Comments of "*Various lithospheric de formation patterns derived from rheological contrasts between continental terranes: Insights from 2-D numerical simulations*"

Comments from Referee #1:

Suggestions for revision or reasons for rejection

(visible to the public if the article is accepted and published)

The manuscript titled "*Various lithospheric de formation patterns derived from rheological contrasts between continental terranes: Insights from 2-D numerical simulations*" by Renxian et al. addresses the topic of continental collision-dynamics in the presence of multiple terranes using two-dimensional thermo-mechanical numerical experiments.

This is my second round on the manuscript and I am generally happy to accept the changes to the text and the arguments put forward by the Authors. I only have minor comments about the modified text regarding the language that I picked up on and one more pertinent issue with the comparison to natural systems. I will start with this latter:

(1) Adding extra cross-sections and improving the map helped quite a lot with this aspect of the text, but the comparison to South-eastern China still only references a solitary paper from 2009 and gives very little context. I am ever so sorry to write this, but I would strongly suggest the Authors to revisit this section and at least slightly expand on it.

Responses #1: Thanks for your suggestions. We added more comparisons and references about the Early Paleozoic Orogen in Southeastern China to enrich our discussion in section 4.3.3. Please see Lines 372-394.

Minor comments on the language:

(2) Line 76-78: "apply the simulations to better understand on going and past deformation histories of various orogenic belts in the global, especially in eastern Asia": in the global makes no sense in this sentence. I would strongly suggest rephrasing this.

Responses #2: We deleted "in the global", please see Line 77.

(3) Lines 133-135: So there is sediment in the model-domain everywhere below 5 km? With the low sedimentation-rate employed, this is probably not a big deal, but I would like a line on justifying this choice of parametrization.

Responses #3: In our models, topography is calculated by subtracting the initial position (e.g., Y=20 km in our models) from the current vertical position of the surface. If the topography is convex upward and its height is higher than 5 km, then it will suffer erosion with a rate of 0.3 mm/yr; otherwise, if the topography is depressed downward and its depth is lower than 5 km, then it will undergo sedimentation with a

rate of 0.3 mm/yr.
Owing to surface processes are not our focuses in this study, for the aim of simplification, we set a small erosion and sedimentation rates of 0.3 mm/yr, which are similar to previous studies (Gerya and Yuan, 2003b; Bian et al., 2020). As well, we simply choose a very large value of 5 km as the threshold for initiating denudation and sedimentation to further weaken the influences of surface processes on the evolutions of our model. We added some explanations about the choices of these parameters. Please see Lines 136-140.

(4) Lines 162-163: Nothing to be done here, I just wanted to note to the Authors, that Now I understand the boundary condition employed. I feel a bit silly that this was unclear for me the first time around.
Responses #4: Thank you very much.

(5) Line 234: "starts to form folding" I would remove the word form.
Responses #5: It was corrected as "starts to fold". Please see Line 240.

(6) Line 243: I would reintroduce the removed "how" into this sentence to make it grammatically correct again.
Responses #6: Thank you very much. We re-add "how" in this sentence, please see Line 249.

(7) Line 387: I think influence should be singular here.
Responses #7: Yes, sometimes those local preexisting weak zones may control the lithospheric deformation.

(8) Line 391: "chosen" instead of "chose" and "they also have" instead of "they also has" would be the grammatically correct wording.
Responses #8: They were corrected. Please see Line 411, thank you very much.

(9) Lines 392-396: I think "some studies believe" is an inappropriate phrasing. These studies made scientific arguments. Belief has not much to do with that. Furthermore, the authors did not explore different convergent velocities (as far as I know) so stating that "the impact of the convergence rate almost can be ignored" is just plain wrong. Unless the Authors have tested the model-behaviours for different velocities, they should just acknowledge that varying these parameters was beyond the scope of the study and have not been carried out. There is nothing wrong with that.
Responses #9: We changed "believe" to "suggest" so that it can express what we mean more accurately. Please see Line 412.
In addition, we also pointed out that we did not discuss the impact of convergence rate in this study in section Model Limitations, please see Line 417.

(10) Figures: I am happy you have changed the colormaps. I would suggest acknowledging the source of the colormaps in the Acknowledgements section.

Responses #10: Thank you very much. We acknowledged Crameri et al. (2018) for using their color bar of batlow in the figures of viscosity field. Please see Lines 454-455.

Comments from Referee #2:

Suggestions for revision or reasons for rejection
(visible to the public if the article is accepted and published)

Responses: We are grateful to reviewer #2's the insightful comments on improving our manuscript, and we are also grateful to reviewer #2 for the recognition of our first round of revisions.