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Interactive comment on “Numerical models of trench migration in continental collision zones” by V. Magni et al.

V. Magni et al.

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Author Comments - reply to the review of R. Pysklywec

This is an interesting contribution that considers the dynamics of a plate boundary during the transition from ocean plate subduction to continental collision. Namely, the authors focus on the kinematics of the “trench” position during the transition. The manuscript is well written and sets up the problem effectively with a fairly straightforward computational model. The presented modelling results seem to be a suitable illustration of the concepts: viz., showing lithospheric evolution with plots of trench migration. Overall, I thought that the work is a useful contribution that explores a fairly specific subset of plate boundary dynamics.

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One of my primary comments relates to the broad implications of the work. In particular, I was wondering how much these are “continental subduction” models as described in a number of places in the manuscript. In essence, the models start with retreat as the ocean plate falls back, then there is some modest “recovery” advance of the continental suture. I call it “recovery” because the models just advance the same rather small distance of continental retreat; there is no continental subduction after that. I do see that there is some consumption of continental crust in the models (Fig2. 2c, 4c), but the models (including Fig. 7) just seem to suggest that some amount of retreat and recovery happens here. I suppose I’m suggesting that it would be helpful if the authors discussed in more detail what I see as the apparent coupling between the retreat and “advance” phases/distances (which I don’t really understand here. With this model where internal forces alone are driving the system, I don’t expect much continental subduction, but some clarification would be helpful. In our models the continental crust arrives at depth of about 200 km (model C4, Fig. 4c, 21.2 Myr) and, therefore, we think that the definition of “continental subduction” is suitable for these kinds of models. The term “recovery” suggests some sort of elasticity, which isn’t in the model, and might therefore be unsuitable here, and we prefer to avoid this term. However, we better clarified the mechanisms allowing for the complex kinematics of the trench/suture (see also the reply to point 5 of the other referee comment).

1. I don’t really like the conclusion on page 440; line 25 (“Since internal lithospheric deformation is common in collisional settings. . . this suggests that the effective viscosity of Earth’s lithosphere is less than 10^{24} Pa s”). Given the assumption in the model that the lithosphere is rigid, the lack of external driving forces in this case seems like a bold, broad-ranging statement. Are these experiments really effective at constraining a lithospheric effective viscosity between 10^{23} and 10^{24} ? (e.g., non-geodynamicists using this paper as a constraint on the rheology of the lithosphere. . .?) Can this sentence just be deleted?

1 - REPLY: We took in to account the reviewer’s suggestion writing in the revised ver-

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sion of the text (p440 from line 25): “However, internal lithospheric deformation is common in collisional settings (as expressed in e.g. back-arc basins and orogeny). Modelling results are difficult to export to a natural system, given the several assumptions made in the models (such as absence of possible external forces). Keeping in mind these assumptions, however, we propose that a viscosity of 1024 Pa s is too high to model the effective viscosity of Earth’s lithosphere.”

2. I think it would be helpful for the authors to define (or modify?) their use of “trench” in this ocean-continent system. In particular, following collision, it’s not necessarily evident what the “trench” is: e.g., the crustal suture, the mantle lithosphere-crust suture, etc. It’s not clear on Figure 2, for example, what the trench arrow relates to. Even in the ocean subduction case, I’m not really sure (at 3.3 Myr, Fig. 2c seems to be at an arbitrary point in the gap). Given that this is the whole focus of the paper, some enhanced explanation would be useful.

2 - REPLY: This was indeed unclear in the submitted text. The location of the trench in our models is defined as the location where the weak zone decouples the two converging plates most, and therefore has the largest horizontal velocity gradient. This is, in turn, calculated with respect to a point within the overriding plate. We have rewritten the part that describes the technique we use to move the trench p434 lines 18-22 to clarify this point (see also the reply to point 1 and 3 of the other referee comment).

3. I was left wondering a bit what the role of the mantle wedge in the model was. What are the models like without it? Also, it doesn’t seem to close up or be modified with the “continental subduction”, even though it is the thinner, weaker zone vs. the pro-plate. Some elaboration on these points would be interesting.

3 - REPLY: The presence of the mantle wedge is necessary to decouple the plates (we tested that without it the plates stick together and subduction is not possible anymore). Previous work by e.g. Billen and Gurnis (2001) have illustrated this point as well. This viscosity area simulates in a simplified way the weakening of mantle wedge above the

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slab due to slab dehydration and mantle hydration. We took in to account the reviewer's suggestion writing in the revised version of the text "that simulates the weakening of the area above the slab due to the slab dehydration and the mantle hydration" at the end of the sentence at p.434 lines 13-16.

4. I assume not, but did any of the models show any transition to delamination? i.e., as the ocean plate retreated did it peel back/decouple the continental lithosphere as well, rather than just stopping? Can the authors comment on what kept these models in this "advance" mode? e.g., the continental geotherm was low enough to prevent decoupling between crust and mantle lithosphere. Partially why I ask is that one of the co-authors of this contribution was involved in very similar work (Gogus et al., 2011; disclosure that the reviewer was also a co-author and is trying not to sound self-serving here...) that had analogue models exploring essentially the same type of ocean subduction to continental collision (including plots of trench/hinge positions through time) that instead showed delamination. Some link to this work seems to be suitable in the overall context of the geodynamic problem.

4 - REPLY: In our models delamination does not occur since we assume the same rheology for crust and lithospheric mantle. In other words, there is no weak layer (as the lower continental crust would be) that let the crust decouple from the lithospheric mantle. It is beyond the aim of this paper to study this kind of mechanism. We added "Gogus et al., 2011" in the Introduction (p.432, line19)

430; line 24: Are there really only three possible scenarios with collision? Change to: "After collision three possible scenarios include:"?

REPLY: The reviewer is correct that this is (unintentionally) restrictive, and we changed it with: "After collision different scenarios may occur:"

431; line 23: The paragraph beginning "An important aspect. . ." is oddly written and/or seems out of place. Heavily revise; delete?

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REPLY: As suggested by the reviewer we deleted the paragraph

432; lines 25-4 (on causes for advance) Ideas are inconsistent with how the intro was set up (at least how it's written). There (page 430; lines 20-), describe the system as slab-pull based, including the "three possible scenarios". In general, the Intro could use another read-through to refine and clean up structurally.

REPLY: Thanks for this, this indeed causes some confusion: we did not intend to state that the causes of trench advance in continental subduction are external, but we think is due to talk about it since it is something that has been suggested by many authors for some continental collision areas. However, following the suggestion of the reviewer, we revised the Introduction (see also the reply to point 2 of the other referee comments).

436; line 4: "A brittle yielding behaviour is calculated close to the surface". What does this mean? How close? Why choose this?, etc.

REPLY: We took into account the reviewer's suggestion and we changed the sentence at p. 436 (line 4) with: "In addition, the viscosity for the yielding mechanisms is calculated to reduce the strength of the lithosphere:"

436; line 12: Is a minimum viscosity also used in the models? E.g., as a numerical consideration?

REPLY: Yes, it is. It is imposed at $1e18$ Pa s but this value is never reached in the models, so it is irrelevant.

437; line 3: use Greek symbol for mu. REPLY: As suggested by the reviewer we changed the symbol with μ

438; line 27: Maybe this is semantics, but "steady-state" doesn't seem quite correct here; e.g., the trench velocities are still fluctuating and given the obvious limit to the extent of the slab, this does end.

REPLY: According to the reviewer's suggestion the sentence has been re-written and

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now is: “At this stage the direction of trench migration in the two models is opposite: the trench starts to advance in models C1, whereas, in model O1, it keeps retreating.”

Figures: I wasn’t sure that it was entirely necessary to plot both the viscosity and temperature fields for the models. I tended to just look at one or the other. I don’t really feel strongly on this—just a comment if space is a concern.

REPLY: As suggested by the reviewer we eliminated the column of temperature plots in Fig.2 and 4

Interactive comment on Solid Earth Discuss., 4, 429, 2012.

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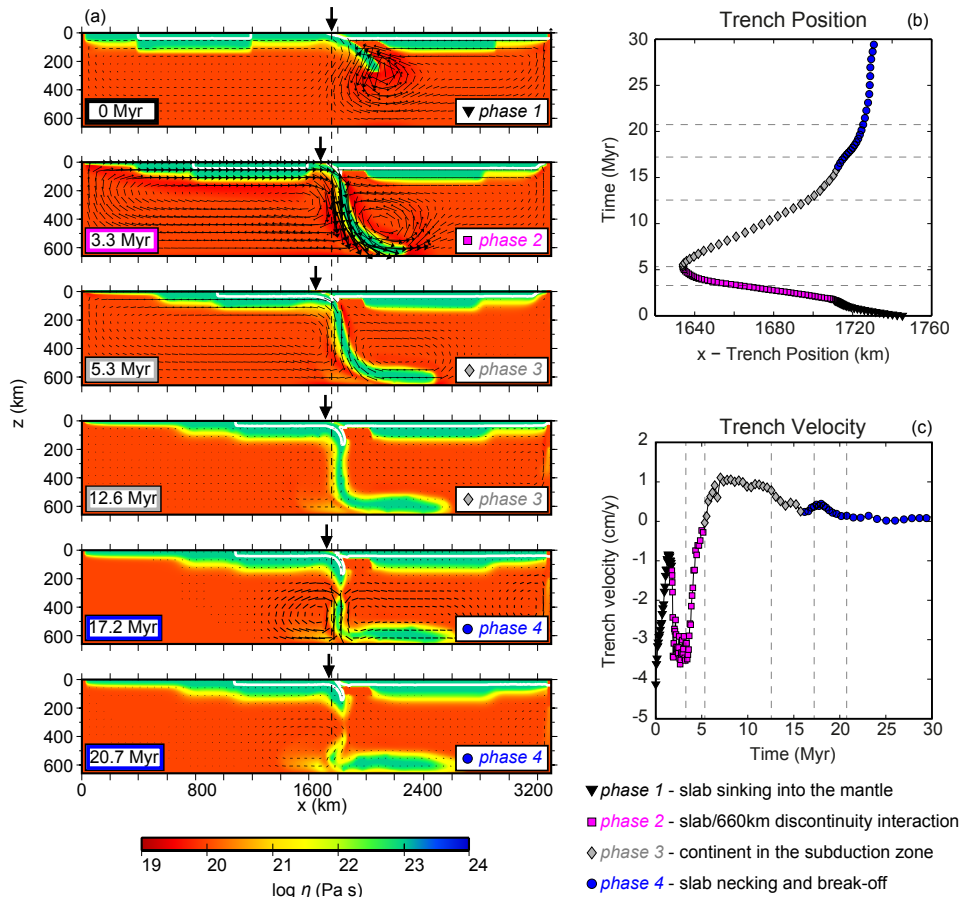


Fig. 1. 2 and 3 - After review

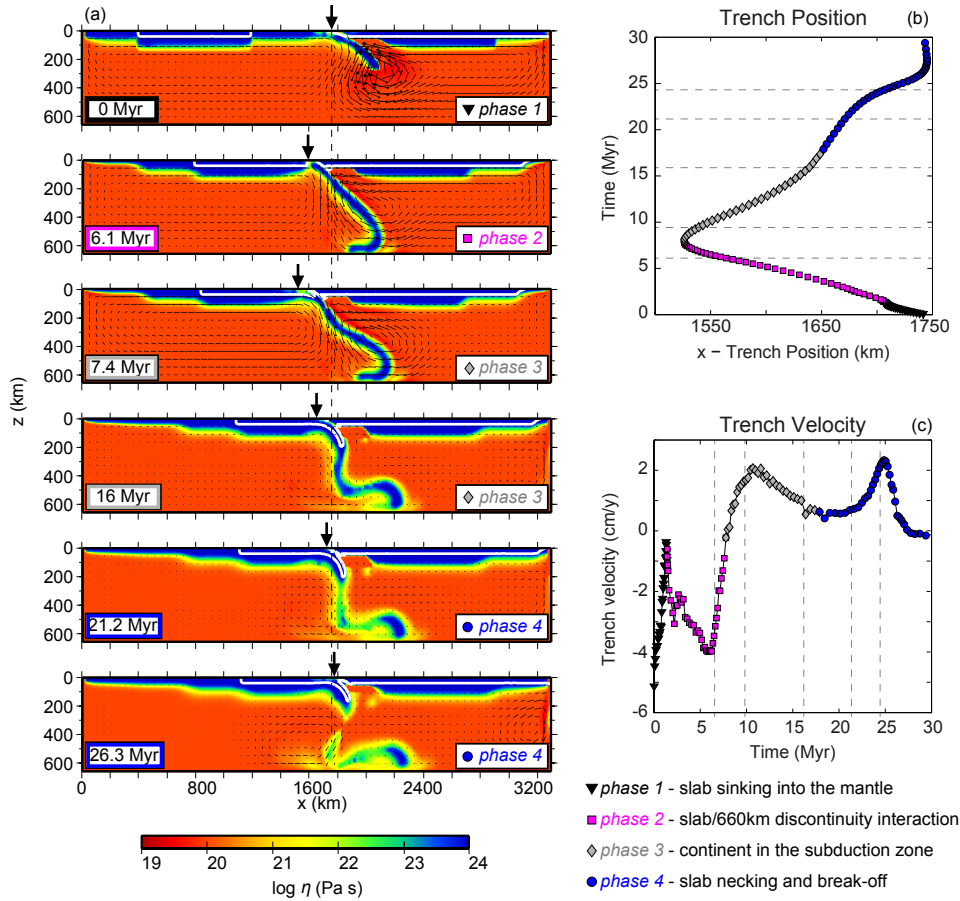


Fig. 2. 4 and 5 - After review