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Solid Earth Discussions

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Interactive Comment

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

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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.

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.

I don't really like the conclusion on page 440; line 25 ("Since internal lithospheric deformation is common in collisionalsettings . . .this suggests that the effective viscosity of Earth's lithosphere is less than 1024 Pa s"). Given the assumption in the modelâĂŤnamely, the lack of external driving forces in this caseâĂŤthis seems like a bold, broad-ranging statement. Are these experiments really effective at constraining a lithospheric effective viscosity between 10ˆ23 and 10ˆ24? (e.g., non-geodynamicists using this paper as a constraint on the rheology of the lithosphere. . .?) Can this sentence just be deleted?

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 s-point, 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.

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.

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., G^3, 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.

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

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

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.

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

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

437; line 3: use Greek symbol for mu.

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.

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 thisâĂŤjust a comment if space is a concern.

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

SED

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