ("Vigorous trench-ward..." and "Plate convergence through...") as text that presents both the methodological side and the results. Or alternatively, add a Method section where both approaches (analysis of mantle flow and modeling) are shortly presented.

We appreciate your valuable suggestions. It is inappropriate to use the global mantle convection model as the result of this study. There is a big gap between the scientific question and the modeling results. So, we reorganized the logic of the whole manuscript.

- 1) We deleted the subsection of "Vigorous trench-ward horizontal mantle flow triggered by slab detachment" from the Result section and the corresponding Fig. 1. Moreover, we have renamed the section as "Modeling strategy and geodynamic scenarios" adding a brief explanation of the modeling strategy (Lines 69-90) and clarifying that more details are provided in the Methods section.
- 2) We moved the global mantle convection model to the Discussion section to show the slab detachment role in present-day tectonics (Lines 225-268).
- 3) We used a new Fig. 1 to show the elevation in various slab detachment scenarios. This figure not only corresponds to the Introduction section, which introduces the scientific question, but also to the Result section, which clarifies the basis of the model setup.

(2) The analysis of mantle flow and comparison to plate velocities: 1) it is not clear why the certain global mantle flow was chosen (ref. 40) – would it be different if another model was considered? If so, how much? – please add a justification for this decision is needed, and an assessment of how different the outcome could be if one had chosen a different model 2) how was this model constructed? If the model was constructed by using the existing global plate kinematic model (as the boundary condition for the mantle flow model) and, from that, deduced the mantle flow, then it means that the coupling between mantle and plates was already considered. These details should be cleared out. 3) If the magnitude of plate surface velocities is lower than that of the

asthenosphere velocities (Lines 92-93), it does not necessarily indicate that the plate is dragged by the interior. For instance, it may happen when a recent change in plate motion occurs, and the mantle has not reached equilibrium with the new state. 4) There is no indication of the possible influence of mantle plumes on plate velocities. Did the authors make sure that these regions are not influenced by this process? This needs to be clarified in the text. 5) Maybe the most important issue in that regard is that it seems from Figure 1 and text that this analysis is rather qualitative – the details of the analysis are not laid out (where exactly the authors compare mantle flow with respect to plate motion?) and there is no quantitative comparison that will make the case strong. As of now, Figure 1 does not make this case strong, I think. If the authors could quantify the difference in velocities (sum up the misfits within the different regions) and show the outcome in a plot (misfit vs. age of detachment, for instance), that would be great. As it reads now, the subsection “Vigorous trench-ward horizontal mantle flow triggered by slab detachment” is not very convincing.

Thanks for the helpful suggestion. We have deleted the previous subsection “Vigorous trench-ward horizontal mantle flow triggered by slab detachment” and added a new subsection of “Slab detachment role in the present-day plate tectonics” in the Discussion section (Lines 225-268). Since the choice of global mantle flow and the analysis of the effect of mantle flow on plate motion are less rigorous, we made a thorough revision of this part. Below are our responses to your main points:

1) it is not clear why the certain global mantle flow was chosen (ref. 40) – would it be different if another model was considered? If so, how much? – please add a justification for this decision is needed, and an assessment of how different the outcome could be if one had chosen a different model

In addition to the model of Conrad et al. (2010), we added a new model of Hu et al. (2024) for comparative analysis (Figs. 5c-d). Correspondingly, we added a brief description and assessment of these two models (Lines 229-246).

2) how was this model constructed? If the model was constructed by using the existing global plate kinematic model (as the boundary condition for the mantle flow model) and, from that, deduced the mantle flow, then it means that the coupling between mantle and plates was already considered. These details should be cleared out.

Yes. The model of Conrad et al. (2010) was constructed by using the existing global plate kinematic model by combined with mantle density heterogeneity to deduce the mantle flow. The best-fitting model was determined by comparing the model predictions of asthenospheric anisotropy to a global data set of shear wave splitting measurements. The new added model of Hu et al. (2024) is just the opposite, which used the existing global plate kinematic model

as the constraint to determine the best-fitting model. The related description can be seen in [Lines 229-246](#).

3) If the magnitude of plate surface velocities is lower than that of the asthenosphere velocities (Lines 92-93), it does not necessarily indicate that the plate is dragged by the interior. For instance, it may happen when a recent change in plate motion occurs, and the mantle has not reached equilibrium with the new state.

Thanks for this suggestion. We deleted this arbitrary description. In the new version, we use drag coefficients to quantify the effect of mantle drag versus plate drag according to the method of Coltice et al. (2019). We added the related description in [Lines 247-268](#) and [Figs. 5e-h](#).

Coltice N, Husson L, Faccenna C, Arnould M. What drives tectonic plates? *Science Advances* 5, (2019).

4) There is no indication of the possible influence of mantle plumes on plate velocities. Did the authors make sure that these regions are not influenced by this process? This needs to be clarified in the text.

Yes. We added some related discussion and references.

"The present mantle-driven drag at the Zagros region may come from the Afar mantle plume in the south. As previously studied, mantle plumes are also an important mechanism for driving plate motion (Becker and Faccenna, 2011; Stotz et al., 2021)." ([Lines 262-265](#))

Becker, T. W. & Faccenna, C. Mantle conveyor beneath the Tethyan collisional belt. *Earth and Planetary Science Letters* 310, 453-461 (2011).

Stotz, I. L., Vilacís, B., Hayek, J. N., Bunge, H. P. & Friedrich, A. M. Yellowstone Plume Drives Neogene North American Plate Motion Change. *Geophysical Research Letters* 48 (2021).

5) Maybe the most important issue in that regard is that it seems from Figure 1 and text that this analysis is rather qualitative – the details of the analysis are not laid out (where exactly the authors compare mantle flow with respect to plate motion?) and there is no quantitative comparison that will make the case strong. As of now, Figure 1 does not make this case strong, I think. If the authors could quantify the difference in velocities (sum up the misfits within the different regions) and show the outcome in a plot (misfit vs. age of detachment, for instance), that would be great. As it reads now, the subsection "Vigorous trench-ward horizontal mantle flow triggered by slab detachment" is not very convincing.

We agree. Following your suggestion, we added a qualitative misfit analysis (i.e., drag coefficients) between the plate velocity and the horizontal mantle flow at

200 km and 250 km, respectively. This revision is much clearer in showing the effect of mantle drag versus plate drag on plate motion. The related description in Lines 247-268 and Figs. 5e-h.

(3) Modeling: The authors define reference models and control models (Figure 2). I did not understand why these four modes are treated differently and labeled into two separate "groups". These four models describe four different geodynamical situations, all of which could and are happening on Earth. So why two of them are regarded as references and other controls? This is confusing. I suggest that the authors re-write and explain better the modeling approach. Also, it would be good to link, at the stage of presentation of the models, the models to the locations where they are appropriate.

Thanks for your valuable suggestion.

- 1) We deleted the previous global mantle convection model of Fig. 1. We added a new geological figure to show the elevation in various slab detachment scenarios (Fig. 1). This revision can correspond to the scientific question and at the same time can induce the model setup in Fig. 2.
- 2) The division of the reference model and the control model is improper. We renamed these four geodynamic scenarios in Fig. 2 as Reference Model 1, 2, 3, and 4.
- 3) We have added a new section "Modeling strategy and geodynamic scenarios" to describe the geologic basis for the model setup. (Lines 71-85)

(4) Line 14: replace "plate" with "lithosphere"
Revised. (Lines 15)

(5) The last sentence of the abstract (Lines 25-26) comes as a surprise and it is not clear how the previous sentences justify this claim. Some re-writing is needed (or delete the last sentence).

We have rewritten the Abstract section (Lines 15-26). The last sentence of the abstract is expressed as "Mid-ocean ridges bounding incoming plates accelerate plate motion in ridge-trench collision scenarios and induce considerable tectonic uplift in continent-trench collision scenarios, as evidenced by the acceleration of the Farallon-Pacific ridge spreading and the rapid uplift of the Himalayas after slab detachment episodes." (Lines 23-26)

(6) Line 35: "For regional tectonic plates" – is there a place where tectonic plates are not "regional"? please re-write this sentence to make it clear and logical. Line 36-38: I would replace this motivation sentence with a text that says up-front: the influence of slab detachment on plate kinematics and deformation under different tectonic settings is poorly understood and was mostly overlooked, yet it could provide important...

Good point. A frank expression is more understandable and better expresses the scientific question. So, we reorganized the Introduction section.

The first paragraph states the significance and motivation of our study (Lines 28-41). The second paragraph describes the geologic observations and draws out our scientific question (Lines 42-57). The third paragraph briefly summarizes our research (Lines 58-67).

The motivation sentence "However, the influence of slab detachment on plate kinematics and deformation under different tectonic settings remains poorly understood and has been mostly overlooked, albeit it could provide an important insight into plate motion." has been added at the end of the first paragraph (Lines 39-41).

(7) Line 71: "In this study, we construct the global..." but in Line 86, the authors say that they analyzed a pre-existing global mantle flow model and estimated from that the horizontal flow. So, clearly, this text is not accurate and needs to be rewritten.

Yes. We have deleted the previous subsection "Vigorous trench-ward horizontal mantle flow triggered by slab detachment" and added a new subsection of "Slab detachment role in the present-day plate tectonics" in the Discussion section (Lines 225-268).

The related expression has been clarified.

Lines 228-229: "..... we further analyzed the spatial distribution of the magnitude and direction of horizontal mantle flow and its relationship to slab detachment."

Lines 229-231: "The horizontal flow field was estimated by the southward and eastward velocity components at 200 km and 250 km depth (Figs. 5a-d) from the global mantle flow model of ref.^{40, 42}"

(8) Lines 240-242: This claim that relates the size of the plate with plate motion does not come out of the numerical models shown in the figures in the sense that there is no analysis of how the velocities are behaving with respect to the size of the plate (at least I did not realize that from the figures). Some explanation or addition of a plot that shows that would be helpful.

Thank you for the reminder. This will make our modeling results more convincing. We added a subsection about "Effects of the subducting plate size" in the Result section (Lines 168-180) and a Figure in the main text (Fig. 4). Besides, we also added a subsection about "Effects of plate boundary weakness of the subducting plate" in the Result section (Lines 139-167).

(9) Line 258: add to the end of the sentence this text: ... toward the trench in the same direction shortly after the cessation of subduction (i.e., slab detachment).

The new sentence is expressed as "..... the vigorous mantle flow triggered by slab detachment may provide additional forces that drive the Pacific plate moving toward the trench in the same direction shortly after the cessation of the Izanagi plate subduction and the corresponding acceleration of the Farallon-Pacific ridge spreading." (Lines 287-290)

(10) Lines 245-266: An important point in that discussion is how long it takes the system to "relax" and to end the influence of slab detachment. It would be good to mention this number when analyzing the models, and use it here.

Thanks for this suggestion. We have modified the logic of our expression. In previous version, we try to use our modeling results to explain these two typical ridge-trench and continent-trench collision events in geology. But it is clearly not sufficient to explain both of these major geologic events in our simplified modeling. So, in the new version, we change to use the geological observation of these two collision events to compare with our modeling results, which may provide another validate evidence (such as the global mantle convection model) for our study.

Besides, we added a statement about our model limitation at the end of the Discussion section: "Nevertheless, it is also necessary to acknowledge the limitations of our study - the single slab detachment event in our model results in a short duration of mantle-driven plate convergence. While vertical plate deformation can be recorded by topography, rapid plate motion is hard to be recorded by paleomagnetism. Future work with multiple slab detachment events that lasted for a long geological time will help to refine our understanding of the complex lithosphere-mantle interaction beneath collisional belts." (Lines 325-330)

Reviewer #3

(1) This study has utilized a global mantle flow model from a study that was done nearly 20 years ago (Conrad et al., 2007). If I understand it correctly, the mantle flow is a combination of plate-driven flow and mantle-density-driven flow. From their definition of mantle-density-driven flow, there are no slabs at depths above 325 km. This means there is no connection between surface plates and deep slabs below 325 km. In other words, there is no slab pull force in this model, which is a major defect that should be considered by this study because this study is highly relevant for the relative importance of slab pull and slab suction. There are other problems in the mantle flow model that reduce the credibility. For example, it is not a coupled plate-mantle system. The plate motion is not generated itself, but rather imposed. The mantle flow is compared against anisotropy, but in a crude way using ISA. Such comparison won't give a tight constrain on mantle flow. The rheology is Newtonian in the asthenosphere.

This kind of study can provide insights 20 years ago, but unlikely now.

There have been tremendous efforts made to improve global mantle convection modeling, especially from the Caltech group led by Michael Gurnis. The subduction zone structures are now properly modeled, including a mechanically coherent slabs defined using the Slab2 model, a well-defined narrow plate interface to decouple the subducting and overriding plates, more realistic rheology including plasticity, diffusion and dislocation creep. It is a coupled plate-mantle system, with the plate motion generated self-consistently. See the series of publications, including Stadler et al. (2010, Science), Hu et al. (2022, Nature Geoscience), Hu et al. (2024, PNAS), etc. These improvement are essential for a better understanding of slab pull and slab suction, and further the mantle flow. This study have neglected this branch of studies, but used a "flawed" mantle flow to support their findings, which I think is inappropriate. Ideally, this study should use the mantle flow from these recent updates, instead of the outdated mantle flow model. I'd be happy to provide the flow field. At least, a discussion should be included in the manuscript to compare these studies and reveal the limitations of the current study.

Thanks for your valuable comments. Based on your comments, we have made the following improvements.

- 1) We deleted the subsection of "Vigorous trench-ward horizontal mantle flow triggered by slab detachment" from the Result section and the corresponding Fig. 1.
- 2) We moved the global mantle convection model to the Discussion section as further evidence to support our modeling results (Lines 225-268).
- 3) In addition to the model of Conrad et al. (2010), we added an advanced model from Hu et al. (2024) for comparative analysis (Figs. 5c-d). Correspondingly, we added a brief description and assessment of these two models (Lines 229-246).

(2) While I find the ridge-trench and continent-trench collision scenarios (mainly the reference models) are interesting and representative, other control experiments do not provide much insights, from my perspective. They can be generally classified as whether the subducting plate or the overriding plate is fixed, weakly coupled to the model boundary, or free from the model boundary. The results are expected. Those fixed show restricted motion and minor uplift at the convergent boundary, while those weakly coupled show faster convergent motion towards the trench and more uplift, and those free plates exhibit the fastest convergence and strongest uplift. They are hardly representative, because I don't think there is any plate that is absolutely fixed, and for the weakly coupled scenario which is assumed to induce some resistance for plate motion, the result is not quantitative enough to provide

insights for the understanding on the importance of the slab suction force. Nearby plates exert various kinds of resistive force to the target plate. How do these forces compare to the resistive force within the imposed weak zone in your model? How do these forces compare to the slab suction force? I believe the answer to these questions depends on a lot of variables, including mantle viscosity, the size of plates, etc. This means that carrying out a thorough investigation is very difficult. I would prefer to test some variable that can be better quantified, such as the size of the plate. I think it would be interesting to see how large a plate can be dragged by the slab suction force, when the subducting plate is free.

Thank you for the valuable suggestion. We have reorganized the logic of the Introduction and Result sections.

- 1) We used a new Fig. 1 to show the elevation in various slab detachment scenarios. This figure not only corresponds to the Introduction section to introduce our scientific questions but also aligns with the Result section to elucidate the basis for our model setup. This figure not only corresponds to the Introduction section, which introduces the scientific question, but also to the Result section, which clarifies the basis of the model setup.
- 2) Before the subsection of "Plate convergence in response to slab detachment", we add a section as "Modeling strategy and geodynamic scenarios" to briefly explain the modeling strategy (Lines 68-90).
- 3) We conducted a series of numerical experiments and added two subsections in the Result section, including "Effects of plate boundary weakness of the subducting plate" (Lines 139-167) and "Effects of the subducting plate size" (Lines 168-180). Correspondingly, we add a discussion about "Dynamic mechanism of plate convergence induced by slab detachment" in the section of Discussion and Conclusions (Lines 182-224).
- 4) We added new Fig.3 and Fig.4 in the main text to show the effect of plate boundary weakness and trailing plate size.

(3) How to verify your model? What the manuscript presents are just model results. They are case-specific, because the chosen parameters are largely uncertain and the model results are not compared to observation quantitatively. It is a bit out of my expectation that a detached slab can induce a plate motion reaching 10 cm/yr in the ridge-trench collision scenario (reference model 1). Is there a case that can support this finding?

This takes me to the next point.

We reorganized the logic of the whole manuscript.

- 1) In the new version, we used the geological observation in Fig. 1 to constrain mode setup and compare with the modeling results.
- 2) We added two subsections in the Discussion and Conclusions section, including "Slab detachment role in the present-day plate tectonics" (Lines 225-268 and "Slab detachment role during the Cenozoic" (Lines 269-319).
- 3) We added a description about the geological significance of the trench-ward motion rate in the Result section, expressed as "In this case, we can observe a trench-ward motion of the subducting plate with a maximum transient velocity of 7.3 cm/yr. The corresponding geological response is characterized by spreading acceleration of the mid-ocean ridge on the subducting plate." (Lines 107-110). Besides, we showed the relationship between the trench-ward motion rate and the factors including plate boundary weakness (Fig. 3c) and ridge-trench distance (Fig. 4c) in the ridge-trench collision scenarios.
- 4) In the subsection of "Slab detachment role during the Cenozoic", we show that "The 56-46 Ma magmatic gap formed across ca. 1500 km of the Eurasian continental margin between Japan and Sikhote-Alin, Russian Far well constrains the timing of slab detachment at 56 Ma. At the time of slab detachment, the Farallon-Pacific full spreading rate increases from 75 ± 5 mm/yr at 57.6 Ma to 101 ± 5 mm/yr at 55.9 Ma (Fig. 6b). The key points are that the 57.6–55.9 Ma surge of the Pacific-Farallon plates spreading precedes the Izu-Bonin-Mariana (ca. 52 Ma) and Tonga-Kermadec (ca. 50 Ma) subduction initiation (Fig. 6b), and there are no indications of significant changes in the Pacific plate absolute motion at this time..... Alternatively, the vigorous mantle flow triggered by slab detachment may provide additional forces that drive the Pacific plate moving toward the trench in the same direction shortly after the cessation of the Izanagi plate subduction and the corresponding acceleration of the Farallon-Pacific ridge spreading. This spreading acceleration is lower than our ridge-trench collision modes in Figs. 2b and 4c, possibly due to the relatively long ridge-trench distance." (Lines 273-291)

(4) I don't think the fast plate motion would last long in the reference model 1. The fast plate motion can be just a transient phase. It might be misleading if the fast plate motion lasts for only 0.2 Ma, for example. A short duration of fast plate motion won't be recorded by paleomagnetism due to the low resolution. Therefore, it is necessary to show the evolution of plate motion with time before and after the slab detachment.

Thanks for this suggestion. We added a statement about our model limitation at the end of the Discussion section: "Nevertheless, it is also necessary to acknowledge the limitations of our study - the single slab detachment event in our model results in a short duration of mantle-driven plate convergence. While

vertical plate deformation can be recorded by topography, rapid plate motion is hard to be recorded by paleomagnetism. Future work with multiple slab detachment events that lasted for a long geological time will help to refine our understanding of the complex lithosphere-mantle interaction beneath collisional belts. " (Lines 325-330)

(5) About model setup. The depth of the model is 1100 km. It is not mentioned whether a lower mantle is included in the model. I think the inclusion of lower mantle is important in this case, because the mantle circulation pattern would be very different if there is not a lower mantle. And this affects the strength of slab suction. Additionally, if there is not a lower mantle, there is more space for slab to sink in the low-viscosity upper mantle. The strong slab suction force should last artificially longer.

Thanks for your reminder. In our model, the velocity boundary conditions are all free slip. The depth of lower mantle extent to 1100 km, which was generally used as the model boundary. This because previous studies showed that there is a high viscosity layer around 1000 km depth in the interior of Earth (e.g., Rudolph et al., 2015; Fei et al., 2023). This high viscosity layer always causes the stagnant of the subducting slab in the upper part of the lower mantle. So, using 1100 km depth as the velocity boundary should have a minor effect on mantle flow during slab detachment. Based on the comment, we added figures to show the initial viscosity field in our model (Figs. S1b and S1d).

Rudolph, M., Lekic, V., & Lithgow-Bertelloni, C. Viscosity jump in Earth's mid-mantle. *Science* 350, 1349–1352 (2015).

Fei H., Ballmer M., Faul U., Walte N., Cao W., Katsura T. Variation in bridgmanite grain size accounts for the mid-mantle viscosity jump. *Nature* 620, 794–799 (2023).

(6) I notice that this work used a value of 238 kJ/mol for the activation energy of dry olivine. This value is much lower than laboratory experiments (see for example, Korenaga & Karato, JGR, 2008) and numerical inversion using adjoint method (Hu et al., PNAS, 2024). This can have important influence on mantle viscosity and further the slab suction force. It is necessary to state why such a low value is used.

Thanks for your reminder. We made a mistake here. The activation energy of dry olivine used in the model is 532 KJ/mol. Using a value of 238 KJ/mol will produce an anomalously low viscosity mantle, which is inconsistent with our viscosity field.

(7) The application of model results to the formation of Hawaiian-Emperor Bend is not straightforward to me. There had been long-lasting paleo-Pacific subduction since the Mesozoic beneath East Asia, which should have generated a lot of slabs in the deep mantle. Slab suction should have existed before and

after the HEB. If they are suggesting that the slab suction was stronger after the detachment of the Izanagi slab, they need to consider the duration of this stronger slab suction. Can it remain strong (relative to the background slab suction) for ~5 Ma from the subduction of the ridge to the initiation of IBM? It is not clear to me why slab suction can explain the delay between Izanagi-Pacific Ridge subduction and the HEB, either. According to the model results, fast plate motion is induced immediately.

In fact, Hu et al. (2022, NG) has investigated the mechanisms that have caused the change of Pacific Plate motion. They find the northward motion of Pacific before 47 Ma was caused by the Kronotsky subduction. The slab detachment at 47 Ma resulted in the westward shift of Plate motion. This means that slab suction is much weaker than slab pull. Additionally, the real question for HEB is why the Pacific could move northward before 47 Ma, rather than why the Pacific Plate moved westward after 47 Ma, because no mechanism was revealed for the former before Hu et al. (2022, NG). This highly relevant study, in fact the only numerical model that can predict a significant change in Pacific Plate motion, is also omitted by this study. Note Faccenna et al. (2012) is not a numerical model. It is a force balance analysis based on a lot of assumptions.

We agree. The application of model results to the formation of Hawaiian-Emperor Bend is inapposite and the over-interpretation of the simulation results. So, we deleted the related discussion. In the new version, we use the geological evidence to compare with our modeling results but not to interpret geological phenomena (Lines 270-291).

8) The application of model results to the uplift of the Himalaya needs more discussion. I agree that the uplift is related to slab break-off. However, it is also generally believed that the break-off could have caused a flatter slab which increased the coupling between Tibet and India, causing the uplift of Himalaya. In terms of the driving force, there have been a lot of mechanisms. Even for slab suction, some studies suggest the slabs beneath East Asia is more important considering the volume. It is not clear whether the detached Indian slab play a significant role or not.

Yes. We added the related discussion (Lines 303-319).

"Slab suction induced by the slabs beneath East Asia may be important for the Himalayan uplift. However, the trench-ward (i.e., north-ward) mantle flow beneath the Indian plate may rule out the effect of mantle flow from East Asia. Previous studies indicated that the initial subduction of a large portion of the north-western India continental took place at 35 ± 5 Ma along a 1500-km-long suture zone, the slab breakoff started around 25 Ma and then propagated eastwards until the complete slab breakoff around 15 Ma. The time of slab detachment is consistent with the rapid uplift along the Himalayas suture zone, which is accompanied by the fluctuation of the Carlsberg ridge spreading (Fig.

6d). Slab detachment could have caused a flatter slab that increased the coupling between the Tibet and India plates. The buoyancy of the low-velocity zone above the flat slab facilitates maintaining the high elevation of Tibet Plateau, but it may have difficulty in explaining the mechanism of rapid Himalayas uplift. By contrast, the rapid uplift along the Himalaya suture zone may be consistent with the mantle-driven plate convergence in our continent-trench collision models when there is a mid-ocean ridge on the trailing plate (Fig. 2d). Moreover, the difference in elevation between the Zagros and Himalayan collision zones (Fig. 1b) may be related to the plate boundary property (i.e., continent rifting versus spreading ridge) of the trailing plate during slab detachment (Fig. 3f)."

(9) Line 50: The concept of Poiseuille flow can also been discussed in Peng et al. (GRL, 2021) for East Asia.

Added. (Line 38)

(10) Lines 260-262: I don't understand this sentence. More explanation is needed.

This sentence is inappropriate, so we have deleted it.

(11) Lines 272-274: Recent studies suggest that the initial collision took place at ~60 Ma. See Ding et al. (Sci.China.Earth.Sci., 2017), Hu X.M. et al. (ESR, 2016), Zhou et al. (Nature, 2024), just for your reference.

We revised the description and added the related reference. (Lines 298-300)

"The uplift time of Tibet Plateau, which began at ca. 50-60 Ma (Fig. 4e), coincides with a sharp deceleration of the Carlsberg ridge spreading (Fig. 4f), suggesting an initial continental collision between India and Asia."

Reviewer comments are presented in black.
Our responses to each comment are provided in blue.
The revised contents are highlighted in red.

**Point-by-Point response for Communications Earth & Environment
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We have carefully revised the manuscript according to the requirements listed in the Editorial Requests Table, and we have provided responses to each requirement in the right column of the table.

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Thanks for your reminding. We have prepared the materials for submission according to the items listed as flow:

- 1) Cover letter
- 2) Main text
- 3) Manuscript with changes
- 4) Figures in PDF documents (upload separately)
- 5) Supplementary Information in a PDF document.
- 6) Transparent Peer Review file (the full recording file)
- 7) Point-by-point responses (the second round)
- 8) Editorial Request Table, Editorial Policy Checklist

Reviewer #1

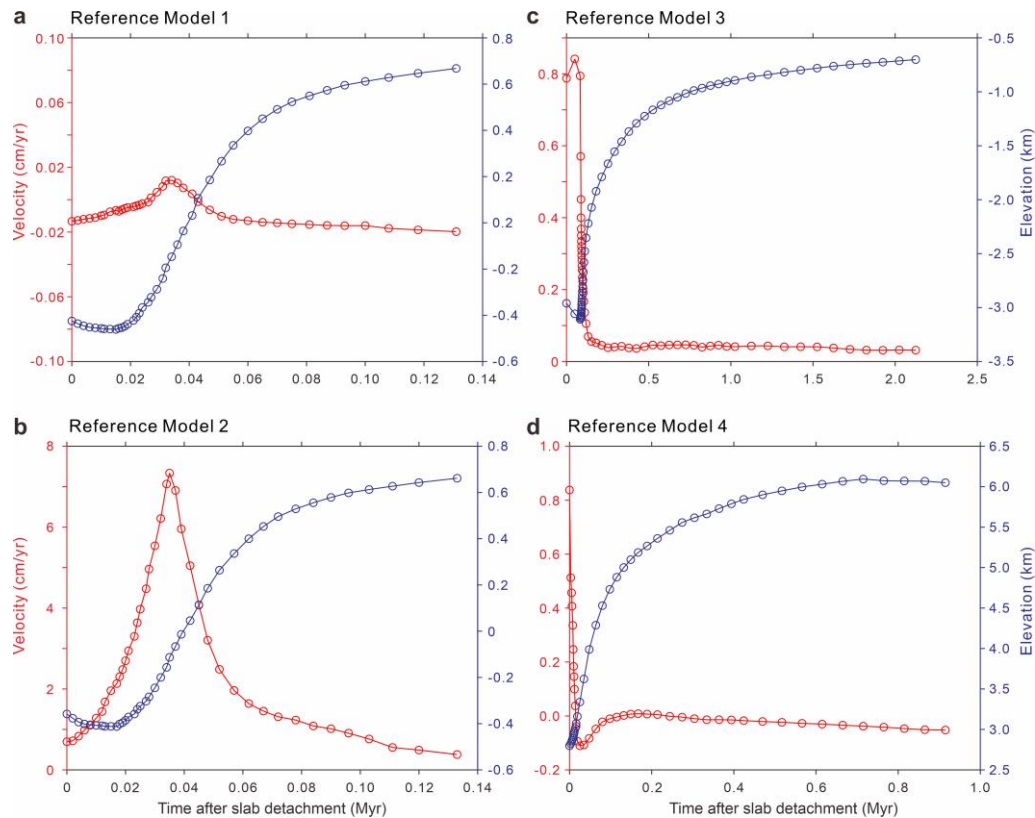
It is the second time I've reviewed the manuscript of Wu et al. The revised work has improved significantly, and it is clear that the authors have made a great effort to revise their work. After taking care of the following issues (simple to address), I believe that the quality of the work would reach the level appropriate for publication. I congratulate the authors on an interesting and important work.

Thank you very much for your positive feedback on our work. Your insightful comments have provided valuable insights into areas for improvement and refinement. We have revised the manuscript point by point based on your suggestions.

(1) Once a down-going slab is detached, the entire system responds, the trailing plate (may) change its convergence velocity, and induce topographic uplift. This response does not happen instantaneously, as it takes several Myr until it reaches the new equilibrium (if equilibrium is achieved). Although important for the tectonic environments where detachments have occurred over the past few Myrs, this transitional period is ignored in the paper, and the situation is treated as a binary one (prior- and post-detachment). Therefore, I recommend that the authors add a simple plot showing how the velocity and topography evolve after slab detachment. Showing that for the ridge-trench and continental-trench scenarios should work (a single figure with four lines would do). I would also add a short text describing this transitional behavior as it might be useful for future works concentrated on the impact of slab detachments.

We added a plot to show how the motion rate of the subducting plate and the topography of the suture zone evolve after slab detachment in the four different reference models.

Because the topography evolution of the whole model during the model time and the typical velocity profile of the whole model at some special times have been shown in Fig. 2, we presented this data graph in the supplementary Fig. S2. The related description and citation were also added in the main text (Lines 95-97, 105, 111, 121-122, 127, 129).



Supplementary Fig. 2 Evolution of the motion rate of the subducting plate and the topography of the suture zone after slab detachment in the four different reference models. **a** Ridge-trench collision without plate boundary weakness on the subducting plate. **b** Ridge-trench collision with mid-ocean ridge on the subducting plate. **c** Continental-trench collision without plate boundary weakness on the subducting plate. **d** Continent-trench collision with mid-ocean ridge on the subducting plate. The red circles indicate the motion rate of the subducting plate, calculated from the mean value in the range $y = 28$ km depth (i.e., 8 km below the initial surface) and $x = 1500$ – 2500 km. The blue circles indicate the elevation of the suture zone, calculated from the maximum value at $x = 3100$ – 3200 km in the suture zone. The horizontal axis denotes the time at which relative slab detachment arises.

Along the same line, it is unclear why the authors have picked these specific time snapshots in the model results shown in Figures 2–4. For instance, in Figure 2, 0.08, 0.69, 4.7, and 9.14 Myr snapshots are shown. There is no explanation why these specific time intervals are shown, why they are different (e.g., 0.69 vs 9.14 Myr) for the various scenarios, etc. The new plot showing the evolution of velocity/topography may help us understand that. Also, it would be helpful to know when detachment occurs in these plots (do the upper panels show the moment when detachments arise?). I would recommend showing time as time after detachment (so, the upper plots should show the zero age – the time of the detachment, and the other ages should correspond

to the time passed since the detachment occurred).

Thank you for highlighting this important point. In response, we have revised the presentation of timing in the relevant figures:

1) **Figs. 2, S3, S4, and S5:** The upper and lower model snapshots display the moment of slab detachment onset and vigorous mantle flows evoked, respectively. Consequently, the time axis for these figures is defined as time after detachment, where $t = 0$ corresponds to the detachment event.

2) **Figs. 3-4:** To maintain clarity and avoid excessive panel size, these figures do not include the snapshot at the moment of slab detachment onset. Therefore, the time is shown as absolute model time.

3) **Consistent marking of slab detachment:** For consistency across all models, the model time corresponding to the onset of slab detachment is clearly indicated by a green dashed line within the topographic evolution panel of every figure (including Figs. 3 and 4).

(2) The figure captions are not informative enough to easily understand the plots. A few examples: In Figure 2, there is no description of the different panels; it is left to the reader to understand how the figure is structured and that it shows two snapshots for each model. In Figure 5, it is unclear which color palettes are relevant to which of the maps. All of this is frustrating and should be avoided.

The same goes for each one of the figures. I urge the authors to rewrite all of the figure captions and revise the figures so that they will be very clear and easy to follow.

We appreciate your suggestions. We have revised all the figure captions to make them clear and easy to understand.

We have revised all figure captions for improved clarity. The updates are as follows:

Fig. 1: Added: "Global topography data are from the ETOPO1 arc-minute global relief model (earth_relief_05m)⁶⁵. Subduction zone geometries are based on the Slab 2 model⁶⁶. Mid-ocean ridge data are from the global tectonics package⁶⁷."

Fig. 2: Added: "The upper and lower model snapshots display the onset of slab detachment and subsequent vigorous mantle flows, respectively. The time of the lower snapshot is relative to the upper snapshot. The bottom of each panel shows the topographic evolution over model time, with the green dashed line indicating the onset time of slab detachment. Color palettes at the figure base represent viscosity, mantle flow velocity, and elevation from left to right."

Figs. 3-4: Added: "The model snapshot depicts vigorous mantle flow conditions."

Velocity profiles, topography evolution plots, and color palettes follow the conventions established in Figure 2."

Fig. 5: Added: "Color palettes at the figure base represent (from left to right): horizontal mantle flow velocity (panels a-d), slab depth (panels a-h), and mantle drag coefficient (panels e-h)."

Fig. 6: Added: "Gray-shaded areas denote the 95% confidence intervals for spreading rates."

Corresponding revisions have been implemented in the supplementary figure captions.

Reviewer #2

I reviewed the manuscript after revision and it seems better than the first draft that I have reviewed. so I Recommend it for publishing in its present form.

Thank you very much for your recognition of our work.

Reviewer #3

The manuscript by Wu et al. is much improved and better organized. My comments are addressed well. The language needs to be polished. I spotted quite some grammar issues, and some awkward terminology as well.

We appreciate the time and effort you have dedicated to evaluating our manuscript. The language has been polished per your suggestions.

Line 21: "lithosphere-mantle-ridge" interaction. I believe this term is rarely used, because one can not capture its actual meaning from the words.

We replaced the expression of "lithosphere-mantle-ridge interaction" by "lithospheric mantle-ridge interaction". (Line 21)

Line 49: "showing a being pronounced".

This expression has been revised by using "showing pronounced...". (Line 50)

Line 55: "physical conditions"? What kinds of physical conditions do they refer to?

The previous expression was wordy, so we have removed the expression "and the role of different physical conditions" to make the sentence clear. (Line 55)

Line 73: the sentence does not read smooth. "Introduction" seems not the

proper word.

The word "introduction" has been replaced by "incoming". (Line 73)

Line 132: should be "consistent with the case with the mid-ocean ridge prescribed on the subducting plate".

Corrected. (Lines 136-137)

Line 151-152: The sentence beginning with "The result shows a nearly stationary ..." is confusing. Maybe "in the case of ..." is not needed.

We have revised "in the case of ..." to "with ...". (Line 156)

Line 213: "younger of".

We have revised "the younger of oceanic lithosphere" to "the younger oceanic lithosphere". (Line 217)

Line 221: I would say "a longer plate" without "size".

We have revised "a longer plate size" to "a longer plate". (Line 225)

Line 237: I would say "plastic deformation of the mantle".

We have revised "plastic damage of mantle deformation" to "plastic deformation of the mantle". (Line 241)

Line 249: method of ref. 41.

Corrected. (Line 253)

Line 260: Why do they use "whereas"? From the sentence, both Figs. 5e-f and Figs. 5g-h show strong mantle-driven drag in Mediterranean-Carpathian region. I don't see the difference.

We revised this sentence as "In continent-trench collision zones, the model of Figs. 5e-h shows obvious mantle-driven drag at the Mediterranean-Carpathian and Zagros regions and slight mantle-driven drag along the Himalayas region." (Lines 261-264)

Line 266: what kind of geophysical observation? Better to spell it out.

We revised the expression of "geophysical observations show that ..." to "bathymetric mapping, high-resolution seismic reflection imaging, geodetic and earthquake data show that ...". (Lines 269-270)

Line 305: What does "rule out" mean?

We replaced the expression of "rule out" with "indicates ... may be minor". (Lines 309-310)

Line 307: "continental"?

The expression of "India continent" has been revised to "India continent". (Line 311)

Line 328: "short-duration" rapid plate motion is hard to be recorded.

Corrected. (Line 332)

Fig. 3f: there is a label "Fig. 4c" which I think is a wrong label.

Corrected. The label "Fig.4c" should be "Fig.4d" and the label "Fig. 4d" should be "Fig. 4e".

From the models, there is no trench-ward motion of the subducting plate for the continent-trench collision scenario. But when discussing the slab detachment in Tibet at 20 Ma, the authors have highlighted the speed up of convergence, for example, in Fig. 6d. It's better to make them consistent.

We have removed the emphasis on convergence acceleration in Fig. 6d.

Fig. 6d illustrates that the pronounced deceleration of the Carlsberg Ridge spreading indicates an initial continental collision between India and Asia at ca. 50-60 Ma. This timing substantially precedes the period of rapid uplift recorded in the India-Asia suture zone (Himalayan Mountains).

Our models highlight distinct first-order geological responses: trench-ward motion of the subducting plate dominates during ridge-trench collisions, while tectonic uplift characterizes continent-trench collisions. Crucially, both responses can occur concurrently during plate convergence, with their relative prominence governed by the specific geodynamic context.

Furthermore, slab detachment in our continent-trench collision models (Fig. 2d) generates fluctuations in ridge-axis spreading rates. Consequently, the minor spreading rate fluctuation observed in the Carlsberg Ridge record at ca. 20 Ma should be consistent with mantle-driven plate convergence along the Himalayan suture zone triggered by slab detachment.